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CONTENTS

ARTICLES :

PAGE

ELECTRON OPTICS 1

G. I. FINCH, D.Sc., F.R.S., and H. WILMAN, Ph.D., Applied Physical Chemistry Laboratories, Imperial College of Science and Technology, London

THE MECHANISM OF ACTION OF INSULIN 13

F. G. YOUNG, D.Sc., Ph.D., F.R.I.C., Professor of Biochemistry in the University of London, University College

SYNTHETIC PERFUMES 38

T. F. West, D.Sc., Ph.D.(Lond.), F.R.I.C., Assistant Director, Department of Chemistry, Ontario Research Foundation, Toronto, Canada

SOME APPLICATIONS OF ELECTRONICS IN METROLOGY 55

E. J. B. WILLEY, D.Sc.

FIFTY YEARS OF THE INERT GASES 66

M. SCHOFIELD, M.A., F.R.I.C., Lecturer in Chemistry at the Wolverhampton and Staffordshire Technical College

RECENT ADVANCES IN SCIENCE :

ASTRONOMY. By A. Hunter, Ph.D., F.R.A.S., Royal Observatory, Greenwich 70

PHYSICS. By F. A. VICK, O.B.E., Ph.D., F.Inst.P., The University, Manchester 78

METEOROLOGY. By M. K. Miles, M.Sc., A.R.C.S., Imperial College of Science and Technology, London 86

GENERAL AND PHYSICAL CHEMISTRY. By J. W. Smith, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic 101

ORGANIC CHEMISTRY. By A. W. Johnson, Ph.D., A.R.C.S., The University, Cambridge 112

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow 121

BOTANY. By Professor W. H. Pearsall, D.Sc., F.R.S., University College, London 127

ZOOLOGY. By William Holmes, M.A., D.Phil., The University, Oxford 131

NOTES :

New Journals 139

Quarterly Journal of Mechanics and Applied Mathematics 140

Miscellaneous 140

ESSAY REVIEW :

A Century of British Chemistry. By T. S. Wheeler, D.Sc., Ph.D., F.R.I.C., Professor of Chemistry in University College, Dublin. Being a Review of *British Chemists*, edited by A. Findlay and W. H. Mills (Chemical Society Centenary Volume) 148

REVIEWS :	PAGE
Physique appliquée des installations à rayons X. By G. Destriau	153
Introduction to Atomic Physics. By S. Tolansky	153
Applied Architectural Acoustics. By M. Rettinger	154
The Experimental Study of Structures. By A. J. S. Pippard	154
Fluid Mechanics. By R. C. Biader	155
Mechanical Vibrations. By J. P. Den Hartog	156
Modern Gas Turbines. By A. W. Judge	156
Aircraft Engines, Vol. II. By A. W. Judge	157
Modern Advances in Inorganic Chemistry. By E. B. Maxted	158
Laboratory Experiments in General Chemistry and Qualitative Analysis. By G. W. Watt	158
A Practical Chemistry for Higher School Certificate and Intermediate Students. By G. P. McHugh	159
An Introduction to Quantitative Inorganic Analysis. By R. Belcher and M. B. Thompson	159
Controlled Atmospheres for the Heat Treatment of Metals. By I. Jenkins	160
Organic Chemistry. By P. Karrer. Translated by A. J. Mee	162
Unit Processes in Organic Synthesis. Edited by P. H. Groggins	162
The Chemical Constitution of Natural Fats. By T. P. Hilditch	163
Advances in Protein Chemistry, Vol. III. Edited by M. L. Anson and J. T. Edsall	164
Concise Chemical and Technical Dictionary. Edited by H. Bennett	165
An Introduction to Textile Finishing. By J. T. Marsh	167
Matthews' Textile Fibers. Edited by H. R. Mauersberger	168
The Technology of Industrial Fire and Explosion Hazards, Vols. I and II. By R. C. Smart	168
Outlines of Palaeontology. By H. H. Swinnerton	169
The Pulse of the Earth. By J. H. F. Umbgrove	170
Limestones of Canada: Their Occurrence and Characteristics. Part V: Western Canada. By M. F. Goudge	171
The Canadian Mineral Industry in 1945. By the Staff of the Bureau of Mines, Canada	173
Introduction to Plant Ecology. By A. G. Tansley	173
Practical Field Ecology. By R. C. McLean and W. R. Ivimey Cook	173
Plant Ecology and the School. By A. G. Tansley and E. Price Evans	173
Peat Moss Deposits in Canada. By H. A. Leverin	173
Mother Earth. By G. W. Robinson	174
Essentials of Biology. By W. F. Wheeler	174
Radioactive Tracers in Biology. By M. D. Kamen	175
The Artificial Insemination of Farm Animals. By E. J. Perry and others	176
Advances in Genetics, Vol. I. Edited by M. Demerec	176
Animal Nutrition. By L. A. Maynard	177
Food, Their Values and Management. By H. C. Sherman	178
Chemistry and Methods of Enzymes. By J. B. Sumner and G. F. Somers	179
Dynamic Aspects of Biochemistry. By E. Baldwin	180
Vitamins and Hormones. Advances in Research and Applications, Vol. IV. Edited by R. S. Harris and K. V. Thimann	181
Recent Progress in Hormone Research, Vol. I. Edited by G. Pincus	182
Annual Review of Physiology, Vol. IX. Edited by V. E. Hall	183
The Design of Experiments. By R. A. Fisher	184
Statistical Methods in Research and Production, with Special Reference to the Chemical Industry. Edited by O. L. Davies	185
Van Nostrand's Scientific Encyclopedia	186
X-Ray the City! By E. Fooks	187
BOOKS RESERVED	188

CONTENTS

ARTICLES :

THE RELATION OF RHYTHMIC SEDIMENTATION TO CRUSTAL MOVEMENTS	193
A. E. TRUEMAN, D.Sc., F.R.S.	
THE SPRAYING OF LIQUIDS FROM AIRCRAFT	206
E. G. RICHARDSON, B.A., Ph.D., D.Sc., Reader in Physics in the University of Durham, King's College, Newcastle-on-Tyne	
ELECTRICAL PROPERTIES OF NERVOUS TISSUE	214
W. F. FLOYD, B.Sc., F.Inst.P., A.M.I.E.E., Lecturer in Physiology, Middlesex Hospital Medical School	
SOME BASIC FEATURES OF CLOUD CHAMBER OPERATION	226
J. G. WILSON, M.A., Ph.D., F.Inst.P., Senior Lecturer in Physics in the University of Manchester	
THE USE OF ISOTOPES IN BIOLOGY	239
G. POPJÁK, M.D., The National Institute for Medical Research, Hampstead	

RECENT ADVANCES IN SCIENCE :

ASTRONOMY. By A. Hunter, Ph.D., F.R.A.S., Royal Observatory, Greenwich	262
PHYSICS. By F. A. VICK, O.B.E., Ph.D., F.Inst.P., The University, Manchester	268
GENERAL AND PHYSICAL CHEMISTRY. By J. W. SMITH, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic	273
BIOCHEMISTRY. By W. O. KERNACK, D.Sc., LL.D., F.R.S., Royal College of Physicians' Laboratory, Edinburgh	283
GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow	292
PEDOLOGY. By G. V. JACKS, M.A., Commonwealth Bureau of Soil Science, Rothamsted Experimental Station, Harpenden	300
PLANT PHYSIOLOGY. By Professor Walter STILES, Sc.D., F.R.S., The University, Birmingham	306
ENTOMOLOGY. By A. D. LEES, M.A., Ph.D., Agricultural Research Council, Unit of Insect Physiology, Cambridge	313

NOTES :

The Mechanism of Absorption of Sound on Porous Materials (R. A. S.)	322
Miscellaneous	323

ESSAY REVIEWS :

Mathematical Physics. By H. S. W. MASSEY, Ph.D., F.R.S., Goldsmid Professor of Mathematics in the University of London, University College. Being a Review of <i>Methods of Mathematical Physics</i> , by Harold Jeffreys, M.A., D.Sc., F.R.S., and Bertha Swirles Jeffreys, M.A., Ph.D.	334
German War Research. By W. E. CURTIS, D.Sc., F.R.S., Professor of Physics in the University of Durham, King's College, Newcastle-upon-Tyne. Being a Review of <i>German Research in World War II: An Analysis of the Conduct of Research</i> , by Leslie E. Simon.	338

REVIEWS :

PAGE

Advanced Calculus. By D. V. Widder	342
Applied Bessel Functions. By F. E. Relton	342
Guide to the Literature of Mathematics and Physics including Related Works on Engineering Science. By N. G. Parke	343
A University Text Book of Physics. Vol. I: Properties of Matter. By J. H. Poynting, Sir J. J. Thomson and G. W. Todd	344
Time and Thermodynamics. By A. R. Ubbelohde	345
Methods of Measuring Temperature. By E. Griffiths	346
Fundamental Principles and Applications of Induction Heating. By "Heat-Treater"	346
Achievements in Optics. By A. Bouwers	347
Photography by Infra-red. By W. Clark	348
Light, Vision and Seeing. By M. Luckiesh	349
Elementary Applied Aerodynamics. By P. E. Hemke	350
Aircraft Strength of Materials. By H. D. Conway	350
The Escalator Method in Engineering and Vibration Problems. By J. Morris	351
A Manual of Vacuum Practice. By L. H. Martin and R. D. Hill	351
Klystron Tubes. By A. E. Harrison	352
Electronics and their Application in Industry and Research. Edited by B. Lovell	352
A Survey of the Principles and Practice of Wave Guides. By L. G. H. Huxley	353
Records and Research in Engineering and Industrial Science. By J. E. Holmstrom	354
Sub-station Practice. By T. H. Carr	355
Manual for Process Engineering Calculations. By L. Clarke	356
A Rational Approach to Chemical Principles. By J. A. Cranston	357
Chemistry for our Times. By E. C. Weaver and L. S. Foster	357
The Rare Earth Elements and their Compounds. By D. M. Yost, H. Russell and C. S. Garner	358
Lecture Experiments in Chemistry. By G. Fowles	358
The Elements of Physical Chemistry. By F. W. Goddard and E. J. F. James	359
The Chemical Kinetics of the Bacterial Cell. By C. N. Hinshelwood	360
Quantitative Organische Mikroanalyse. By F. Pregl and H. Roth	361
Fatty Acids. By K. S. Markley	362
Practical Emulsions. By H. Bennett	363
The Technology of Adhesives. By J. Delmonte	364
An Introduction to Palaeontology. By A. Morley Davies	364
The Geology of Oxford. By W. J. Arkell	365
Petrographic Micro-Technique. By A. V. Weatherhead	366
The Microscope. By J. H. Wredden	367
The Intelligent Use of the Microscope. By C. W. Olliver	367
La Vie des Requins. By P. Budker	368
Mankind So Far. By W. Howells	368
Experimental Embryology in the Netherlands, 1940-1945. By M. W. Woerdeman and C. P. Raven	369
The Louse. By P. A. Buxton	370
Chemistry of Muscular Contraction. By A. Szent-Györgyi	371
Patterns of Mammalian Reproduction. By S. A. Aspell	372
Drugs from Plants. By T. I. Williams	373
Curare. By A. B. McIntyre	373
Malaria, with Special Reference to the African Forms. By W. K. Blackie	374
The Chemical Society 1841-1941. By T. S. Moore and J. C. Philip	375
A Century of British Chemistry. By F. Sherwood Taylor	375
English Naturalists from Neokham to Ray. By C. E. Raven	375
Science in Transition. By A. W. Haslett	377
English-Spanish Chemical and Medical Dictionary. By M. Goldberg	377
Mother Earth. By T. A. Ryder	378

BOOKS RECEIVED

379

CONTENTS

ARTICLES:	PAGE
THE SILICONES	385
H. J. EMELÉUS, M.A., D.Sc., F.R.S., Professor of Inorganic Chemistry in the University of Cambridge	
THE NATURAL LIMIT OF ACCURACY OF OBSERVATION	396
R. FORTH, Dr.Phil., F.R.S.E., F.Inst.P., Reader in Theoretical Physics in the University of London, Birkbeck College	
THE THIXOTROPY OF CERTAIN SEDIMENTARY ROCKS	412
P. G. H. BOSWELL, D.Sc., F.R.S., Emeritus Professor of Geology in the Imperial College of Science and Technology	
PURE AND APPLIED SCIENCE OF THE SEA	423
F. S. RUSSELL, F.R.S., Director of the Plymouth Laboratory of the Marine Biological Association of the United Kingdom	
WAVE MECHANICS IN PHYSICS, CHEMISTRY AND BIOLOGY	436
C. A. COULSON, D.Sc., Professor of Theoretical Physics in the University of London, King's College	
THE GENETICS OF THE CILIATE PROTOZOA	450
R. F. EWER, Ph.D., Lecturer in Zoology, Natal University College, Pietermaritzburg	
RECENT ADVANCES IN SCIENCE:	
ASTRONOMY. By A. Hunter, Ph.D., F.R.A.S., Royal Observatory, Greenwich	460
PHYSICS. By F. A. VICK, O.B.E., Ph.D., F.Inst.P., The University, Manchester	468
METEOROLOGY. By P. A. SHEPPARD, B.Sc., F.Inst.P., Imperial College of Science and Technology, London	472
GENERAL AND PHYSICAL CHEMISTRY. By J. W. SMITH, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic	483
ORGANIC CHEMISTRY. By A. W. JOHNSON, Ph.D., A.R.C.S., The Univer- sity, Cambridge	496
GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow	506
POTANY. By Professor W. H. PEARSALL, D.Sc., F.R.S., University College, London	513
ZOOLOGY. By David Barker, M.A., D.Phil., The University, Oxford	518
NOTES:	
Fraunhofer's Collaborator (M. Schofield)	526
Miscellaneous	527
BRIEF REVIEW:	
Atomic Energy for the Non-Specialist. By N. Feather, Ph.D., F.R.S., Professor of Natural Philosophy in the University of Edinburgh. Being a Review of <i>The Atom</i> , by Sir George Thomson, F.R.S. (third edition), and of <i>The Atom and its Energy</i> , by E. N. da C. Andrade, D.Sc., Ph.D., F.R.S.	535

Galaxies By H. Shapley	---
The Story of Variable Stars By L. Campbell and L. Jachia	---
Between the Planets By F G Watson	539
Sound By E G Richardson	540
Electricity By M M Das	541
Theory and Applications of Electricity and Magnetism By C A Culver	541
Electromagnetism By J C Slater and N H Frank	542
Nuclear Physics in Photographs By C F Powell and G P S Occhialini	543
Electrons (+ and -), Protons, Photons, Neutrons, Mesotrons, and Cosmic Rays By R A. Millikan	544
What is Atomic Energy? By K Mendelssohn	544
Meet the Atoms By O R Frisch	545
The Electron Microscope By V E Coslett	545
Micro Waves and Wave Guides By H M Barlow	545
Radar System Engineering Edited by L N Ridenour	547
Applied Thermodynamics By A C Walshaw	547
Mathematical Theory of Rocket Flight By J B Rosser, R R Newton and G L. Gross	548
Dissociation Energies and Spectra of Diatomic Molecules By A G Gaydon	549
Chimie Organique, Vols I and II By A Kirmann	550
The Terpenes, Vol I By J L Simonsen and L N Owen	551
The Methods of Cellulose Chemistry By C Doré	551
Adventures in Man's First Plastic By N S Knaggs	552
The Chemistry of Commercial Plastics By R L Wakeman	552
The Chemistry and Technology of Plastics By R Nauth	553
Igneous Minerals and Rocks By E E Wahlstrom	555
The Permo-Triassic Formations By R L Sherlock	555
Glacial Geology and the Pleistocene Epoch. By R F Flint	557
Catalogue des Fossiles Tertiaires du Bassin de Paris By R Furon and R. Sayer	558
Ancient Plants and the World they Lived in By H. N Andrews	558
An Introduction to Paleobotany By C A Arnold	558
The Evolution of "Gossypium," and the Differentiation of the Cultivated Cottons By J B Hutchinson, R A Silow and S. G. Stephens	559
Leguminous Forage Plants By D H Robinson	561
General Biology By P D Strausbaugh and B R Weimer	561
Conseil Permanent International pour l'Exploration de la Mer, Annales Biologiques, Vol. No II, 1942-1945	562
Genetics By E Altenburg	562
Heredity and Variation in Microorganisms. Cold Spring Harbor Symposium on Quantitative Biology, Vol XI	562
The Physiology of Man in the Desert By E F. Adolph and Associates	563
The Biological Standardisation of the Vitamins. By K. H. Coward	563
Detoxication Mechanisms By R. T. Williams	564
Fundamentals of Immunology. By W. C Boyd	564
Dynamische Reaktionspathologie By K von Neergaard	564
On Understanding Science. By J. B. Conant	564
An Outline of the Development of Science. By M. Davies	565
The Psychology of Ego-Involvements. By M. Sharif and H. Cantrell	565
Science News V. Edited by J Krieger	565

CONTENTS

ARTICLES:

NUTRITION AND LIVER DISEASE	577
H. P. HIMSWORTH, M.D., F.R.C.P., Professor of Medicine in the University of London; Director of the Medical Unit, University College Hospital Medical School, London	
AUGUSTIN FRESNEL: HIS TIME, LIFE AND WORK, 1788-1827.	587
G.-A. BOUTRY, Director of the Laboratoire d'Essais, Paris	
INDETERMINISM AND THE WISH	605
JOSHUA C. GREGORY, B.Sc., F.R.I.C., Honorary Lecturer in the History of Science at Leeds University	
THE ELECTRONIC STRUCTURE OF SOLIDS	614
E. E. SCHNEIDER, Dr.Phil.Nat., Physics Department, King's College, Newcastle-upon-Tyne	
THEORETICAL ASPECTS OF CLAY CONSOLIDATION.	633
By H. H. MACKY, M.Sc., A.R.C.S., F.Inst. P., The British Ceramic Research Association, Stoke-on-Trent	

RECENT ADVANCES IN SCIENCE:

ASTRONOMY. By A. Hunter, Ph.D., F.R.A.S., Royal Observatory, Greenwich	642
PHYSICS. By F. A. VICK, O.B.E., Ph.D., F.Inst.P., The University, Manchester	650
GENERAL AND PHYSICAL CHEMISTRY. By J. W. Smith, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic	656
BIOCHEMISTRY. By W. O. KERMACK, D.Sc., LL.D., F.R.S., Royal College of Physicians' Laboratory, Edinburgh	666
GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow	676
BOTANY. By Professor W. H. PEARSELL, D.Sc., F.R.S., University College, London	685
PLANT PHYSIOLOGY. By Professor Walter STILES, Sc.D., F.R.S., The University, Birmingham	690
ENTOMOLOGY. By A. D. LEES, M.A., Ph.D., Agricultural Research Council, Unit of Insect Physiology, Cambridge	695
PHYSICAL ANTHROPOLOGY. By N. A. BARNICOT, B.Sc., Department of Anthropology, University College, London	702

NOTES:

The Synthesis of Adenosine Triphosphate	708
Mathematical Tables for Science and Industry	709
Miscellaneous	710

BRAY REVIEWS:

The Training of a Doctor. By Harold C. EDWARDS, C.B.E., M.S., F.R.C.S., Being a Review of <i>The Training of a Doctor</i> , Report of the Medical Curriculum Committee of the British Medical Association	721
A Study of Fungi. By C. T. INGOLD, D.Sc., F.L.S., Professor of Botany in the University of London, Birkbeck College. Being a Review of <i>The Fungi</i> , Vols. I and II, by F. A. Wolf and F. T. Wolf.	726

REVIEWS:

Methods of Algebraic Geometry, Vol. I. By W. V. D. Hodge and D. Pedoe	731
Theory of Functions. By J. F. Ritt	732
A Text Book of Mathematical Analysis. By R. L. Goodstein	732
Five Figure Tables of Natural Trigonometrical Functions Prepared by H.M. Nautical Almanac Office	734
Theoretical Aerodynamics. By L. M. Milne-Thomson	734
Mechanics. By J. C. Slater and N. H. Frank	735
Theory of Servomechanisms. Edited by H. M. James, N. B. Nichols and R. S. Phillips	736
Intermediate Physics. By C. J. Smith	736
Paramagnetic Relaxation. By C. J. Gorter	737
New Developments in Ferromagnetic Materials. By J. L. Snoek	738
Electrons in Gases. By Sir John Townsend	738
Exclusion Principle and Quantum Mechanics. By W. Pauli	739
Cosmic Rays. By L. Jánosy	739
Introduction to Atomic Physics. By H. Semat	740
Also. By S. A. Goudsmit	741
The Cathode Ray Oscillograph in Industry. By W. Wilson	742
The Technique of Microwave Measurements. Edited by C. G. Montgomery	742
Radar Beacons. Edited by A. Roberts	743
Electronic Circuits and Tubes. Craft Laboratory Harvard University	743
Radio Aids to Navigation. By R. A. Smith	744
Radio Engineering, Vol. I. By E. K. Sandeman	745
Introduction to Wireless. By W. E. Pearce	746
Heat Pumps. By P. Sporn, E. R. Ambrose and T. Baumeister	746
Scientific Instruments. Edited by H. J. Cooper	747
Fundamental Chemistry. By H. G. Deming	747
Cyanogen Compounds. By H. E. Williams	748
Physical Chemistry. By E. D. Eastman and G. K. Rollefson	749
The Kinetics of Reactions in Solution. By E. A. Moolwyn-Hughes	750
Tables of Properties of Gases. By E. W. Geyer and E. A. Bruege	751
Reactions of Organic Compounds. By W. J. Hickinbottom	751
A Text-Book of Practical Organic Chemistry. By A. I. Vogel	752
The Water-Soluble Gums. By C. L. Mantell	753
Synthetic Resin Chemistry for Students. By S. R. W. Martin	754
Boiler House and Power Station Chemistry. By W. Francis	755
Chemical Engineering Fundamentals. By C. G. Kirkbride	755
Encyclopedia of Chemical Technology, Vol. 1: A to Anthrimides. Edited by R. E. Kirk and D. F. Othmer	756
Safety Rules for Use in Chemical Works, Pt I: Model Rules, A.B.C.M.	757
The Formation of the Continents by Convection. By G. F. S. Hills	758
Nature and Prevention of Plant Diseases. By K. S. Choster	758
Chemical Insect Attractants and Repellents. By V. G. Dethier	759
Hormones and Horticulture. By G. S. Avery and E. B. Johnson	760
Forest Valuation. By H. H. Chapman and W. K. Meyer	760
Harmony of Nature. By L. Richmond Wheeler	761
The Bee Community. By F. H. Metcalf	762
Nucleic Acids and Nucleoproteins. Cold Spring Harbor Symposia on Quantitative Biology, Vol. XII	762
Biochimica et Biophysica Acta, Vol. I, Nos. 1 to 6, Vol. II, No. 1	763
Lectures on the Liver and its Diseases. By H. P. Himsworth	764
The Limits of Science. By L. Chwistek	765
L'Évolution de la Notion de Phénomène physique des Primitifs à Bohr et Louis de Broglie. By J. Pelsencer	766
Nicolaus Copernicus: De Revolutionibus, Preface and Book I. Translated by J. F. Dobson	767
The Royal Society Newton Tercentenary Celebrations, 1946	767
Christian Huygens and the Development of Science in the Seventeenth Century. By A. E. Bell	768
Horology. By J. E. Haswell	768
BOOKS RECEIVED	770
INDEX TO VOL. XXXVI (1948)	776

SCIENCE PROGRESS

ELECTRON OPTICS

By G. I. FINCH, D.Sc., F.R.S., AND H. WILMAN, Ph.D.

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THE physicist of the last century explained his phenomena in terms of particles or waves. To him the impact of the bullet on the target or ball on bat was to be described only in terms of energy associated with mass in motion. On the other hand, the heating of a surface by the absorption of light or the motion of a diaphragm under the impact of sound emitted from a distant source found their rational explanation in terms of the handing on of energy from one part of a medium to another without the transfer of matter, by a wave mechanism which anyone can appreciate who has seen the waves spread out from a stone dropped into the placid waters of a pond. The nineteenth-century physicist became, in fact, reconciled to the idea that these two kinds of energy transfer could not be co-ordinated into a single picture; it seemed each had to be described in a manner exclusive of the other.

In some great minds of the nineteenth century, however, the ideas of Fermat and Maupertuis, culminating in Hamilton's great Principle of Least Action, had started a ferment of dissatisfaction and stimulated a search, more unconscious than declared, for some unified mechanics of waves and particles. There followed the great ideas and experimental discoveries of the last few years of the nineteenth century and the first quarter of this century (Boltzmann, Planck, Einstein, Laue, Bohr and Compton) which, though presenting alternative wave and particle pictures of radiation phenomena, seemed at first only to emphasise the irreconcilability of the two. Thus, for nearly thirty years the conflict between the wave and particle concepts had dominated the minds of the leaders in physical thought and discovery; and all this time, each new discovery seemed only to sharpen the conflict. When Sir William Bragg whimsically complained that on Mondays, Wednesdays and Fridays light was a wave phenomenon, but on Tuesdays, Thursdays and Saturdays had to be regarded as streams of swiftly moving particles,

he was giving expression to the state of bewilderment of the physicist in the early nineteen-twenties.

By this time, however, the way had, in fact, been paved for advance, and it was Einstein and L. de Broglie who took the first great strides into the new world of unified mechanics. Einstein pointed out that the photon momentum ought to be inversely proportional to the wavelength of the light, the proportionality factor being Planck's constant. In 1924 de Broglie took the next bold step and predicted a similar relationship between waves and material particles in motion. According to this revolutionary idea not only photons but indeed any moving body should be accompanied and, in a sense, be guided in its motion by a system of waves.

The experimental physicist was slow to react to the stimulus of de Broglie's new ideas. It was not until 1927 that Davisson and Germer, on the one hand, and G. P. Thomson, on the other, quite independently and by chance carried out experiments which, though intended to study the particle behaviour of electrons, gave the entirely unlooked-for result of confirming de Broglie's wave hypothesis for moving electrons. Thus, Davisson and Germer directed a narrow stream of relatively slowly moving electrons on to the surface of a single crystal of nickel and observed the directions into which the electrons were strongly deflected. They found that these could only be explained by supposing that the electrons had been guided by waves of the wavelength predicted by de Broglie's theory. Thomson, who in his experiments used high-speed electrons, recorded on a photographic plate the directions into which the electrons were deflected on passage through a thin polycrystalline film. The similarity of Thomson's photographs with the well-known Debye-Scherrer X-ray powder pattern of rings was immediately evident, and here again the results were in quantitative agreement with de Broglie's theory. In short, in passing through crystalline matter, Thomson's beam of swiftly moving electrons was being diffracted by the regularly arranged atoms in the crystalline film, just like X-rays.

The advances in fundamental physical knowledge which we have so lightly sketched were tremendous ; and it is interesting to reflect that, throughout, the stimulus to discovery arose solely from a disinterested search for new knowledge and understanding. It would be safe to say that it never occurred to any of the great physicists whose names we have mentioned to attempt to assess the possible material value of his work ; and yet, as is so often the case, the material rewards which have accrued since Einstein and de Broglie

gave us their unified picture of quantum and wave mechanics have been truly remarkable. Without this clarification in physical thought and conception, such tremendous technical advances as "radar," television and electron optics would hardly have been possible.

With the experimental confirmation of de Broglie's theory the way was opened up for the development of a new technique for the experimental study of fine structure. Thomson and others were quick to realise that the electron beam would give information about atomic arrangement in structures such as exceedingly thin films and surface layers which cannot be analysed by X-rays, and later Busch pointed out the possibility of constructing a new instrument, the electron microscope, which would have a resolving power far in advance of that of the optical microscope.

The electron-diffraction pattern from even the simplest kinds of crystalline matter is so full of significant detail that the Faraday cylinder exploration method, used by Davisson and Germer for determining the directions of the diffracted electron beams, is cumbersome and laborious compared with the photographic method of recording used by Thomson. Nowadays in the application of electron diffraction to the study of the fine structure of matter the electrons are accelerated to a speed of roughly half that of light and the diffracted beams photographically recorded.

What can electron diffraction tell us about the atomic arrangement in crystals that X-rays cannot do equally well or even better? In answer to this question we need only draw attention to the fundamental difference between these two types of radiation. X-rays, in interacting with matter, are to be regarded as photons guided in their motion by associated waves and, similarly, electrons in motion have their attendant guiding wave systems. But photons have no electric charge, whereas electrons are negatively charged. Hence, the secondary spherical waves expanding out from the atoms influenced by X-rays are virtually wholly due to the disturbance of the circumnuclear electrons and, in the case of X-rays of the wavelength range commonly used in structure analysis ($0.5 \text{ \AA}$ to $5 \text{ \AA}$, where $1 \text{ \AA}$ or $\text{\AA}$ (Angström) $= 10^{-8} \text{ cm.}$), it is mainly the most tightly bound electrons of the small K and L orbits which are effective. With electron waves, on the other hand, the circumnuclear electrons play a minor rôle in contributing to the intensity of the scattered waves; it is the nucleus with its highly localised positive charge and intense field which, under the influence of the approach of the negative electron, acts as the main scattering agent. This fundamental difference in the scattering conditions makes it

clear immediately why, as we know from experience, X-rays are capable of penetrating considerable thicknesses of matter, but moving electrons are so easily scattered that they can be stopped by a film of matter only a few hundred atoms in thickness. Hence if we analyse the structure of a crystal by X-rays the information to be drawn from the diffraction pattern is confined to the average atomic arrangement deep within the crystal, because the amount of X-ray scattering contributed by the surface atoms is so slight as to be completely swamped by that from the interior of the crystal. The electron beam, on the other hand, never reaches the interior of the crystal; hence the electron-diffraction pattern is due entirely to a surface region of comparatively few atomic layers in thickness.

One might ask what advantage there is in determining the structure of the surface of a crystal as compared with the structure in its interior. Surely the atoms in a crystal have the same arrangement everywhere, not only deep inside the crystal but also on the surface? But the truth is, in fact, otherwise. A great host of experiences has long since taught the chemist and physicist that, not only the arrangement, but often also the nature of the atoms and molecules on the surface of most solids and liquids must be very different from that of the internal structure. To see the truth of this we only have to call to mind such phenomena as surface tension, adsorption, catalysis, photo-electric and thermionic emission and passivity. In electron diffraction we have for the first time an experimental probe which can give direct information about that peculiar structure of the surface in which must lie the fundamental explanation of these and many other surface phenomena. Let us see how to set about using electron diffraction for the study of surface structure and what kind of new knowledge has, in fact, been gained by the use of electron waves.

Electron diffraction is the electron-optical counterpart of optical interferometry. In interferometry a beam of light is directed on to and diffracted by an artificial grating, that is, by a manufactured regular arrangement of scattering centres. In electron diffraction the electron waves are roughly a hundred thousand times shorter than the waves of visible light. Hence, instead of an artificial grating, we can use crystals, *i.e.* gratings provided by Nature herself, where the average distance between the scattering centres, the atoms, is some ten thousand times shorter than in the artificial grating. In optical interferometry we usually know the distance between the scattering centres, and study the optical diffraction phenomena in order to examine some unknown property of the light such as its wavelength or spectral composition. In electron diffraction, on the

other hand, the electron wavelength is known, because this is a simple function of the potential drop through which the electrons have been accelerated in forming the beam; our unknown is the arrangement and distances between the atoms in the crystal surface.

Perhaps the most remarkable thing about the electron diffraction camera is its simplicity (Fig. 1). A stream of electrons is emitted from a cathode and accelerated by a potential drop of the order of 60,000 volts towards an anode which is pierced by a fine hole. Some of the electrons pass through this hole and emerge as a narrow stream or beam into a long, highly evacuated lower chamber. At the bottom of the chamber is a photographic plate protected by a sheet of metal painted with a fluorescent material which glows brightly wherever it is struck by the electron beam. When the fluorescent screen is removed the beam impinges directly on the photographic plate and forms a latent image which can be developed in the usual way. The material to be examined, familiarly known as "the specimen," is inserted into the camera, between the anode and the photographic plate, on the end of a rod which can be rotated and moved about without interfering with the vacuum tightness of the camera. After suitable adjustment the diffraction pattern can be seen on the fluorescent screen and then permanently recorded by raising the screen out of the way and thus exposing the plate for a few seconds.

If the specimen is in the form of a very thin film, not more than a few hundred atoms thick, the beam can pass through it without too many of the electrons being stopped or slowed down. The diffraction pattern obtained is then one which completely surrounds the intense central spot marking the point of impact of the undeflected electrons on the photographic plate. If the surface layer the structure of which is to be examined cannot be detached from its thick massive substrate we can adjust the specimen so that the beam just glances off the surface; in this case rather more than half the diffraction pattern will be masked by the shadow cast by the massive substrate material. This is, however, no real disadvantage, because the portion of the diffraction pattern not blotted out by the shadow usually contains all the information needed.

The beginner in the field of electron-diffraction soon acquires a facility for forming a good picture of the structure, and to some extent also of the texture of the specimen, from what appears to be a cursory inspection of the diffraction pattern. For example, consider Fig. 2. The material yielding this pattern is obviously largely if not wholly crystalline, otherwise there would be no rings. Further, the ring radii and intensities show that the lattice structure

is almost certainly body-centred-cubic ; later, this can be definitely settled by accurate measurement of the ring diameters which will not only give the values of the lattice spacings but also will generally enable the chemical nature of the material to be deduced. Since the rings in Fig. 2 are rather broad relative to the sharpness of the beam, we deduce that the crystals are rather small, probably about 100 Å overall. As the rings are of uniform intensity all round their circumference, and as none of the rings associated with a body-centred structure is missing or is abnormally weak or strong, we know that the crystals lie at random and point in all sorts of directions.

When the crystals are randomly disposed and relatively large (not less than two or three hundred atoms in diameter) the diffraction rings are sharp as in Fig. 3. Some of the rings in this pattern are made up of little spots instead of being uniformly dense all round. This must mean either that the crystals producing these "spotty" rings are so large that there is only room for a few hundred of them in the path of the electron beam, or that, though large enough to give sharp diffractions, they are of sub-microscopic size and are spread out thinly in space. A glance at the specimen with the optical microscope should settle which is the right answer.

Again, in Fig. 4, a reflection pattern from a thin surface film of platinum deposited on glass, the relative radii of the arcs show that the crystals have a face-centred-cubic structure, and the circumferential positions at which the arcs occur show that the crystals had all grown in orientations such that a cube face was roughly parallel to the surface of the glass substrate on which they were deposited. Although all the arcs are broad, showing that the crystals are exceedingly small, only about ten or fifteen atoms in diameter, some are unusually broad and have well-marked non-circumferential extensions. From this broadening and the directions of these extensions it can be immediately inferred that the crystals are little cubes with their corners truncated by octahedral faces.

The relation between such extensions of the diffractions and the shape of the specimen surface can be seen more clearly from Fig. 5. Here the electron beam was nearly parallel to an edge of a hæmatite crystal where two highly smooth, mirror-like faces met at an obtuse angle, the beam grazing both faces simultaneously. This single-crystal specimen gives a pattern of spots in a regular arrangement characteristic of the crystal and its orientation relative to the beam, and the spots are drawn out from a point at their upper end into lines normal to the two crystal faces, except where prevented by

the shadows from one or both surfaces. These lines increase in length with the nearness of the spots to the shadow edge, showing that the elongation is due to refraction of the electron beam at the practically plane but slightly undulating crystal surfaces; this is entirely analogous to the refraction of light at a plane (or approximately plane) interface between two media. The refraction of the electrons on entering and leaving the crystal surface is due to the crystal having an internal potential, the result of ionic or atomic distribution, which is higher than that in the vacuum outside it. Most ionic crystals, like rocksalt, galena, fluorspar, calcite, etc., are estimated to have an inner potential of the order of 10 volts.

A good idea of the surface texture of the specimen, i.e. the degree of roughness or smoothness, and the direction and perfection of the crystal facets, can thus be quickly gained by comparing the form of the reflection diffraction pattern near the shadow edge with that farther away. Fig. 6, for example, shows that in a highly polished macrocrystalline copper surface the individual crystals, though in different random orientations, had mostly very smooth but slightly undulating surfaces practically parallel to the mean surface of the specimen. On the other hand, the absence of such spot elongations, as in Fig. 7, shows that the beam is passing through spike-like projections steeply inclined to the beam, and that the pattern is therefore really a transmission pattern partially masked by the shadow of the massive substrate.

The fundamental basis of this power of interpretation at sight is the realisation that the diffraction pattern is essentially a picture of the degree and nature of orderly atomic arrangement within the specimen. In the case of a liquid, like mercury, or an amorphous solid, like glass, disorder is at a maximum; almost the only feature of regularity is the average distance of approach of the atoms to each other. Beyond a few broad ill-defined haloes the diffraction pattern is featureless (Fig. 8). But with increasing degree of order in the arrangement of the atoms the diffraction pattern features themselves increase in orderliness. Finally, when the orderly arrangement of the atoms reaches its limit, as it does in the case of a single crystal, the diffraction pattern faithfully reproduces this orderliness. For example Fig. 9 was obtained from a thin single crystal of graphite. The diffraction pattern consists of spots, each one of which is equidistant from its six neighbours, and reproduces the bird's-eye view of the symmetry of arrangement of the atoms in the flake as seen along the electron beam direction.

Curious and beautiful effects are seen in diffraction patterns obtained from massive single crystals (Fig. 10). Where the electrons

have to travel considerable distances (of the order of some hundreds of atoms) inside the crystal, some of the electrons after losing a small amount of energy are diffracted a second time and give rise to beautiful patterns of black and white line pairs or bands, which again show the nature of the high order of symmetry of arrangement of the atoms in the single crystal. As with the spot patterns, refractive displacements of the lines can be used to measure the inner potential of the crystal. These are especially marked with diamond whose potential (about 20 volts) is one of the highest recorded.

So far our discussion has been confined to the diffraction of electron waves. This is logical, because microscopy is nothing more nor less than the instrumental art of reassembling diffracted waves, be they light or electronic, into an image. Thus, like its optical counterpart, the electron microscope has three lenses (Fig. 11, Plate I). The first is a condenser which floods the specimen with a convergent beam of electrons, all moving at the same speed and therefore associated with the same wavelength. Next comes an objective lens which collects the electron beams diffracted by the specimen or object under examination and reassembles them into an enlarged image of the specimen. The last lens is the projector which further enlarges the first image and projects it on to the fluorescent screen or photographic plate.

Electron beams can be bent or deflected by either an electrostatic or an electromagnetic field. An axially symmetrical electric field causes electrons diverging from the axis direction to be deflected towards or repelled from the axis, according to the sign and distribution of the charges maintaining the field, in a way somewhat similar to the action of a convergent or a divergent lens on light beams. Similarly a divergent electron beam passing near the axis of an electro-magnetic field follows a converging spiral path, because the electron stream constitutes an electric current.

In sharp contrast to the modern optical microscope with its high aperture, the electron microscope of today uses lenses which can form good images only by using electron beams of very small divergence, i.e. nearly axial rays. Nevertheless, the beam intensity obtainable is sufficient to enable images to be seen in good detail on the fluorescent screen at magnifications up to about 20,000. The photographs can then be enlarged optically if desired, up to some 80,000-fold. One advantage of the low aperture of the electron lens lies in its depth of focus which is far greater than that of the high-power optical microscope. Thus, in the latter the detail in the object can only be seen by focussing layer by layer, each sharp

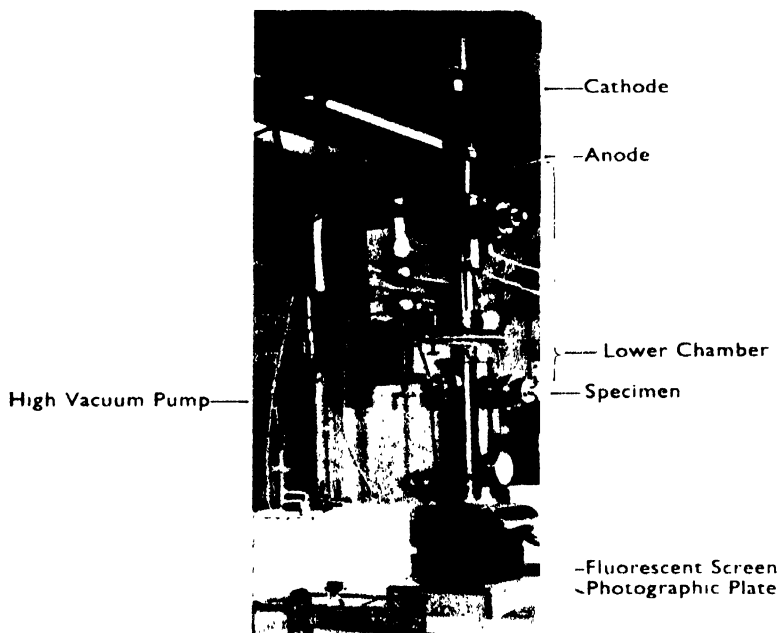


FIG. 1. Electron-diffraction camera

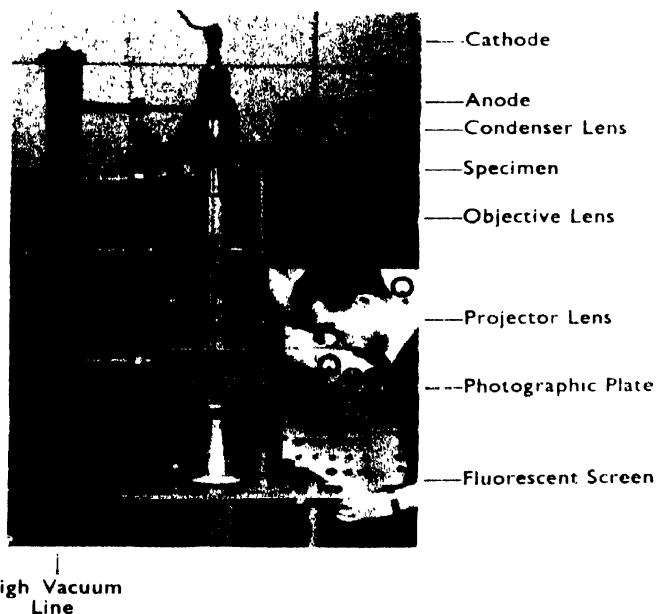


FIG. 11.—Combined electron-diffraction camera and electron microscope.

PLATE II

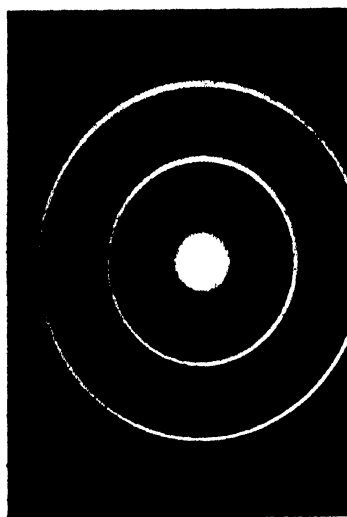


FIG. 2.—Polycrystalline chromium film.

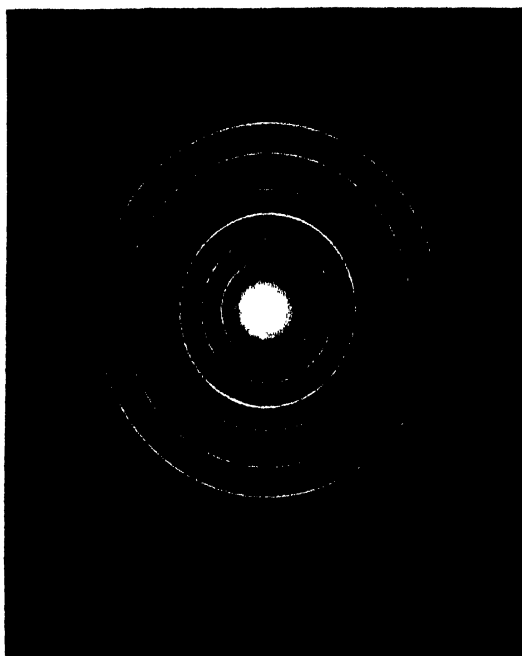


FIG. 3.—Partially oxidised tin foil.



FIG. 4.—Sputtered platinum film.

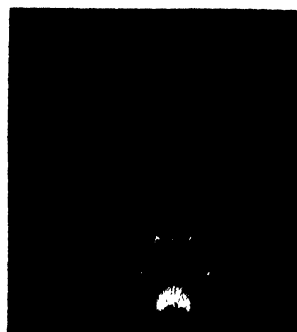


FIG. 5.—Haematite single crystal.



FIG. 6.—Macrocrystalline copper.

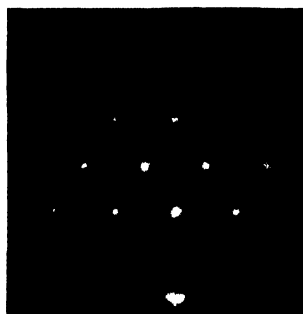


FIG. 7.—Zinc blende single crystal.

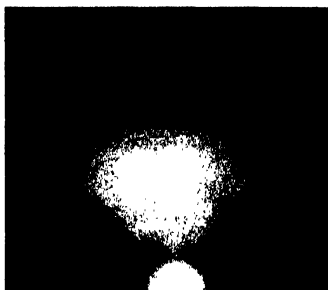


FIG. 8. - Amorphous quartz.

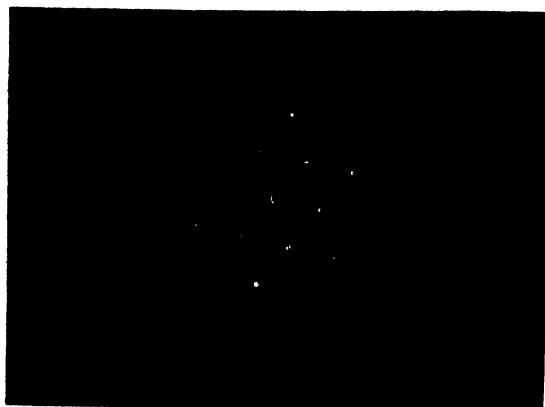


FIG. 9.—Graphite single crystal flake.



FIG. 13.—Zinc oxide pigment crystals (magn. 10,000 $\times$).

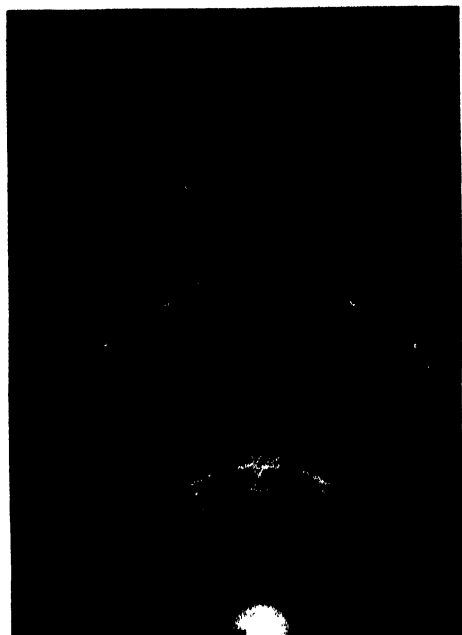


FIG. 10.--Diamond (natural octahedral face).



FIG. 12.—Oxide-film replica of an etched aluminium surface (magn. 12,000 $\times$).

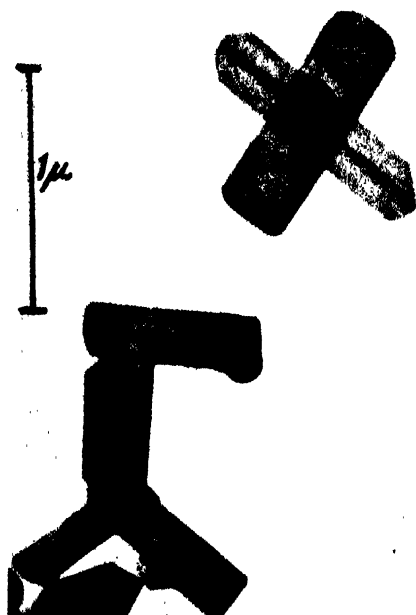


FIG. 14.—Molybdenum oxide crystals (magn. 33,000 $\times$).

image being superimposed on blurred irregular diffraction effects due to the layers which are out of focus. In electron microscopy, however, all the detail is in focus at the same time. Further, the photographic plate can be situated at a level several centimetres distant from the fluorescent screen where the image is observed during adjustments. The method of focussing and changing the magnification of the electron microscope differs from that of the optical microscope, in that the power of the electromagnetic lens can be controlled by varying the current flowing in its winding. Hence, instead of the axial movement and interchange of the lenses necessary in the optical microscope, all that is required is an appropriate change in the lens currents.

Sometimes the great depth of focus which results in all the detail being seen at once throughout the thickness of the specimen is a disadvantage; but this is being overcome by staining techniques where heavy atoms, like gold or platinum, are used. Also "shadowing," particularly with oblique parallel molecular beams of heavy atoms is proving extremely valuable in showing details of surface texture not otherwise clearly revealed.

Another important effect of the small aperture of the electron lens is that some of the beams diffracted by the specimen are not collected into the image; this contributes to contrast in the image, since the regions of greatest scattering power (highest atomic weight) in the specimen will remove a greater number of electrons from the beam. Crystalline specimens, however, have a diffracting power, and therefore also an image brightness, which is highly dependent on their orientation relative to the beam. In optical microscopy, on the other hand, most of the contrast is caused by differences of absorption in different parts of the specimen.

The main source of defective image formation in the electron microscope is spherical aberration. Chromatic aberration effects have now been largely overcome by the production of virtually mono-kinetic, and hence "monochromatic," electron beams. A further defect in the image field is associated with the spiral path of electrons passing through, but not along the axis of, an electromagnetic lens.

The range of wavelength used in the optical microscope lies between about 4000 and 8000 Å. The ultimate resolving power of the microscope is therefore such that with its aid we can just distinguish two points or lines when they are separated from each other by not less than about half a wavelength, i.e. about 2000 Å. To achieve this, however, a high degree of perfection of lens construction is essential. Both condenser and objective must have high apertures

in order that the maximum cone angle of light may be thrown on the object and collected by the objective. In the case of electron waves with their wavelength of the order of $0.05 \text{ \AA}$, however, the ultimate resolving power is so enormous that lenses of extremely small aperture, some hundred times less than the good optical lens, can be used in spite of the corresponding loss in resolution. A very great advantage of the low aperture electron lens is its simplicity of design and construction.

The fundamental scattering centres in any object viewed in the electron or optical microscope are the atoms. But as neighbouring atoms are never more than about one and a half Ångströms apart they are seen in the optical microscope only as large groupings of many millions. In the case of the electron microscope it may some day be possible to resolve individual atoms; but atoms are never still, and when we do succeed in building a microscope with a resolving power sufficient to see individual atoms or small molecules, their thermal motions, aggravated by the energy communicated to them by the electron beam, will have the effect of blurring still more the already unsharp distribution of mass and electric charge in the atomic pattern. At present, owing to the low apertures of the electron lenses so far constructed, the resolving power is in general not better than about $50 \text{ \AA}$. This means that the smallest detail visible in the electron microscope contains some few thousand atoms. Even this, however, is good enough to enable us to see individual molecules of the more complex kinds, like proteins.

We must be careful not to exaggerate the value of the high effective magnification of the present-day electron microscope. If we reflect that, even now, the interpretation of the detail revealed by the thousand-fold magnification of the optical microscope is often a matter of difficulty and doubt, despite three hundred years of experience in practical optical microscopy, how much more difficult must be the appreciation of the significance of the electron microscope image in which the fifty- or even hundred-thousand-fold magnification of the object reveals a wealth of hitherto hidden and unsuspected detail. Another handicap is that, unlike with the optical microscope, we cannot use polarised beams or take advantage of colour variations. Nevertheless, despite all this, electron microscopy has an overwhelming advantage over its optical counterpart. No matter what the object under examination may be, it is built up of assemblages of atoms within each one of which there exists an orderly arrangement of the atoms in space. The fewer the number of such orderly assemblages into which the object can be resolved, the greater will be the amount of information which the diffraction

pattern will yield about the nature of the atomic arrangement within the object.

From what has been said above it will be clear that, until it has been found possible to construct an electron microscope which can take such advantage of the potential resolving power of the electron wave that individual atoms can be recognised, the ideal electron-optical instrument is one in which are effectively combined the principles of the electron diffraction camera and the electron microscope. With this instrument it should be possible, by the mere flick of a switch, to see in the fluorescent screen either the diffraction pattern or the highly magnified image of one and the same object under examination. Instruments of this kind are already being planned and constructed.

Electron diffraction and microscopy have already contributed materially to our knowledge of the structure and texture of surfaces, thin films and small particles. Electron diffraction, in particular, has led to important scientific and practical advances in fields where surface properties are determining factors, such as mechanical sliding and lubrication, catalysis, electro-deposition, thermionic and photo-electric emission, paints, corrosion prevention and plastics.

Electron microscopy has also clarified obscurities in other fields such as metallurgy (Fig. 12), and the texture of crystals (Fig. 13, Plate III) and minerals (Fig. 14) in general. Already in a few cases a combined electron diffraction-electron microscopic attack on a problem of practical interest has led to its solution. Thanks to such an investigation we know now why a polished rocksalt cleavage surface becomes opaque and roughened on exposure to the atmosphere, and how it is that a simple annealing of the surface at a temperature well below the melting-point of rocksalt can prevent this weathering.

In biology, electron microscopy has opened up new avenues of approach to those problems the solution of which lies in the shape and nature of the sub-microscopic features of entities such as fibres, cells, bacilli, viruses, protein molecules and the like. In objects which derive more or less directly from living matter, the internal atomic arrangement is usually such that little or no useful information can be gained directly from the diffraction pattern. It is the geographical features, the outlines, convolutions, density variations and general shape—in short, the texture of the particles of biological matter—which is of primary interest.

It is to an effective combination of electron diffraction and electron microscopy that we must turn to discover the atomic structure and texture of the stuff of the submicroscopic world. The

potentialities of such a combination have as yet hardly been appreciated, so strong is the tendency to draw a false parallel between the optical microscopist who can look for no assistance from diffraction and the electron microscopist who can. Nature provides no gratings from which a visible light beam can be split up into an understandable pattern of diffracted beams ; to the electron beam, on the other hand, matter is an assemblage of natural gratings.

Our thanks are due to Drs. Cosslett, Crowe and Brown, of the Cavendish Laboratory, Cambridge, for the electron microphotographs Figs. 12 and 14 ; and to Dr. L. A. Jordan, of the Paint Research Station, Teddington, for the electron microphotograph Fig. 13.

THE MECHANISM OF ACTION OF INSULIN

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THE preparation, by Banting and Best in 1921, of insulin-containing extracts of the pancreas which were effective in the treatment of experimental and clinical diabetes heralded a time of immense activity in the field of carbohydrate metabolism. Despite the tremendous volume of relevant literature now available, it is unfortunately true to say that today there is still no general agreement about the mechanism of action of insulin. It nevertheless seems opportune at the present time to review the situation, because the recent important work of Cori and others has indicated two points of action of insulin upon the enzyme systems of the body, and in order to place this and other recent work in perspective it is necessary to review in a general way our present knowledge of carbohydrate metabolism.

THE STORAGE OF CARBOHYDRATE FOOD IN THE ANIMAL BODY

1. THE COMPOSITION OF THE BODY

The animal body obtains the energy necessary for its continued existence by releasing, in controlled oxidative processes, the free energy of the foodstuffs it consumes. The free energy of the food has ultimately been derived from solar energy, almost entirely by the activity of plant life. In the last resort plants utilise the excreta of animals to build up complex substances such as proteins and polysaccharides, obtaining from sunlight the energy required to drive these starting materials up a free-energy gradient.

This general view was enunciated more than a century ago by Dumas and Boussingault, who wrote : " Green vegetables constitute the grand laboratory of organic chemistry. They are the agents which, with carbon, hydrogen, azote, water and oxide of ammonia, slowly form the most complex substances. . . . Animals assimilate or absorb the organic substances which plants have formed. They alter them by degrees ; they destroy or decompose them. New organic substances may arise in their tissues, in their vessels ; but these are always substances of greater simplicity, more akin to the elementary state than those they had received " (Dumas, J. B., and

Boussingault, J. B., *The Chemical and Physiological Balance of Organic Nature*, 1844). It can hardly now be maintained that the new substances produced in animal tissues are always "more akin to the elementary state than those they had received," but the general idea of the balance of nature expressed by Dumas and Boussingault is still generally accepted.

The complex organisation of chemical substances which we call the animal body depends for its existence upon a temporary hesitation on the part of molecules which are inexorably moving down a free-energy gradient from food to excreta. For a time these molecules, and those formed from them in the processes of metabolism, are "mutually attracted, by forces hitherto unknown, and cohere in regular figures" (Isaac Newton, Preface to *Principia*). The pattern so created is not a static one, but, as recent investigations with tracer elements have emphasised, is highly dynamic; the existence of the body may be attributed to the development of an uneasy and temporary relationship between the rate at which atoms enter the body and the rate at which atoms of the same kind leave, free energy having been released in the rearrangements from molecule to molecule which these atoms have undergone in the processes of metabolism.

Growth of the young animal is associated with a condition in which the flow of atoms into the organised system exceeds the rate of egress of atoms of the same kind, while death and decay are accompanied by the reverse process. In the normal adult animal in good health the number of atoms of any kind (C, N, O, H, P, Na, Cl, etc.) entering the body during a given period of other than short duration is equal to the number leaving the body during the same interval of time.

TABLE 1

AVERAGE COMPOSITION OF HUMAN BEINGS

Weight of Material (kg.).				Percentage of Total.		
Material.	New Born.	Adult.	Added during Growth.	New Born.	Adult.	Added during Growth.
Total mass	2.9	70.0	67.1	—	—	—
Water	2.1	41.0	38.9	72	59	58
Dry matter	0.8	29.0	28.2	28	41	42
Carbohydrate . . .	0.01	0.40	0.39	0.35	0.57	0.58
Fat	0.35	12.60	12.25	12.1	18.0	18.2
Protein	0.34	13.10	12.76	11.7	18.7	19.0
Salts	0.10	3.00	2.90	3.5	4.3	4.3

TABLE 2

RATE OF ADDITION OF MATERIAL ADDED TO HUMAN BODY DURING 7000 DAYS
(APPROX. 19 YEARS) OF GROWTH

Material.	Weight of Material in Adult Body (kg.).	Weight of Material added during Growth (kg.).	Rate of Addition of Tissue (g./day).	Approx. Percentage of Ingested Food retained in Body during Growth.*
Total mass . . .	70.0	67.1	9.6	0.32
Water . . .	41.0	38.9	5.6	0.22
Dry matter . . .	29.0	28.2	4.0	0.85
Carbohydrate . . .	0.40	0.39	0.06	0.02
Fat . . .	12.60	12.25	1.7	2.27
Protein . . .	13.10	12.76	1.8	2.40
Salts . . .	3.00	2.90	0.4	2.60

* Based on mean daily food intake for 0-19 years of 300 g. carbohydrate, 75 g. fat, 75 g. protein, 20 g. salts, 2600 ml. water (including water formed by oxidation).

TABLE 3

STORAGE OF ENERGY IN THE ADULT HUMAN BODY

Material.	Weight of Material ingested Daily as Food (g.).	Weight of Material in Adult Human Body (g.).	Number of Days' supply stored in the Adult Body (approx.).
Carbohydrate . . .	400	400	1
Fat . . .	100	12,600	126
Protein . . .	100	13,100	131
Salts . . .	27	3000	111

If we consider the approximate gross composition of the body of the human being, we see that the amount of carbohydrate present in the body both in the new-born and in the adult is very much smaller than the amount of protein and fat (Table 1). If we consider the mean food intake over a period of 19 years of growth we see that only 0.02 per cent. of the total amount of carbohydrate ingested during this period is present in the body of the adult, compared with over 2 per cent. for fat and protein (Table 2). If, during a period of starvation, there were no discrimination between the different constituents of the adult body as a source of fuel, and if, during starvation, the body continued to oxidise these substances at the same rate as under normal dietary conditions, the amount of stored carbohydrate would provide for only one day's consumption, as compared with over 100 days for protein and fat (Table 3). Such an elementary consideration entails two obvious possibilities. First, it seems likely that the metabolic pattern in the starving animal differs substantially from that of the animal in equilibrium; second,

it is to be expected that a metabolic interconversion of foodstuffs will occur under various conditions.

Under conditions of health the amount of glucose present in the blood of the human being is remarkably stable. After a heavy carbohydrate meal it may rise to 160 mg./100 ml., while in a condition of prolonged starvation it may fall to rather less than one-half this value. In the post-absorptive state, about 10-16 hours after a meal, the blood sugar usually lies between 70 and 90 mg. per 100 ml. and such a figure may be regarded as a "normal" blood-sugar level. Under pathological conditions the above limits may be widely exceeded. In *diabetes mellitus* the blood-sugar level may exceed 1000 mg./100 ml., while in hyperinsulinism it may fall to below 40 mg./100 ml., accompanied by symptoms of hypoglycæmic derangement of the nervous system. When the blood-sugar level rises above the so-called renal threshold (170-180 mg./100 ml.) a partial failure of tubular reabsorption of glucose in the kidney occurs, with consequent appearance of glucose in the urine (glycosuria). The loss of uncombusted glucose in the urine deprives the body of free energy which it would otherwise obtain in the oxidation of this substance to CO_2 and water, and thus diminishes the mean extent of the free-energy change over which the body is working. The relatively small magnitude of the variations in the blood-sugar level encountered in health indicates the existence of a closely knit mechanism of glucose control and in this mechanism insulin plays an important role. Before we discuss this in detail we may usefully consider the nature of the problem encountered in the disposal of ingested carbohydrate by the human body. We may reasonably and conveniently assume that the normal 70-kg. man consumes approximately 400 g. of carbohydrate food each day, and for the purposes of calculation we may also assume that he ingests this amount in three equal portions. If the glucose so formed at each meal is initially distributed evenly throughout a volume of blood plasma and tissue fluids equal to the thiocyanate space—a volume which we may take to be approximately 17 l., i.e. about 25 per cent. of the body mass—then the amount of sugar in the blood plasma and tissue fluid will increase by $\frac{133,333}{17,000} \times 100 = 784$ mg./100 ml.

If the sugar does not diffuse into the red blood corpuscles the rise in the blood-sugar level may amount to about one-half of this value—say approximately 350 mg./100 ml. Since, after a carbohydrate meal, the observed rise of blood sugar rarely exceeds 80 mg./100 ml., it is clear there must exist a mechanism for the rapid disposal of sugar within the tissue cells. The conversion of glucose to

glycogen constitutes one mechanism for storage ; conversion to fatty acids, which are stored in the fat depots, also provides an important mechanism for the disposal of carbohydrate, the significance of which has been realised only recently.

2. THE FORMATION OF TISSUE GLYCOGEN FROM GLUCOSE

When Claude Bernard discovered glycogen in the liver, he regarded it primarily as the immediate source of the glucose secreted by the liver into the blood-stream, and never came to a definite conclusion as to whether or not conversely liver glycogen could be directly formed from blood glucose. He considered that the glycogen stores of the liver were primarily replenished by the conversion of non-carbohydrate material to glycogen, and that this process of glycogenesis was followed by the hydrolysis of the glycogen (glycogenolysis) in the liver, with the production of glucose which was liberated into the blood-stream by that organ.

The demonstration that glycogen is present in muscle tissue also was readily associated with the fact that, under normal conditions, the skeletal muscles of warm-blooded animals are continuously absorbing glucose from the blood flowing through them. The muscle glycogen is diminished in amount by muscular exercise, the glycogen store being subsequently replenished at the expense of the glucose absorbed from the blood-stream.

If one compares the physical and chemical properties of glucose and glycogen, it seems reasonable that glycogen should be used as a storage form of glucose by animal tissues. The masking of the majority of the reducing groups in the constituent glucose residues renders glycogen much more chemically stable than glucose, particularly in alkaline solution. In fact, glycogen is more resistant to hot alkali than any other constituent of the animal body. The masking of a large proportion of the hydroxyl groups of the glucose residues makes glycogen much less water-soluble and less hydrophilic generally than glucose. Accordingly glycogen can be precipitated from aqueous solution, of a concentration little more than about 15 per cent., in discrete granules easily accommodated within the cytoplasm of the cell. Moreover, the high molecular weight of glycogen causes its aqueous solution to exert such a small osmotic pressure that a concentrated solution can be accommodated within the cell membrane without undue disturbance. Nevertheless, Claude Bernard was wise—and wiser than most of the later investigators—in hesitating to give a final answer to the question as to whether or not glucose can be directly converted to liver glycogen. Thus, experiments in which bicarbonate containing tracer carbon

exists, therefore, a simple mechanism whereby liver glycogen can be converted to muscle glycogen by the intermediary formation of blood glucose.

3. THE INFLUENCE OF INSULIN ON GLYCOGEN STORAGE

In animals made diabetic by pancreatectomy the amount of glycogen present in the liver and muscles is subnormal. Treatment of the diabetic condition with insulin can restore the depleted glycogen. It is probable that smaller doses of insulin than are required to reduce the blood sugar to a normal level and maintain it there in diabetes will significantly raise the level of stored glycogen under such conditions. Hence, in animals with an incomplete *diabetes mellitus*, high glycogen levels may be found co-existing with diabetic hyperglycæmia. This may be the explanation of the surprisingly large amounts of glycogen which are found under some conditions in rats made diabetic by treatment with alloxan, since in such animals there may still exist some insulin-secreting activity.

The general conclusion to be drawn from experiments involving the administration of insulin to diabetic animals is that insulin promotes the storage of glycogen in both the liver and the muscles. But if insulin is given to a normal animal the liver glycogen, and possibly the muscle glycogen also, falls significantly under most conditions. This is to be attributed to secondary effects of the depression of blood sugar which follows insulin administration. In fact, it may reasonably be said that, in experiments which involve injection of insulin alone into intact animals with a normal blood-sugar level, one is largely investigating not the influence of insulin itself but the secondary and countervailing effects of the fall of the blood-sugar level which is the more direct result of the action of insulin. A diminution in the amount of glucose in the blood induces stimulation of the sympathetic nervous system, with liberation of adrenaline secreted by the adrenal medulla. These sequelæ invoke hepatic glycogenolysis, a process which tends to combat the fall of blood sugar, but of course depletes the stores of liver glycogen. If the fall of blood sugar is sufficient to involve cerebral dysfunction, muscular twitching may lead to a fall of muscle glycogen also, the product of glycogenolysis being, in this case, lactic acid.

If a drastic fall of blood-sugar level is avoided by the administration of only small doses of insulin under conditions in which the initial blood sugar is well maintained, then insulin is found to facilitate the so-called homeostatic mechanism in the liver. The idea of such a mechanism is founded in the writings of Claude Bernard, and has been developed in recent years particularly by the

investigations of Soskin, Bouckaert, De Duve, Crandall and others. The experimental methods necessary for such investigations are technically involved, but the conclusions drawn from them are relatively straightforward. According to the ideas thus developed the liver is capable of a dual function ; that of secreting glucose into the blood when the blood sugar is low, and that of absorbing glucose with the formation of stored glycogen when the blood sugar is above a critical value. This value, the liver threshold, probably lies between 90 and 110 mg./100 ml., and at this blood-sugar level the liver is neither liberating nor absorbing glucose (or is performing these two processes simultaneously at identical rates). With a blood-sugar level above the liver threshold the liver is, overall, absorbing glucose from the blood and utilising it for the laying down of glycogen. When the blood sugar falls below the liver threshold, hydrolysis of the stored liver glycogen predominates, with the liberation of the glucose so formed into the blood-stream. The glycogen which is thus lost from the liver may be restored in part by a process of glycconeogenesis, whereby liver glycogen is formed from such non-carbohydrate materials as amino-acids (from proteins), glycerol (from fat) and lactic acid. Such non-carbohydrate substances may also be directly converted to glucose in the liver (gluconeogenesis). Since the skeletal muscles form lactic acid, but not glucose, as the result of the breakdown of glycogen the conversion of lactic acid to liver glycogen provides the last step in a cycle (the " Cori " cycle) whereby liver glycogen can give rise to muscle glycogen and conversely. Thus :

liver glycogen $\rightarrow$ blood glucose $\rightarrow$ muscle glycogen

- blood lactic acid -

Glycconeogenesis or gluconeogenesis are processes which are probably taking place continuously in the liver at a rate determined by the availability of amino-acids, lactic acid, etc., and the level of sugar in the blood may be regarded as determining whether the final end-product is glucose, for liberation into the blood-stream, or glycogen, for storage in the liver tissue. In this way the blood sugar is prevented from falling excessively, even during starvation when other tissues (and in particular the brain) are continuously absorbing glucose from the blood-stream, or from rising to a level above the renal threshold even after a meal of carbohydrate. The power of the liver thus to assist in the control of the blood-sugar content is facilitated, but may not be entirely dependent upon, the presence

of insulin in the blood-stream. It is possible that in the absence of insulin the liver's homeostatic mechanism can function at the high blood-sugar level which obtains in the diabetic state, but since, under such conditions, the blood sugar exceeds the renal threshold, glycosuria is present and the mechanism in the liver is relatively ineffective in promoting the storage of administered carbohydrate.

When the blood-sugar level of the normal animal rises as a result of the ingestion of glucose it is probable that the secretion of insulin by the pancreatic islets is enhanced, but, as Soskin and others have shown, the homeostatic action of the liver can function in response to an elevation of the blood-sugar level even though the amount of insulin present in the blood-stream remains unchanged.

A direct demonstration that insulin can promote the conversion of glucose-1-phosphate to glycogen (Reaction (3) of Fig. 1) has recently been provided by Sutherland and Cori (1947), who have shown that when glucose-1-phosphate is added *in vitro* to slices of liver tissue from starving rats the formation of glycogen occurs, and that the amount so formed is greatly enhanced by the addition of insulin to the system *in vitro*. On the other hand, experiments with perfused livers have so far failed to demonstrate convincingly a stimulation of glycogen storage from glucose under the influence of added insulin under these conditions. Indeed, in many such experiments insulin is found to exert a glycogenolytic action (see below). *In vitro* verification has not yet been adduced for the action of insulin on glycogen storage in muscle tissue, but there is ample evidence from experiments involving the perfusion of isolated muscle masses to show that insulin promotes glycogen formation from glucose in skeletal muscle.

The experiments discussed above support in general the view that insulin promotes the formation of glycogen from glucose both in liver and in skeletal muscle. But we must now consider the experiments of Bridge, which provide important contrary evidence concerning the action of insulin on glycogen formation in the liver. Bridge (1938) infused glucose, with and without added insulin, into the veins of normal rabbits over a period of some hours, and found that less liver glycogen and more muscle glycogen was present in the animals which had received insulin in addition to the glucose. De Duve (1945; see also Bouckaert and de Duve, 1946) has brought forward evidence that the insulin used by Bridge in his experiments (a product of Eli Lilly Inc., U.S.A.) possesses a capacity to promote hepatic glycogenolysis which is not shared by "Novo" insulin. It is perhaps desirable at this point to recall that many years ago Bürger (see Bürger and Brandt, 1935; Bürger, 1937) and others

observed that some commercial preparations of insulin contained a substance active, in a dose of 20 μ g., in raising the blood-sugar level of a normal rabbit. They named the active substance "glukagon" and showed that it was a protein very similar to insulin in chemical and physical properties, though, unlike insulin, it was stable in alkaline solution. Since crystallinity of a protein is not an adequate criterion of its purity, it is possible that even crystalline preparations of insulin, with the possible exception of Novo insulin, are contaminated to a varying degree with such a substance. Its presence might account for the glycogenolytic action of insulin observed in perfused livers (*cf.* Lundsgaard *et al.*, 1939) and in liver slices *in vitro* (Shipley and Humel, 1945; Sutherland and Cori, 1947), when such slices have been obtained from recently fed animals. Such a contaminating substance may consist only of inactivated insulin and may thus possess no physiological significance. On the other hand, if it is physiologically significant its secretion by the pancreas might be related to the fact that dogs with metahypophyseal diabetes (Marks and Young, 1939) or with alloxan diabetes (Thorogood and Zimmerman, 1945)—conditions in which the diabetes is associated with destruction of the β -cells of the pancreatic islets while other parts of the pancreas may be left largely untouched—require in many instances significantly more insulin for the adequate control of glycosuria than do fully depancreatized dogs. The secretion of a hepatic glycogenolytic substance by pancreatic tissue other than the β -cells of the islets might explain such an effect (*cf.* Young, 1946). It might be surmised that such a substance could act in conjunction with insulin to assist the transfer of liver glycogen to muscle glycogen through the mediation of blood glucose.

From Fig. 1 it will be seen that the formation of glycogen from glucose in the muscles and in the liver may involve the phosphorylation of glucose as the initial step, this process being catalysed by the enzyme hexokinase. Since insulin promotes glycogen formation from glucose we shall find especial interest in an examination of the influence of insulin on the activity of hexokinase *in vitro* (see p. 31).

4. THE ADRENAL GLANDS IN RELATION TO GLYCOGEN STORAGE

(a) *Adrenaline*.—If, under the influence of insulin or for any other reason, the blood sugar falls below a value of 60–70 mg./100 ml. then, as has been pointed out above, there occurs a stimulation of the sympathetic system, associated with an enhanced liberation of adrenaline, which promotes the conversion of liver glycogen to glucose and thus tends to combat the fall of blood-sugar level. The adrenaline so liberated also catalyses the breakdown of muscle

glycogen, but in this case, as in general, the product is lactic acid and not glucose. There is some evidence that at higher blood-sugar levels (120–140 mg./100 ml.), levels which might be reached in an intact animal with a good store of liver glycogen exposed to emotional stimulation with a consequent outpouring of adrenaline, adrenaline depresses that enhancement of uptake of glucose by the skeletal muscles which is usually manifest when the blood-sugar level rises. At first sight these findings are not obviously in agreement with Cannon's "emergency theory" of the action of adrenaline, according to which the extra liberation of adrenaline during a time of stress, such as fight or flight, assists an animal to greater effort than would otherwise be possible. It might be assumed that in such circumstances the glucose liberated from the liver under the influence of adrenaline ought to be quickly oxidised by the skeletal muscles. On the other hand, the tissues of the heart and brain may hold a position of dominating importance in the hierarchy of the body; since these tissues readily oxidise both glucose and lactic acid under normal conditions, it is perhaps not unreasonable that adrenaline promotes the conversion of liver glycogen to glucose and of skeletal muscle-glycogen to lactic acid, while depressing the tendency of such muscles to absorb the extra glucose if the blood-sugar level rises as the result of the increased hepatic glycogenolysis. Fuel will thus be made plentifully available for the heart and brain, while the skeletal muscles, which are especially able to do so, can obtain their energy largely by a process of anaerobic metabolism for a time.

(b) *Adrenal Cortical Hormones*.—When an animal is deprived of its adrenal cortical secretions the amount of glycogen present in the body may be slightly though not greatly diminished, provided that the food intake is not depressed. A short period of starvation will, however, induce an abnormally rapid disappearance of glycogen both from the liver and from the muscles, and as a result the blood-sugar level may fall to a dangerously low level. The hypoglycæmic action of insulin may be greatly exaggerated in a condition of deficiency of adrenal cortical secretions, and a very small dose of insulin may precipitate fatal hypoglycæmia.

Determination of the Respiratory Quotient of a starving adrenal-deficient animal indicates an abnormally high rate of carbohydrate oxidation, and the rapid disappearance of the stored glycogen may be attributed in part to such rapid oxidation, and in part to a depression of hepatic glyconeogenesis, particularly from amino-acids. When glucose is given to adrenalectomised rats a greater proportion of the administered material is oxidised, and a smaller

proportion is deposited as glycogen, than is the case with the intact animal. This accounts for the tendency of the glycogen stores of the adrenal-deficient animal to be subnormal even though the food intake is good.

Administration to adrenalectomised animals of adrenal steroids with an oxygen atom or a hydroxyl group at position 11 promotes glycogenesis, depresses carbohydrate oxidation, and diminishes the excessive hypoglycæmic effect of insulin administration and of fasting. The activity of such adrenal steroids appears to be more particularly directed towards promoting glycogen storage in the liver than in the muscles; nevertheless, any process which raises the level of glycogen in the liver will almost certainly be capable of doing the same in the muscles, though it may be only in a secondary manner, since the liver and muscle glycogen levels are related by mediation of blood glucose and blood lactic acid (see p. 20). It may, therefore, be concluded that the preservation of stored carbohydrate (1) by depression of the oxidation of carbohydrate and of the action of insulin in promoting the utilisation of glucose and (2) by the stimulation of glycogenesis, is an important function of the secretions of the adrenal cortex.

5. THE ANTERIOR PITUITARY LOBE IN RELATION TO GLYCOGEN STORAGE

In 1924 Houssay and Magenta demonstrated the abnormally high sensitivity to the hypoglycæmic action of administered insulin which results from removal of the pituitary gland (hypophysectomy) in the dog. Subsequently Houssay and others were able to show that removal of the whole pituitary gland, or of the anterior lobe only, diminishes greatly the severity of the diabetic condition of the depancreatized animal. Conversely, the administration of extracts of the anterior-pituitary lobe can induce, in a depancreatized or in a normal dog, a diminution of the hypoglycæmic effectiveness of administered insulin and an exacerbation of, or the appearance of the symptoms of, diabetes. Subsequently it was demonstrated (Young, 1937) that a short period of administration of anterior pituitary extract to normal dogs can induce a persistently diabetic condition associated with serious degeneration of the pancreatic islets.

Russell and others have shown that the adrenalectomised rat and the hypophysectomised rat resemble each other in some respects with regard to the storage and oxidation of carbohydrate, since in both there is an abnormally high rate of carbohydrate oxidation during starvation, associated with a depletion of glycogen stores and

depressed glyconeogenesis. Since, however, the administration of anterior pituitary extract can maintain a normal muscle-glycogen value in starving hypophysectomised-adrenalectomised rats (Bennett and Perkins, 1945) the effect of anterior pituitary secretions on muscle glycogen is not necessarily mediated by the activity of adrenal cortical secretions. Moreover, the influence of anterior pituitary extracts in inducing insensitivity to the hypoglycæmic action of administered insulin (Himsworth and Scott, 1938) and in inducing the appearance of glycosuria in previously non-glycosuric partially-depancreatized dogs (Houssay, 1942; Houssay, Foglia and de Pasqualini, 1946) can be demonstrated in adrenalectomised animals, and it seems certain that the secretions of the anterior pituitary gland can influence carbohydrate metabolism by two pathways, one only of which is mediated by the secretions of the adrenal cortex.

As Lukens (1946) has pointed out, although the actions of insulin and of anterior-pituitary extract are antagonistic in many respects, with regard to muscle glycogen their activities can be regarded as somewhat complementary, insulin stimulating glycogen formation when the animal is fed, while anterior-pituitary secretions maintain the muscle glycogen level during starvation. However, like the appropriate adrenal steroids, anterior pituitary extracts also depress the hypoglycæmic action of insulin, diminish carbohydrate oxidation and increase hepatic gluconeogenesis, reactions which can in no general sense be regarded as complementary to those of insulin.

There is some evidence that in hypophysectomised animals the enzyme system in the liver concerned with the conversion of glycogen to glucose is less sensitive to stimulation by glycogenolytic agents such as adrenaline, so that the blood-sugar level may fall when insulin is injected, without there being a rapid antagonistic breakdown of liver glycogen, under the influence of adrenaline and of sympathetic stimulation, even in the presence of adequate amounts of liver glycogen. The evidence for this is not accepted by all workers in the field, but the experimental support for this view cannot be ignored.

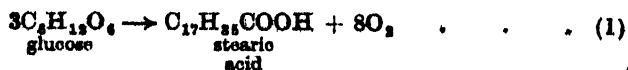
One can assume that the degeneration of the pancreatic islets which may occur in the dog and in the cat treated with suitable anterior pituitary extract is the result of the extreme antagonism to the action of insulin exerted by the pituitary factors, acting both directly and indirectly by way of the adrenal cortex. Under such conditions the islets are damaged, sometimes irretrievably, by overwork. On the other hand, certain anterior pituitary extracts can cause islet hyperplasia in other species, *e.g.* the rat, and it is possible

that there exists a pituitary "pancreatropic" factor which may or may not be identical with other pituitary substances and which stimulates the pancreatic islets to secrete insulin and to hypertrophy. Such an activity would ultimately have an effect largely antagonistic to other actions of the pituitary secretions on carbohydrate metabolism, but might facilitate pancreatic-islet degeneration by overwork under some conditions.

6. THE INFLUENCE OF INSULIN ON THE CONVERSION OF GLUCOSE TO FATTY ACIDS

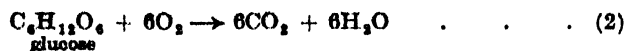
Since the classic experiments of Gilbert and Lawes on the fattening of growing pigs it has been accepted that carbohydrate can be converted to fatty acids and to fat in the animal body, but in only comparatively recent times has the magnitude of such a conversion become apparent. The application of tracer isotopes, and in particular of deuterium, by Stetten (1946) and others has demonstrated not only that such a conversion is of great quantitative significance but has shown unequivocally what was suspected before, namely that insulin catalyses this conversion. Stetten and his colleagues have shown that in the well-nourished rat only about 3 per cent. of the glucose ingested each day is converted to glycogen, while about 30 per cent. is consumed in the manufacture of fatty acids. In the alloxan-diabetic rat the rate of conversion of glucose to fatty acids is much diminished and, as might be expected, the administration of insulin enhances such a conversion. According to Stetten's views glycogen is in the nature of a cul-de-sac with respect to carbohydrate metabolism, the route out being the reverse of that in, and the magnitude of the inflow and of the egress being relatively small, at least in the rat. On the other hand, it is to be presumed that the fat formed from glucose is directly oxidised. Certainly the evidence that any large-scale reconversion of fatty acids to glucose normally occurs in the body is not at present convincing.

In the conversion of glucose to fatty acids a substance relatively rich in oxygen is converted to one rather poor in this element. If we consider the formation of a typical fatty acid, such as stearic acid, from glucose we can write the theoretical equation

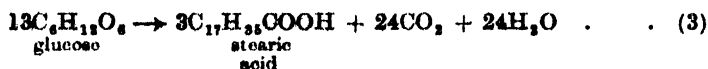


It is unlikely that elemental oxygen would be given off in the tissues in large amounts, and we can consider the possibility that

the oxygen made available is utilised for the oxidation of glucose according to the equation

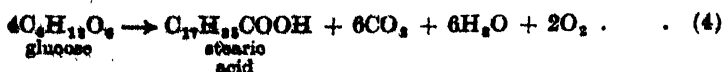


A suitable combination of equations (1) and (2) yields the equation



Since the Respiratory Quotient rises after the administration of glucose to a normal animal, or after the administration of insulin to a diabetic animal under suitable circumstances, and since this rise may be associated with no obvious increase in the rate of oxygen consumption, it has been assumed that insulin has stimulated the oxidation of glucose in such a way that there is a simultaneous equicaloric diminution in the rate of oxidation of protein and fat. Consideration of equation (3) above suggests an alternative explanation. The liberation of CO_2 without concomitant absorption of oxygen, visualised in equation (3), would be added to the respiratory changes already taking place in the animal to which glucose or insulin are administered and would result in a rise in Respiratory Quotient (volume of CO_2 evolved divided by the volume of oxygen absorbed during the same period) of the whole animal. This rise in Respiratory Quotient (R.Q.) would be accompanied by no increase in oxygen consumption and could, therefore, occur under conditions in which there was no rise in Metabolic Rate as determined, by indirect calorimetry, from the oxygen consumption. The classical interpretation of such a rise in R.Q. is that the rate of oxidation of carbohydrate has been increased. In actual fact if equation (3) is physiologically significant, of the 13 molecules of glucose metabolised in the body 9 have been converted to fatty acid and only 4 have been oxidised.

It is possible to combine equations (1) and (2) in a number of different ways, but the form given in equation (3) is the only one in which no oxygen is made available. If we allow the possibility that a little oxygen will be evolved in the process we can write an equation of the following type



The oxygen evolved according to equation (4) would presumably be utilised in the general metabolic processes of the body and would diminish the amount of oxygen absorbed from the air for such

purposes. Such a diminution might compensate for the increased calorific value of oxygen known to be associated with a rise in R.Q., so that the directly measured metabolic rate could be unaltered after the consumption of glucose or the administration of insulin if equation (4) was relevant to the processes occurring in the animal body.

These theoretical considerations emphasise the importance of the demonstration that insulin stimulates the conversion of glucose to fatty acids and indicate that the evidence which has been adduced in the past, from a consideration of the change in R.Q. after the administration of glucose or the injection of insulin, that insulin directly stimulates the oxidation of glucose may have to be reconsidered. Virtually nothing is known about the intermediates concerned in the conversion of glucose to fatty acids although this is a fruitful field for speculation.

THE OXIDATION OF CARBOHYDRATE FOOD IN THE ANIMAL BODY

Since glycogen breakdown and oxidation of carbohydrate are of such importance in the contractive processes of skeletal muscle, it is clear that under normal conditions the combustion of sugar must play a very important role in the economy of the animal body.

During starvation the glycogen stores of the body fall and, as indicated by a diminution in the R.Q., the rate of oxidation of carbohydrate is depressed. Nevertheless, the tissues of the brain, and possibly other active tissues such as those of the heart and contracting skeletal muscle, continue to withdraw glucose from the circulation for oxidative purposes. Under these conditions gluconeogenesis in the liver, chiefly from amino-acids, becomes of paramount importance in preventing a serious fall of blood-sugar level.

Thus during starvation, as during normal existence, the oxidation of glucose is of great importance to the animal body, and we must now consider briefly the metabolic pathways concerned.

1. THE INTERMEDIARY METABOLISM OF CARBOHYDRATE OXIDATION

Fig. 2 summarises in a general way what are at present considered to be the catalytic processes concerned in the metabolism of glucose in the animal body, more particularly in liver and muscle tissues. Fig. 3 outlines the Krebs tricarboxylic acid cycle, while Fig. 4 depicts the general basis of the dehydrogenase-cytochrome system. These cycles between them can account for the complete oxidation of glucose to CO_2 and water in liver or muscle tissue.

Krebs and his colleagues have advanced some evidence that the

oxidative activity of the tricarboxylic acid cycle is enhanced, in pigeon-breast muscle tissue, when insulin is added to the system *in vitro*. Although this has been independently confirmed for pigeon-breast muscle, it does not appear to be of general applicability and, though insulin may directly catalyse the functioning of the tricarboxylic acid cycle in general, that this is so has not yet been generally demonstrated.

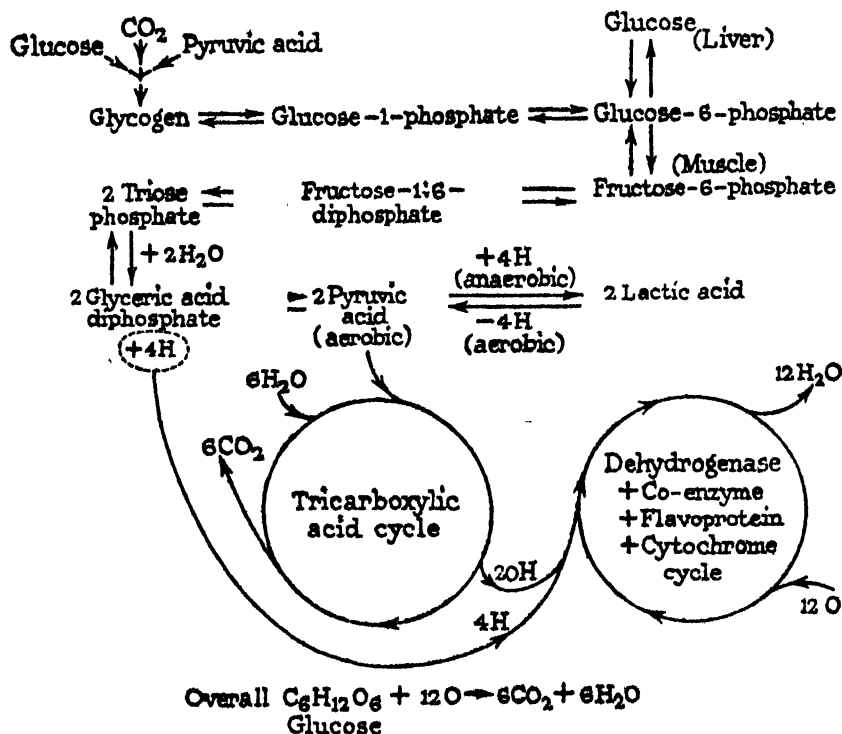


FIG. 2.—Summary of the metabolic fate of glucose.

In their recent monograph Soskin and Levine (1946) have pointed out that the direct evidence that insulin stimulates the oxidation of glucose is not good, and, as we have seen above, the evidence based on a consideration of the R.Q. after insulin administration may be equivocal. On the other hand, there is much evidence to show that the combustion of glucose is depressed in the diabetic animal and that this depression can be overcome by the administration of insulin. It must, therefore, be assumed that insulin stimulates secondarily, if not directly, the oxidation of glucose.

Consideration of Figs. 1 and 2 shows that, according to present views, a preliminary step in the utilisation of glucose for any meta-

bolic processes is its phosphorylation. Such phosphorylation may be indispensable to the passage of glucose across the membrane of the cell, for there is good reason to believe that the cell membrane, unlike cellophan or parchment, is not freely permeable to glucose.

The enzymic hexokinase is of great importance in the phosphory-

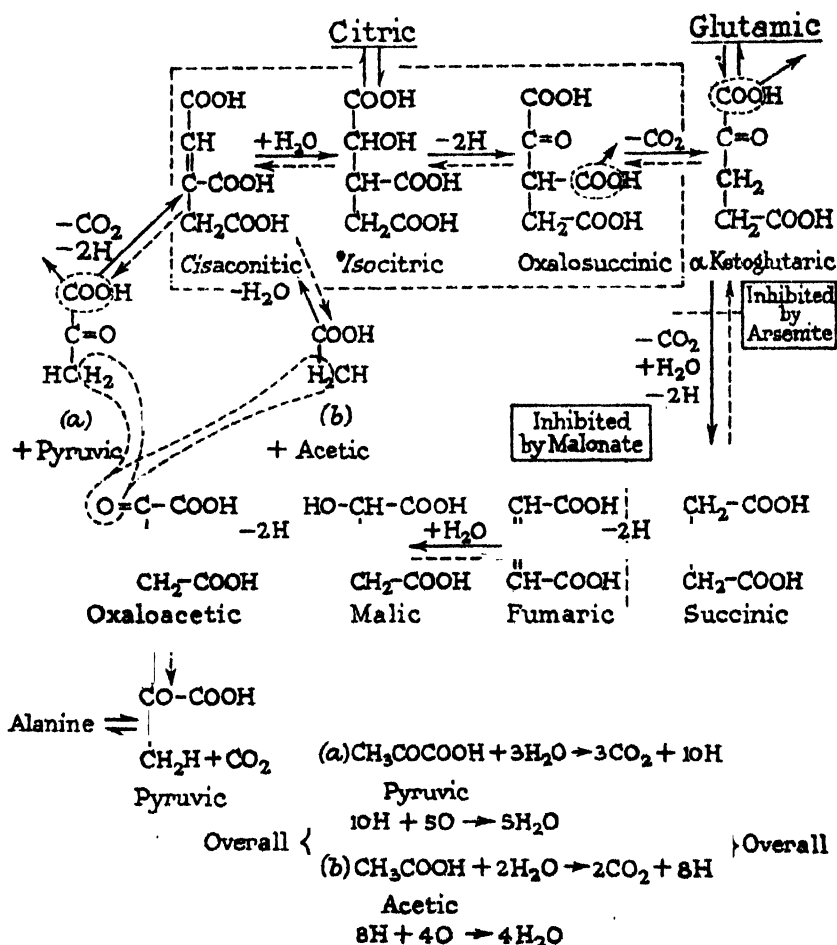


FIG. 3.—Tricarboxylic acid cycle.

lation of glucose and we shall next discuss investigations concerning this enzyme.

2. THE HEXOKINASE REACTION

The enzyme hexokinase is widespread in the animal body and catalyses the formation of glucose-6-phosphate from glucose. The

conversion of glucose to glucose-6-phosphate requires the addition of free energy, *i.e.* it is an endergonic process, and in the body it is coupled with the exergonic conversion of adenosine triphosphate (ATP) to adenosine diphosphate (ADP) (*cf.* Fig. 5). The release of free energy from glucose, either through lactic acid formation or by oxidation, requires, as a preliminary step, the transfer of free energy to the system from an outside source, with the formation of glucose-6-phosphate. We may therefore legitimately regard the obligatory formation of glucose-6-phosphate as an energy barrier which can be surmounted by the co-operative release of free energy from ATP. As Dr. and Mrs. Cori and their collaborators have recently shown (*cf.* Colowick, Cori and Slein, 1947), suitable anterior pituitary extracts can depress the activity of hexokinase from muscle or brain *in vitro*, this depression being reinforced by the addition *in vitro* of certain extracts of the adrenal cortex. Furthermore, the Coris have shown that the depressive action on hexokinase

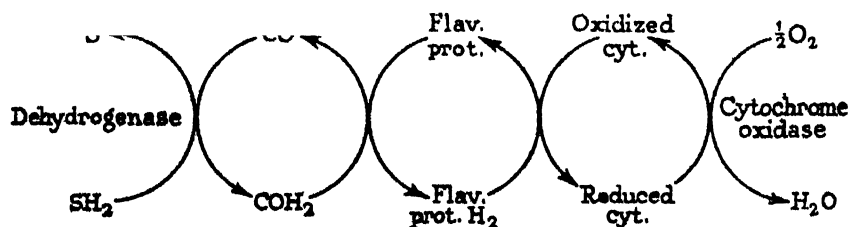


FIG. 4.—Summary of dehydrogenase-cytochrome cycle.

SH₂ = oxidisable substrate; CO = coenzyme; Flav. prot. = flavoprotein; cyt. = cytochrome.

in vitro of anterior pituitary extract plus adrenal-cortical extract can be annulled by the addition of insulin to the system. Thus the antagonism of the anterior pituitary extract and insulin can be demonstrated on an isolated enzyme system. Insulin does not enhance the hexokinase activity of an extract of normal muscle, but the activity of an extract from the muscles of a diabetic rat is elevated by the addition of insulin *in vitro*. It might be suspected that identity would easily be established between the pituitary diabetogenic factor and the pituitary substances which, in inhibiting hexokinase activity, must inhibit the general utilisation and storage of glucose in the body. So far, however, there is evidence that simple identity does not hold, although there is every reason to believe that the two factors are closely related (Reid, Smith and Young, 1947).

It is clear that antagonism with respect to hexokinase activity can explain, to a large degree, the contra actions of anterior pituitary

extract and insulin in many aspects of carbohydrate metabolism (cf. p. 25). But the greatly enhanced hypoglycæmic action of insulin in animals from which the pituitary gland has been removed obviously cannot be explained in terms of this effect. In this respect the positive action of insulin in accelerating the formation

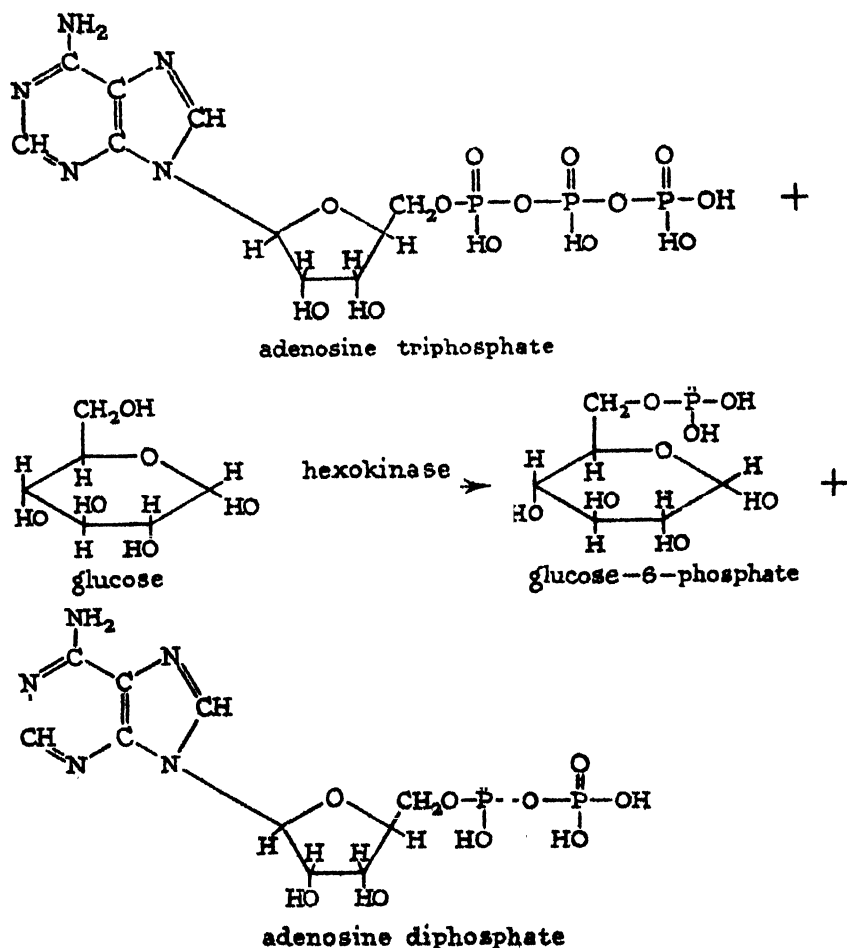


FIG. 5.—The reaction catalysed by the enzyme hexokinase.

of glycogen from glucose-1-phosphate in liver slices *in vitro* may be of significance.

Since insulin facilitates, under some conditions, the preliminary phosphorylation of glucose, it is understandable that it should, therefore, facilitate a large number of metabolic processes each of which is preceded by the phosphorylation of this sugar. Thus,

although insulin may not directly catalyse the oxidation of glucose, it can facilitate the indispensable preliminary phosphorylation without which the catabolic processes leading to pyruvic acid (*cf.* Fig. 2) cannot be set in motion. The oxidation of pyruvic acid may proceed automatically once this substance can be formed from glucose.

Since, in a diabetic condition resulting from insulin deficiency, ketosis develops as well as glycosuria, we must now consider the influence of insulin upon the process of ketogenesis.

KETOGENESIS

In any condition in which carbohydrate oxidation is depressed there appear in the urine, as abnormal constituents, acetoacetic acid ($\text{CH}_3\text{CO}\cdot\text{CH}_2\text{COOH}$), β -hydroxybutyric acid ($\text{CH}_3\text{CHOH}\cdot\text{CH}_2\text{COOH}$) and acetone ($\text{CH}_3\text{CO}\cdot\text{CH}_3$). Acetone is not a primary product, but is formed in the blood and urine by the decarboxylation of acetoacetic acid, and we shall not consider it as a direct product of metabolism. Acetoacetic and β -hydroxybutyric acids are easily interchangeable in the body by simple oxidation or reduction, and we shall refer to these two substances by the traditional but somewhat inappropriate term "ketone bodies."

The ketone bodies are not normally present in the urine, but the blood of a normal animal constantly contains small amounts, of the order of 1-2 mg./100 ml. In the condition of "ketosis" the concentration of these substances in the blood may rise to 50-100 times the normal value and they appear in the urine in large amounts. Ketosis is associated with starvation, a diet deficient in carbohydrate, and with *diabetes mellitus*. In diabetes the condition may become so severe that the accumulation of ketone bodies in the tissues of the body causes death by toxic action and by derangement of acid-base balance.

Since the classic work of Knoop it has been realised that ketone bodies can be formed by the partial oxidation of naturally occurring fatty acids, but there is also evidence that the metabolism of certain amino-acids, particularly those possessing branched carbon chains, can give rise to ketone bodies. We now know from the researches of Stadie, MacKay, Weinhouse, Medes, Floyd, and others that ketone bodies can be formed from fatty acids in the liver by two different but related processes. The first of these is quantitatively the more important and is described as " β -oxidation-condensation." In these processes the bonds between carbon atoms 2 and 3, 4 and 5, 6 and 7, etc., of the fatty acid chain undergo oxidative fission with the formation of two-carbon-atom fragments, which we may formulate, for convenience, as acetic acid, although the identity of the

fragments has not yet been established. There is some evidence that adenosine triphosphate may be concerned in the processes of fatty acid oxidation, although no phosphorylated intermediates have been identified with certainty.

If the two-carbon fragments are not oxidised in the liver they can condense together in pairs to form acetoacetic acid (Fig. 6).

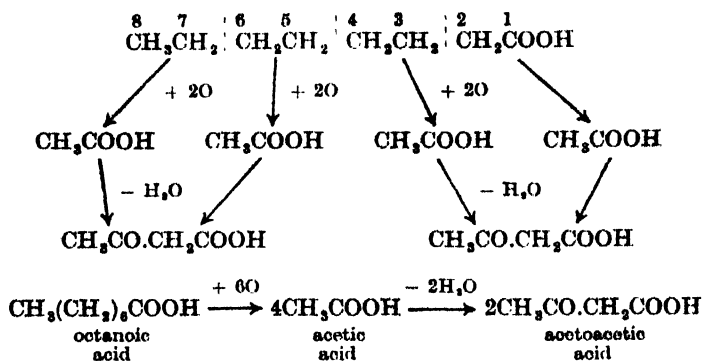


FIG. 6.— β -oxidation-condensation fission of octanoic acid.

The second mechanism for the production of ketone bodies (ketogenesis) from fatty acid in the liver is "multiple alternate oxidation." This is quantitatively much less important than β -oxidation-condensation and results from the fact that the oxidation of the fatty acid chain may lead to fission between carbon atoms 4 and 5, 8 and 9, 12 and 13, etc., while at carbon atoms 3, 7, 11, 15, etc., the oxidation progresses only as far as the formation of keto groups (cf. Fig. 7).

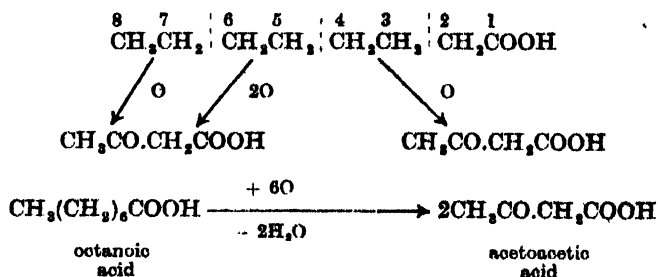


FIG. 7.—Multiple alternate oxidation of octanoic acid.

The important differences between the two processes are: (1) β -oxidation-condensation can give rise to two-carbon fragments without the preliminary production of ketone bodies, while multiple-alternate oxidation cannot; (2) the order of the carbon atoms in

the ketone bodies formed by the second process is necessarily the same as that in the parent fatty acid. That this is not generally so has been demonstrated by Weinhouse, Medes and Floyd, who have in this way produced substantial evidence for the quantitative predominance of β -oxidation-condensation as a mechanism of ketogenesis from fatty acids in the liver.

There is now good evidence that the two-carbon fragments formed from fatty acids by β -oxidation-condensation can be oxidised in the liver by way of the tricarboxylic acid cycle (Fig. 3). There is also reason to believe that acetoacetic acid, formed from fatty acids either by β -oxidation-condensation or by multiple alternate oxidation, can also be incorporated into the tricarboxylic acid cycle and oxidised. It is therefore reasonable to suppose, with Lehninger and others, that, in liver tissue in which the tricarboxylic acid cycle is functioning vigorously, any two-carbon fragments or acetoacetic acid formed by the oxidative fission of fatty acids would be oxidised within the liver itself, and that, therefore, little or no acetoacetic acid would then be liberated into the blood-stream by that organ. Now the components of the tricarboxylic acid cannot themselves be kept at a concentration sufficient to maintain vigorous catalytic oxidation unless pyruvic acid (and possibly other substances concerned in carbohydrate metabolism, including such glucogenic amino-acids as glutamic and aspartic acids) is present in relatively high concentration. Thus, under the influence of the enzyme carboxylase, pyruvic acid and CO_2 combine to give oxaloacetic acid. The latter is also formed by the oxidative deamination of aspartic acid, while α -ketoglutaric acid results from the oxidative deamination of glutamic acid; oxaloacetic and α -ketoglutaric acids are important constituents of the tricarboxylic acid cycle (Fig. 3) and in their absence no oxidation of two-carbon fragments—or of acetoacetic acid—could occur by way of this cycle. Thus, if the cycle wanes as the result of diminished carbohydrate oxidation the liver begins to liberate ketone bodies into the blood-stream.

The ketone bodies liberated by the liver into the circulation can, like glucose, be carried to the muscles and there oxidised. As MacKay has pointed out, it is likely that ketone bodies can be oxidised in the muscles in lieu of glucose when, for any reason, the supply of carbohydrate within the muscle cell is deficient. Whether or not the oxidation of glucose and of ketone bodies always occurs in skeletal muscle tissue by way of the tricarboxylic acid cycle is uncertain. There is also no general agreement as to whether or not in the normal animal ketone bodies are liberated by the liver into the blood-stream in appreciable amounts and carried to the muscles

for oxidation. Some investigators believe that hepatic ketone production occurs only when carbohydrate oxidation is deficient. Whether or not ketone bodies are normal intermediaries of fatty acid metabolism does not affect the importance of the view that they or their immediate precursors—the two-carbon fragments—can be oxidised by a process which also catalyses the oxidation of carbohydrate. Diabetic ketosis can develop because the deficient oxidation of carbohydrate depresses the rate at which ketone bodies can be metabolised in the liver while, at the same time, the deficient oxidation of carbohydrate in the muscles calls for an increased combustion of ketone bodies. The necessary rate of oxidation of ketone bodies in the muscles may be able to take place only at the expense of the presence of relatively huge amounts of ketone bodies in the blood. The toxic effect of this condition then becomes manifest.

Stadie has shown that previous treatment with insulin of depancreatized cats depresses the striking *in vitro* ketogenesis exhibited by respiring liver slices prepared from such animals. It is possible that insulin exerts a direct inhibitory action on the enzyme systems responsible for the production of the ketone body, but it is perhaps more likely that the utilisation of carbohydrate, fostered by the insulin, makes available catalytic amounts of oxaloacetic acid for the functioning of the tricarboxylic acid cycle, with consequent oxidation of the two-carbon fragments or of the ketone bodies formed from them. If the latter postulate is correct insulin influences ketone production only secondarily by way of its more direct action upon carbohydrate utilisation.

GENERAL CONCLUSIONS

The protean activities of insulin on carbohydrate metabolism may be resolved, as we have seen, into actions exerted upon two processes only, both involving glucose phosphate esters; (1) the stimulation of glycogen formation from glucose-1-phosphate under the influence of liver phosphorylase; (2) the phosphorylation of glucose at the expense of adenosine triphosphate under the influence of hexokinase. Insulin probably does not directly stimulate hexokinase activity, but releases the enzyme from the inhibiting influence exerted by adrenal cortical and anterior pituitary hormones acting in unison.

The view that the control of processes in the body can be effected by the balanced opposition of contrary stimuli is not new. In 1936 Young wrote: "The heart has the intrinsic property of beating rhythmically, although the actual rate and type of beat is adjusted

by the mutually antagonistic actions of the sympathetic and parasympathetic nervous systems. Similarly inherent processes of manufacture of sugar in the liver and utilisation in the peripheral tissues probably exist, these processes being intrinsic properties of the relevant tissues; the precise mutual adjustment of the rates of these two processes is effected by the endocrine system, the antagonistic actions of insulin and the pituitary factors playing an important part in this adjustment." By this time we can say that the inherent process of utilisation of sugar in the peripheral tissues may exist in the activity of the enzyme hexokinase, upon which insulin and pituitary factors exert their mutually antagonistic actions. Thus, thanks to the important work of Dr. and Mrs. Cori and their collaborators, we are already able to pin one point of activity of two hormones on to a particular enzyme system. The field of investigation thus opened up is of profound importance.

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SYNTHETIC PERFUMES

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PERFUMERY is an art which has been practised for centuries and references to the use of perfumes are to be found in classical literature and in the Bible. "Take unto thee sweet spices. . . . And thou shalt make it a perfume, a confection after the art of the apothecary. . . ." (Exodus, Chapter xxx).

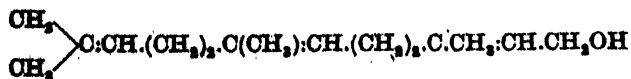
In principle, perfumes are made by mixing essential oils, synthetic perfumery chemicals and products of animal and plant origin, and the actual compounding of perfume mixtures is a task that requires a high degree of aptitude and wide experience. It is an art; thus when comparing the effects produced by different perfumers it is appropriate perhaps to recall Buffon's definition of literary style "Le style est l'homme même." The word "perfume" is used, nowadays, not only to describe the pleasing aroma associated with a natural or artificial mixture of fragrant compounds but also, by custom, the term "synthetic perfumes" is applied to the individual odorous components themselves when prepared synthetically on a commercial scale. In this article an attempt will be made to present an overall picture of the chemistry of the more widely used chemicals—in many instances natural sources are available, but the development of inexpensive synthetic processes of manufacture has brought perfume mixtures within the reach of all classes. Although compounds differing widely in chemical properties can be used to produce a similar odour sensation, it is convenient to discuss these materials as groups of chemical compounds rather than attempt to devise a treatment based on odour similarity. The references to the preparation of individual perfumery chemicals are selective (of necessity in an article of limited length), but the publication of a comprehensive monograph on the subject appears to be warranted.

ALCOHOLS

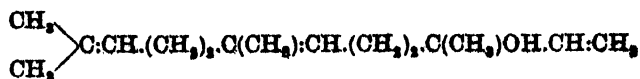
n-Nonyl alcohol, $\text{CH}_3(\text{CH}_2)_7\text{CH}_2\text{OH}$, occurs as its caproate in sweet orange oil and the free alcohol has a sharp rose-like odour. *n*-Decyl alcohol, $\text{CH}_3(\text{CH}_2)_8\text{CH}_2\text{OH}$, which possesses a sweet flower-like odour, has been identified in oil of ambrette seeds and in a number of other essential oils. These alcohols can be prepared synthetically by reducing ethyl pelargonate and ethyl caproate respectively (L. Bouveault and G. Blanc, *G.P.* 164,294/1903). The alcohol, geraniol, which occurs widely in natural essential oils, in

the free state and in the form of its esters, is most probably an inseparable mixture of *cis*-2:6-dimethyl- $\Delta^{1:5}$ -octadien-8-ol and *cis*-2:6-dimethyl- $\Delta^{2:6}$ -octadien-8-ol (J. L. Simonsen and L. N. Owen, *The Terpenes*, Cambridge, 1947, 1, p. 40). The corresponding *trans*-isomer, nerol, possesses a more delicate rose-like odour than geraniol and is invaluable as a constituent of certain delicately balanced perfume mixtures. For this reason a number of processes have been devised for preparing nerol—*e.g.* it is claimed (*G.P.* 462,895/1924) that geraniol is converted into nerol by heating with a metallic alcoholate and that nerol is produced when citral is reduced by Ponndorf's method (*Z. angew. Chem.*, 1926, 39, 138; R. Sornet, *Rev. Chim. Ind.*, 1930, 39, 98). Citronellol is most probably a mixture of 2:6-dimethyl- Δ^1 -octen-8-ol and 2:6-dimethyl- Δ^1 -octen-8-ol (J. L. Simonsen and L. N. Owen, *op. cit.*, p. 36). The *l*-form is a constituent of geranium oil and the *d*-form is obtainable by reducing geraniol (*G.P.* 256,716/1911), citral (R. Adams and B. S. Garvey, *J. Amer. Chem. Soc.*, 1926, 48, 477) or citronellal (H. Rupe and R. Rinderknecht, *Helv. Chim. Acta*, 1924, 7, 541). The synthetic product is much cheaper in price, but the naturally occurring alcohol is regarded more highly for perfumery purposes. 3:7-Dimethyl-octan-1-ol, which is obtainable as a reduction product of geraniol (R. Willstätter and E. W. Mayer, *Ber.*, 1908, 41, 1479) or citronellol (A. Haller and C. Martine, *Compt. rend.*, 1905, 140, 1303), has a soft, rose odour and has been used as an adulterant of otto de rose. It does not appear to have been detected in nature.

The two sesquiterpene alcohols, farnesol and nerolidol, are of considerable perfumery value and bear the same relationship to each other as geraniol and linalool. Farnesol occurs in the form of its esters and in the free state in a number of essential oils, including acacia blossom oil and ambrette seed oil, while the *d*-form of nerolidol is a constituent of orange blossom oil and Peru balsam. Each of these alcohols possesses a mild, very fine, floral odour. *dl*-Nerolidol is prepared synthetically by condensing geranyl chloride with acetoacetic ester and treating the resulting ketone with acetylene and sodamide to produce dehydro-*dl*-nerolidol which is then reduced to *dl*-nerolidol (L. Ruzicka, *Helv. Chim. Acta*, 1923, 6, 492; *B.P.* 213,250/1924). Since nerolidol, on treatment with acetic anhydride, is converted into the acetate of farnesol, this represents a synthetic route for preparing the latter.

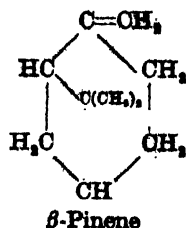
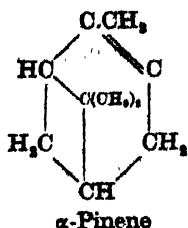


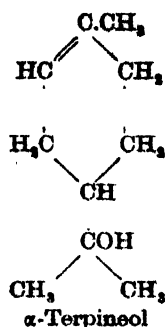
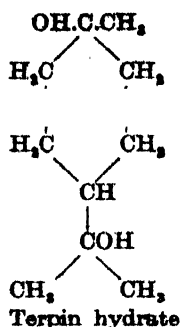
Farnesol



Nerolidol

α -Terpineol is widely distributed in nature as a constituent of essential oils and has a mild odour resembling that of lilac. It is one of the most widely used synthetic perfumes and was one of the first to be produced on a really large scale. For many years turpentine oil was the only raw material, but more recently α -terpineol has been derived from the so-called oil of pine, and this source may replace eventually the earlier synthetic route. As is well known, turpentine oil is prepared by steam-distillation of the oleoresin which exudes from *Coniferae* when incisions are made on the trees. Oil of pine, on the other hand, is produced by destructive distillation or by a steam solvent process from pine wood—usually a mixture of stumps and brushwood—and the second process is stated to yield a superior product. The wood is shredded and subjected to steam distillation to remove the turpentine oil and most of the pine oil and the still residue boiled with a petroleum fraction to extract the resin. The crude oil obtained in the steam distillation process is distilled to give wood turpentine oil, boiling range about 160–170°, and pine oil. When the latter is distilled, 85–90 per cent. passes over between 190–218° and it is the tail end of this fraction which contains most of the α -terpineol (*U.S.P.* 1,743,403/1927; 1,961,398/1928; 2,052,742/1935; 2,052,743/1935). The preparation of α -terpineol from turpentine oil dates from the classical work of Wallach on pinene derivatives (O. Wallach, *Terpen und Campher*). By this process the mixture of α - and β -pinene is converted into terpin hydrate and the elements of water removed from the latter (*inter alia*, H. Tatu, *Technique Moderne*, 1928, 20, 264; S. L. Malowan, *Perf. and Essent. Oil Rec.*, 1933, 24, 81; *U.S.P.* 2,088,030/1936). Under suitable conditions α -terpineol is the main product, although it is obvious that other dehydration products are possible and, owing to the commercial importance of α -terpineol, the reaction has been subjected to rigorous examination.





Benzyl alcohol, $\text{C}_6\text{H}_5\text{CH}_2\text{OH}$, has been identified in various essential oils and possesses a mild, pleasant aromatic odour. This alcohol is used extensively as a constituent of artificial blossom oils and is produced synthetically, in quantity, by treating benzyl chloride with a solution of sodium carbonate. Although β -phenyl ethyl alcohol, $\text{C}_6\text{H}_5\text{CH}_2\text{CH}_2\text{OH}$, was obtained by Radzissewski in 1876 (*Ber.*, 1876, 9, 372) as a reduction product of phenyl acetaldehyde, the alcohol was not detected in nature until 1900 when Walhbaum (*Ber.*, 1900, 33, 1904, 2299) showed it to be present in otto of rose and v. Soden and Rojahn (*Ber.*, 1900, 33, 3063) detected it in rose water (obtained by steam distillation of roses). The alcohol is another of the chemicals used widely by the present-day perfumer and the literature dealing with its preparation is extensive. Of the routes available the one most favoured for large-scale preparation appears to involve reduction of ethyl phenylacetate (*inter alia*, I. Herold, *Deut. Parfümerie-Ztg.*, 1931, 17, 54). Ittner (*Chem. and Ind.*, 1942, 20, 139) has recently drawn attention to the fact that, when using sodium and ethyl alcohol for the reduction, it is essential to use pure sodium and alcohol from which all traces of water have been removed, as the slightest trace of sodium hydroxide brings about hydrolysis of the ethyl phenylacetate, rendering high yields of the β -phenyl ethyl alcohol impossible of attainment. Darzens (*Compt. rend.*, 1937, 205, 684) prepared the alcohol by reducing the triglyceride of phenyl acetic acid, and the Grignard method (*ibid.*, 1905, 141, 44), involving decomposition of $\text{C}_6\text{H}_5\text{CH}_2\text{CH}_2\text{OMg.Br.}$, was reinvestigated later (*F.P.* 807,632/1937; *U.S.P.* 2,058,373/1935). Processes have been devised more recently for producing β -phenyl ethyl alcohol by the Friedel-Craft method from benzene and ethylene oxide in the presence of suitable condensing reagents (*U.S.P.* 2,029,618/1931; 2,047,396/1933; 2,013,710/1933).

An increasing use is being made of various carbinols and their derivatives, some of which possess extremely pleasant, delicate

odours, e.g. dimethylbenzyl-carbinol, $C_6H_5.CH_2.C(CH_3)_2.OH$ (V. Grignard, *Compt. rend.*, 1900, **130**, 1322; A. Reclaire, *Deut. Parfümerie-Ztg.*, 1929, **15**, 285), has a faint pleasing odour recalling that of freshly cut green wood with a flowery note of lily of the valley. The acetate of this carbinol is of importance in view of its mild hyacinth-lily odour.

ETHERS

Diphenyl ether ("geranium crystals"), $C_6H_5.O.C_6H_5$, has a strong and penetrating odour of geranium and is utilised widely in soap perfume compositions. It can be obtained in high yield by heating bromo-benzene with potassium phenoxide in the presence of a copper catalyst (F. Ullmann and P. Sponagel, *Ber.*, 1905, **38**, 2211) or from chlorobenzene under suitable conditions, e.g. (U.S.P. 2,008,987/1933). β -Naphthol methyl ether ("nerolin," "yara-yara") has, when diluted in perfume mixtures, an odour somewhat resembling neroli oil. Perhaps the most convenient and economical method of preparing the ether is by the action of methyl sulphate on the sodium salt of β -naphthol (R. Sornet, *La Technique Industrielle des Parfums Synthétiques*, Paris, 1923). β -Naphthol ethyl ether ("bromelia") has a rather more delicate odour and has been prepared by heating a mixture of ethyl alcohol, β -naphthol, and sulphuric acid (L. Gattermann, *Annalen*, 1888, **244**, 72); by the interaction of nitroso diethyl urea and β -naphthol (G.P. 189,843/1906; 224,388/1906) and from β -naphthol and ethyl sulphate (A. R. Cade, *Chem. Met. Eng.*, 1923, **29**, 320). The isobutyl ether of β -naphthol has a delicate fruity odour and is used to a limited extent in perfumery under the name "Fragarol." The four ethers mentioned above have not been detected in nature but were added to the armoury of the perfumer from the laboratory of the organic chemist.

ALDEHYDES

Higher aliphatic aldehydes are used extensively by perfumers to provide in perfume mixtures a "rounding-off" effect and an immediate particular odour sensation ("top note"). *n*-Octaldehyde, $CH_3.(CH_2)_6.CHO$, which is a minor constituent of lemon and lemongrass oil, has a sharp lemon odour. It can be prepared synthetically by a number of methods, but is derived technically from coconut oil. The oil is saponified, the mixture of acids produced is esterified with methyl alcohol, and the methyl *n*-caproate obtained on fractional distillation is reduced with sodium and ethyl alcohol to *n*-octyl alcohol which is then oxidised to *n*-octaldehyde (A. Wagner, *Die Aldehyde*, 1930, Vol. III, p. 413). *n*-Nonaldehyde,

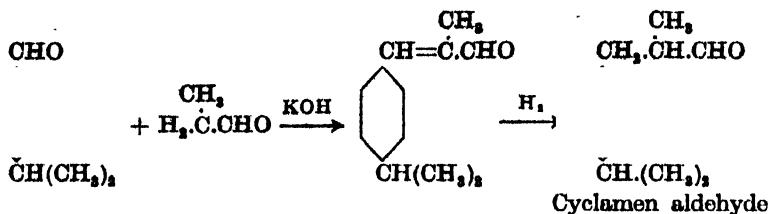
$\text{CH}_3(\text{CH}_2)_7\text{CHO}$, which occurs in a number of essential oils, possesses a rosy-tangerine-like odour and can be prepared by the standard organic chemical methods. A convenient method of preparation is to oxidise *n*-nonyl alcohol by passing the vapour of the alcohol at 3–5 mm. over a copper catalyst (A. Lewinsohn, *Perf. and Essent. Oil Rec.*, 1924, 15, 12) or by using chemical oxidising agents. The aldehyde can also be obtained by the decomposition of oleic acid ozonide (*G.P.* 321,567/1918; *Amer. Chem. Abstr.*, 1940, 34, 981). Stephan (*J. pr. Chem.*, 1900 (2), 62, 523) detected *n*-decaldehyde, $\text{CH}^a_3(\text{CH})^a_8\text{CHO}$, in sweet orange oil in 1900, although the aldehyde had been prepared as early as 1833 by dry distillation of a mixture of the barium salts of formic and caproic acids (see Krafft, *Ber.*, 1883, 16, 117). The aldehyde has a spicy-orange-like odour. Of the preparative methods available that of Bagard (*Bull. Soc. Chim.*, 1907 (iv), 1, 308, 356) by the action of heat upon α -hydroxyundecic acid is often employed. According to a recent report the aldehyde can be obtained in high yield from the oil steam-distilled from the green portions of the coriander plant (*Amer. Chem. Abstr.*, 1942, 36, 3781) and such a natural source might render the synthetic approach uneconomical. Although *n*-undecaldehyde, $\text{CH}_3(\text{CH}_2)_9\text{CHO}$, does not appear to have been detected in nature, the compound has a tenacious odour embodying a note reminiscent of rose and it is used to a considerable extent in perfumery. The classical method of preparing this aldehyde from α -hydroxy-lauric acid has recently been reinvestigated by Davies and Hodgson (*J. Soc. Chem. Ind.*, 1943, 62, 128) and high yields were obtained by distilling the hydroxy-acid in an atmosphere of carbon dioxide. *n*-Dodecaldehyde (lauric aldehyde), $\text{CH}_3(\text{CH}_2)_{10}\text{CHO}$, was isolated from oil of *Abies pectinata* in 1904, although it was probably first detected in nature by Gerhardt in 1848 (*J. pr. Chem.*, 1848, 45, 329) when he reported that oil of rue reduced a silver solution. The aldehyde has a fatty, pungent odour, but in dilute solution the aroma bears a resemblance to that of the wild violet. The preparation of this aldehyde by oxidising lauryl alcohol has also recently been reinvestigated by Davies and Hodgson (*J.C.S.*, 1943, 282). It has been stated that the aldehyde can be produced by passing a mixture of the vapours of formic acid and lauric acid over a manganous oxide catalyst at elevated temperatures (S. S. Nametkin and O. M. Khol'mer, *Amer. Chem. Abstr.*, 1942, 36, 3629); by reaction between calcium laurate and formaldehyde in the presence of steam (*U.S.P.* 2,145,801/1936) and by treating lauric acid with hydrogen at elevated temperatures in the presence of a mild hydrogenation catalyst (*U.S.P.* 2,105,540/1935). Another aldehyde of considerable importance

in perfumery, not yet detected in nature, is methyl-*n*-nonylacetaldehyde, $(C_6H_{11})_2CH.(CH_2)_8CHO$. It has a persistent odour resembling that of a lemon-orange-ambergris mixture and is readily prepared by distilling *in vacuo* the glycidic acid obtained by saponifying the ester prepared by condensing methyl-*n*-nonyl ketone with ethyl monochloroacetate (*F.P.* 337,175/1903). According to Verley (*Bull. Soc. Chim.*, 1923 (iv), 43, 848) the widely used hydroxy-citronellal, probably a mixture of $(CH_3)_2C(OH).(CH_2)_3CH.(CH_2)_3CH_2CHO$ and $(CH_3)(CH_2OH).CH.(CH_2)_3CH.(CH_2)_3CH_2CHO$, was introduced into perfumery as early as 1905, in particular as a constituent of the famous lily of the valley perfume, although details of preparation had not then been published. It can be prepared by the action of sulphuric acid on citronellal-bisulphite, -cyanohydrin or -enolacrylate and, in view of the obvious difficulty in securing high yields and the economic importance of the resulting hydroxy-citronellal, a good deal of effort has gone into investigation of optimum reaction conditions, *e.g.* in *U.S.P.* 2,306,332/1941 it is claimed that improved yields are obtained by treating citronellal-hydrosulphonic acid with hydrochloric acid.

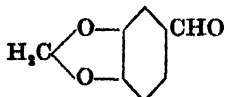
Benzaldehyde, C_6H_5CHO , is the main constituent (98–99 per cent.) of the essential oils of bitter almonds, peach kernels and apricot kernels. It was first detected in bitter almonds by Montres as early as 1803, but was first isolated and studied by Liebig and Wohler in 1837. The main perfumery use of benzaldehyde lies in the preparation of cheap almond perfumes for toilet soap. Commercially the aldehyde is prepared from toluene by direct oxidation or through the intermediate side chain halogen derivatives (*inter alia*, J. McLang, *Chem. Trade J.*, 1926, 79, 191, 307). Anisaldehyde (*para*-methoxybenzaldehyde), $(1)OHC.C_6H_4.OCH_3(4)$, has an odour resembling blossoming hawthorn, although its presence there has not yet been proved. The aldehyde is widely used and is manufactured on a large scale; the processes employed have been reviewed by McLang (*Chem. Trade J.*, 1926, 79, 190) and others. It is prepared by oxidising anethole, or *p*-cresyl methyl ether or from anisole (methoxybenzene) by the Gattermann method (*inter alia*, E. L. Niedzielski and F. F. Nord, *J. Amer. Chem. Soc.*, 1941, 63, 1462). Although cinnamaldehyde, $C_6H_5CH:CH.CHO$, is rarely used in perfumery, its reduction product dihydrocinnamaldehyde, $C_6H_5CH_2CH_2CHO$, (P. E. Weston and H. Adkins, *J. Amer. Chem. Soc.*, 1929, 51, 2589), has a strong hyacinth-like odour and finds a place in certain perfume mixtures. Phenylacetaldehyde, $C_6H_5CH_2CHO$, is widely used as a base for hyacinth perfumes and its dimethyl acetal is also used extensively in perfumery. Although

this aldehyde has not been detected in nature, its occurrence in essential oils containing β -phenylethylalcohol is suspected. The aldehyde is usually prepared by oxidising β -phenylethylalcohol with chromic acid (A. Wagner, *op. cit.*, 1929, Vol. II, p. 570) or by catalytic methods (*Amer. Chem. Abstr.*, 1942, 36, 3794). Phenylacetaldehyde has also been produced by methods based on the observation of Erlenmeyer and Lipp (*Annalen*, 1883, 219, 182; W. Dieckmann, *Ber.*, 1910, 43, 1036; *cf. G.P.* 107,228/1898; *B.P.* 372,013/1931) that when a solution of the sodium salt of phenylglycidic acid is acidified carbon dioxide is evolved and phenylacetaldehyde is obtained on steam distillation. Various routes are available, *e.g.* Rosenmund and Zetzsche (*Ber.*, 1921, 54, 437) hydrogenated a solution in toluene of the acid chloride of phenylacetic acid using a palladium-barium sulphate catalyst and obtained an 80 per cent. yield of the aldehyde. Phenylethanalimine (obtained by hydrogenating benzyl cyanide) decomposes into ammonia and phenylacetaldehyde on treatment with potassium hydroxide solution (V. Grignard and R. Escourrou, *Compt. rend.*, 1925, 180, 1884; H. Stephens, *J.C.S.*, 1925, 127, 1874; I. Herold, *Deut. Parfümerie-Ztg.*, 1931, 17, 246). It is interesting that *p*-methylphenylacetaldehyde has a pungent odour resembling that of phenylacetaldehyde, whereas *p*-methoxyphenylacetaldehyde has a hawthorn blossom type of odour. Vanillin (4-hydroxy-3-methoxybenzaldehyde) is present to the extent of 1.5 to 2.5 per cent. in vanilla pods. The present wide use of vanillin in perfumery, confectionery and foodstuffs developed alongside the steady improvements in synthetic methods of preparation. Vanillin was first produced on a commercial scale, as an alternative method to extraction from vanilla pods, by oxidising coniferyl alcohol (*G.P.* 576/1877). In 1876 Reimer (*Ber.*, 1876, 9, 423) applied his general reaction method and showed that the aldehyde could be prepared by heating guaiacol, chloroform and aqueous sodium hydroxide. Other methods have been based upon the interaction of guaiacol with formaldehyde (*G.P.* 105,798/1898; *B.P.* 161,679/1920) and methylation of proto-catechuic aldehyde (*G.P.* 122,851/1900). The most widely used method of preparation is still that from eugenol (main constituent of oil of cloves). The eugenol is converted by treatment with potassium hydroxide into the isomeric isoeugenol and this phenol then converted into vanillin by oxidation under suitable conditions (*inter alia*, A. Wagner, *op. cit.*, 1930, III-IV, 1940). Various processes have been suggested for converting safrole into isoeugenol (*e.g.* *B.P.* 285,156/1926), but have not proved successful commercially up to the present. During recent years methods have been

developed for producing vanillin from lignin containing materials (*inter alia*, R. H. G. Creighton, J. L. McCarthy and H. Hibbert, *J. Amer. Chem. Soc.*, 1941, **63**, 312, 3049; I. A. Pearl, *ibid.*, 1942, **64**, 1429; K. Freudenberg, W. Lautsch and K. Engler, *Ber.*, 1940, **73**, 167). Ethyl vanillin (4-hydroxy-3-ethoxybenzaldehyde) is finding a growing use in modern perfumery and flavouring materials, as its odour and flavour is about 3 to 4 times as intense as that of vanillin and the aroma is claimed to be closer to that of vanilla pods. The literature (mainly as subject matter of patent specifications) is extensive and it is only possible to give brief outlines of some of the routes developed. The aldehyde can be prepared by the Reimer-Tiemann method from pyrocatechol monoethyl ether (*G.P.* 90,395/1894), by ethylating protocatechuic aldehyde (*G.P.* 81,071/1894) and by a number of methods from safrole or isosafrole (*B.P.* 285,451/1927; 285,156/1926; 317,381/1928; 290,649/1928; 309,929/1927; *G.P.* 557,547/1926; 562,007/1927; 562,008/1927; 505,404/1925). Other methods for preparing ethyl vanillin have been described, such as oxidation of 4-hydroxy-3-ethoxy-phenylglycollic acid or of α -(3-ethoxy-4-hydroxy-phenyl)- $\beta\beta$ -trichloroethanol (*B.P.* 453,482/1935; 445,263/1933; *Dutch P.* 41,184/1935) and of 1-hydroxy-2-ethoxy-4-methylbenzene (*Chem. Zentr.*, 1938, ii, 1861). α -*n*-Amyl-cinnamaldehyde, $C_6H_5.CH:C(C_5H_{11}).CHO$, is a synthetic chemical not yet detected in nature, with a jasmine-like odour. It was not until 1928 that Rutowski and Korolew (*J. pr. Chem.*, 1928, (ii), **119**, 272) first described the preparation of α -*n*-amyl-cinnamaldehyde by condensing benzaldehyde with *n*-heptaldehyde, although it appears that in 1922 a French firm marketed a "jasmine" aldehyde under the trade name "Floral" and this product was eventually shown to consist mainly of this aldehyde. It is an interesting example of the difficulty of prediction in the matter of likely perfumery compounds that Weizmann (*J. Amer. Chem. Soc.*, 1944, **66**, 310) reported recently that the aldehydes derived by condensing anisaldehyde with heptaldehyde and heliotropin with heptaldehyde were of little interest in perfumery. Another interesting compound which has made its appearance during recent years is the so-called "cyclamen aldehyde," *p*-isopropyl- α -methyl-dihydrocinnamaldehyde, $p-C_6H_4.C_6H_4.CH_2.CH(CH_3).CHO$. It has a powerful blossom odour, resembling a bouquet of freshly cut cyclamen, lily, and lily of the valley flowers. The discoverers of this aldehyde, Knorr and Weissenborn (*U.S.P.* 1,844,013/1929), described a method of preparation depending upon the condensation of cuminaldehyde with propionaldehyde and reduction of the resulting *p*-isopropyl- α -methyl-cinnamaldehyde to the required aldehyde.



Poizat (*U.S.P.* 2,242,322/1938) has since devised an alternative method of preparation. Heliotropin (piperonal)



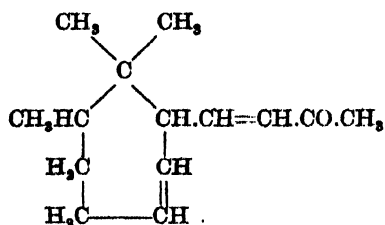
is one of the most well-known and important synthetic perfumery chemicals. Ciamician and Silber (*Ber.*, 1890, **23**, 1159) obtained the aldehyde by oxidising *isosafrole* and the latter is still the raw material used (*see*, for example, J. McLang, *Chem. Trade J.*, 1926, **79**, 359).

KETONES

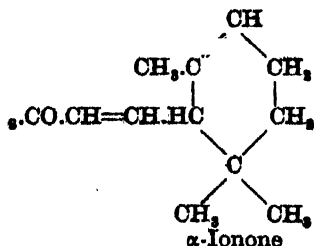
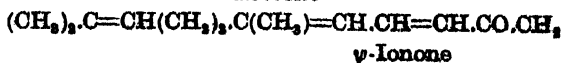
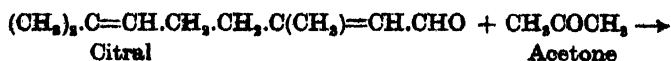
Ethyl-*n*-amyl ketone, $\text{C}_2\text{H}_5\cdot\text{CO}\cdot\text{C}_5\text{H}_{11}$, has a somewhat fruity odour and occurs naturally in French lavender oil, imparting to it a particular freshness. It is probably obtained most readily by oxidising ethyl-*n*-amylcarbinol (*Chem. Zentr.*, 1912, i, 1717). Acetophenone, $\text{C}_6\text{H}_5\cdot\text{CO}\cdot\text{CH}_3$, has a fragrant, hawthorn-like odour and has been detected in the essential oil obtained by steam-distillation of gum labdanum. The ketone can be prepared by the general methods of organic chemistry and special methods have been evolved for large-scale production, *e.g.* oxidation of ethyl benzene with air at elevated temperatures in the presence of a suitable catalyst (*U.S.P.* 1,813,606/1927). *p*-Methoxyacetophenone, $(1)\text{CH}_3\text{O}\cdot\text{C}_6\text{H}_4\cdot\text{CO}\cdot\text{CH}_3(4)$, (trade name "Crataegon"), has an intense hawthorn blossom odour and is especially useful in soap perfumes or in cases where anisaldehyde is unsuitable. *p*-*iso*-Propylacetophenone has an odour resembling that of orris root. Benzyl acetone, $\text{C}_6\text{H}_5\cdot\text{CH}_2\cdot\text{CH}_2\cdot\text{CO}\cdot\text{CH}_3$, has a jasmin-like odour and can be produced, for instance, by saponifying β -benzylacetopyruvic ester (A. Kötze and E. Lemien, *J. pr. Chem.*, 1914 (ii), **90**, 389). Benzylideneacetone, $\text{C}_6\text{H}_5\cdot\text{CH}:\text{CH}\cdot\text{CO}\cdot\text{CH}_3$, which has a tenacious odour resembling that of sweet pea, can be obtained readily by condensing acetone with benzaldehyde in the presence of sodium hydroxide solution (N. L. Drake and P. Allen, *Organic Syntheses*, 1941, Collective Vol. 1, 77). Benzoylacetone, $\text{C}_6\text{H}_5\cdot\text{CO}\cdot\text{CH}_2\cdot\text{CO}\cdot\text{CH}_3$, has a fragrant balsamic odour and can be prepared from ethyl benzoate

and acetone using sodamide as condensing agent (L. Claisen, *Ber.*, 1905, **38**, 695).

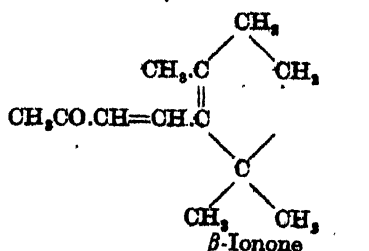
When the ketone irone was first isolated from orris root, Tiemann and Krüger (*Ber.*, 1893, **26**, 2675) assigned an empirical formula, $C_{13}H_{20}O$. Although irone is now known to be a mixture of ketones, mainly $C_{14}H_{22}O$ (L. Ruzicka *et al.*, *Helv. Chim. Acta*, 1933, **16**, 1143; 1940, **23**, 935, 959; 1941, **24**, 1434; 1942, **25**, 188; A. E. Gillam and T. F. West, *J.C.S.*, 1942, 95, 483), the main constituent, possessing the probable structure below, has not yet been prepared synthetically.



However, in attempts to synthesise irone to which the incorrect empirical formula $C_{13}H_{20}O$ had been assigned, Tiemann and Krüger obtained α - and β -ionone and these products are used widely in perfumery because of their violet odour. Owing to the importance of these compounds, methods of preparation and separation have been examined closely and numerous patents have been granted. However, essentially, the chemistry involved remains as it was left by Tiemann and Krüger in 1893. Citral is condensed with acetone to give ψ -ionone, and this alicyclic ketone is converted into a mixture of the isomeric cyclic ketones α - and β -ionone by heating with dilute acid and glycerin. Detailed references to the literature have been given elsewhere (T. F. West, *Thorpe's Dictionary of Applied Chemistry*, in press).



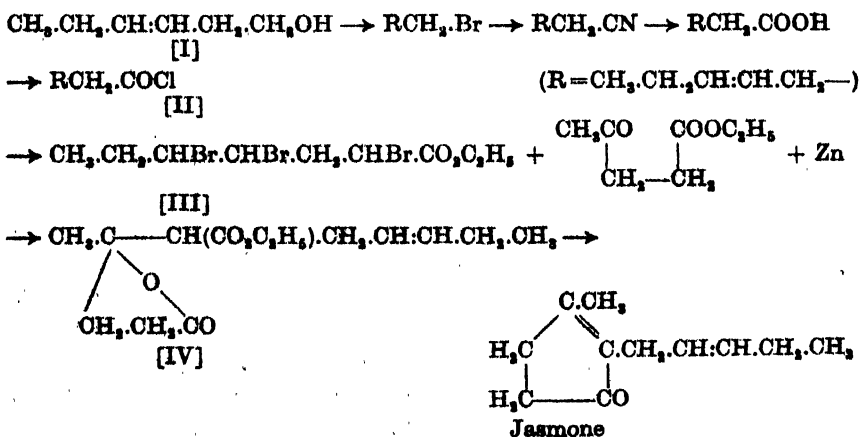
(α -cyclocitrylideneacetone)



(β -cyclocitrylideneacetone)

The compounds ("methyl ionones") prepared by condensing citral and methyl ethyl ketone and cyclisation (*G.P.* 75,120/1893; 127,424/1901; G. W. Pope and M. T. Bogert, *J. Org. Chem.*, 1937, 2, 276) have a characteristic violet odour which is rather stronger than that of the ionones themselves. According to Köster (*J. pr. Chem.*, 1935, (ii), 143, 249; *Deut. Parfümerie-Ztg.*, 1935, 21, 259), the four isomeric methyl ketones, α -*n*-methylionone, β -*n*-methylionone, α -*isomethylionone* and β -*isomethylionone* can be separated satisfactorily from the commercial product by fractional crystallisation of the semicarbazones.

Methyl- β -naphthyl ketone, $\text{CH}_3\text{CO.C}_{10}\text{H}_7$, has a fine neroli-like odour and can be obtained with a minimum amount of the α -naphthyl ketone by using nitrobenzene as solvent when condensing acetyl chloride with naphthalene in the presence of aluminium chloride (A. St. Pfau and A. Ofner, *Helv. Chim. Acta*, 1926, 9, 669). As early as 1899 Hesse (*Ber.*, 1899, 32, 2617) isolated from jasmine blossom oil 3 per cent. of a ketone, $\text{C}_{11}\text{H}_{16}\text{O}$, possessing the characteristic jasmine odour and named this compound jasmone. However, it was not until 1933 that the structural formula given below, involving a five-membered carbon ring, not previously encountered among monocyclic constituents of essential oils, was assigned to jasmone independently by Treff and Werner (*Ber.*, 1933, 66, 1521) and by Ruzicka and Pfeiffer (*Helv. Chim. Acta*, 1933, 16, 1208). Two years later Treff and Werner (*Ber.*, 1935, 68, 640, *cf.* H. Hunsdiecker, *Ber.*, 1942, 75, 460) described a synthesis of jasmone according to the following scheme:



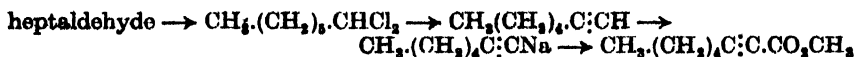
The starting material, *n*-hex-3-en-1-ol [I], was obtained from Japanese peppermint oil and was readily converted by the steps shown into

the acid chloride [II]. This was converted into the tri-bromo-ester [III] which was condensed with laevulinic ester in the presence of zinc, dehalogenation taking place, to give the lactone [IV]. On treatment with sodium followed by dilute mineral acid an oil was formed which provided a semicarbazone of appropriate melting point, and the ketone regenerated therefrom had properties and derivatives identical with natural jasmone. The synthesis of jasmone and similar ketones and the patent position have been reviewed by Werner (*Fette und Seifen*, 1938, 45, 623). After examining a number of synthetic five- and six-membered ring ketones with an alkyl group attached to a carbon atom adjacent to the carbonyl group, Werner came to the conclusion that, in general, these compounds can only be utilised in perfumery when the number of carbon atoms in the molecule are more than C_9 or less than C_{15} . The actual jasmone odour is strongest when the formula is $C_{11}H_{16}O$; among lower members the odour is more aromatic-fruity, while the higher members are more flowery in odour. Usually ketones with a straight chain substituent have a more agreeable bouquet than their isomerides with a branch chain. According to Werner synthetic jasmone is considerably cheaper than the natural ketone but the odour of jasmone is shared by dihydrojasmone, which is synthesised more cheaply, and other synthetic ketones of suitable structure (see above) simulate the jasmone odour.

ESTERS

Esters are used extensively in the perfumery industry—especially those derived from terpenic alcohols. In general, these compounds can be prepared by the standard methods of organic chemistry, but a brief outline of some of the procedures adopted for the preparation of certain widely used esters is included. Linalyl acetate has an odour about identical with that of terpeneless bergamot oil (so-called terpeneless oils are prepared by removing by distillation or other means as high a proportion as possible of the terpenes and sesquiterpenes from essential oils). Both natural and synthetic linalool (and therefore presumably the acetate also) consists of an inseparable mixture of 2:6-dimethyl- $\Delta^{1:7}$ -octadien-6-ol and 2:6-dimethyl- $\Delta^{1:7}$ -octadien-6-ol (J. L. Simonsen and L. N. Owen, *The Terpenes*, 2nd ed., Cambridge, 1947, Vol. I, p. 62). The pure ester can be prepared by treating the sodium salt of linalool with acetic anhydride (F. Tiemann, *Ber.*, 1898, 31, 839), but direct esterification of the alcohol is difficult, because with acetic anhydride acetylation is accompanied by isomerisation and cyclisation, so that a mixture

of the esters of linalool, geraniol, nerol and terpineol is formed together with terpenes produced by loss of the elements of water. It is claimed that satisfactory yields are obtainable by acetylating in the presence of an inert diluent such as xylene (R. Knoll and A. Wagner, *Synthetische und Isolierte Riechstoffe*, Halle a.S., 1928, p. 211; cf. R. Fornet, *Seifens.-Ztg.*, 1936, **63**, 707, 739) or by carrying out acetylation below 40°, using as catalyst a small proportion of an acetic acid-phosphoric acid mixture (V. Isagulyantz and E. Smolyaninova, *Riechstoffindustrie*, 1933, **8**, 194). Terpinyl acetate has a mild flowery odour and is widely used in soap perfumes of the cologne and lavender type. Satisfactory yields of the acetate of the tertiary alcohol α -terpineol have been obtained by refluxing with acetic anhydride using xylene or French turpentine oil as a diluent (H. Paillard and P. Tempia, *Helv. Chim. Acta*, 1931, **14**, 1314). Bertram (*G.P.* 67,255/1892) showed that the ester could be separated from the products obtained on treating turpentine oil (pinene) with acetic anhydride in the presence of sulphuric acid. Kuwata (*Amer. Chem. Abstr.*, 1937, **31**, 3899) obtained a mixture of the acetates of borneol and terpineol by treating α -pinene with a suspension of Japanese clay in acetic acid or by similar methods from limonene (*ibid.*, 1939, **33**, 3899). The interesting synthetic chemical, methyl-*n*-hept-1-yne-1-carboxylate ("methyl heptine carbonate"), $\text{CH}_3(\text{CH}_2)_4\text{C}\equiv\text{C}\cdot\text{CO}_2\text{CH}_3$, has a violet odour and is used in perfume mixtures. Valli-Donan (*Amer. Perfumer*, 1923, **18**, 133) has described its preparation by the following series of steps:



Methyl anthranilate, $\text{C}_6\text{H}_4\cdot\text{NH}_2\cdot\text{COOCH}_3$, has been detected in a number of essential oils and the synthetic ester is widely used in compounding perfumes. It can be prepared by treating anthranilic acid with methyl alcohol in the presence of suitable reagents such as sulphuric acid (H. Meyer, *Monatsh.*, 1904, **25**, 1201) or boron trifluoride (F. J. Sowa and J. A. Nieuwland, *J. Amer. Chem. Soc.*, 1936, **58**, 271). The ethyl ester, which can be prepared by similar methods, has an orange-blossom odour similar to that of the methyl ester, but of a finer and sweeter character. Methyl-*N*-methyl anthranilate, $\text{C}_6\text{H}_4(\text{NHCH}_3)\cdot\text{COOCH}_3$, was detected in 1900 in tangerine oil and the synthetic ester has since been utilised widely as a constituent of artificial tangerine and neroli oils and in perfume mixtures of various types. *N*-Methyl-anthranilic acid is readily prepared as the reaction product between anthranilic acid and methyl sulphate in alkaline solution (R. Willstätter and W. Kahn,

Ber., 1904, **37**, 401) or in acetic acid solution (J. Houben and W. Brassert, *ibid.*, 1906, **39**, 3233) and can be converted into the methyl ester by the usual esterification methods (*G.P.* 122,568/1900).

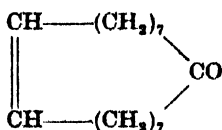
NITROGEN-CONTAINING BODIES

In 1891 Baur (*Ber.*, 1891, **24**, 2832; *G.P.* 47,599/1888; 62,362/1891) drew attention to the fact that various nitro-derivatives of benzene homologues possessed an odour of musk. This work was extended by Baur and others and a number of such bodies have found wide use in perfumery. Xylene musk (2:4:6-trinitro-1:3-dimethyl-5-*tertiary*-butyl benzene is still used in perfumery and can be prepared by nitrating *m-tertiary*-butyl-*m*-xylene (F. Ullmann, *Enzyklopädie*, 1931, **8**, 846). It is interesting that it is only this year (R. C. Fuson, J. Mills, T. G. Klose and M. S. Carpenter, *J. Org. Chem.*, 1947, **12**, 587) that musk ketone has been shown to be 4-*tertiary*-butyl-3:5-dinitro-2,6-dimethylacetophenone instead of 6-*tertiary*-butyl-3:5-dinitro-2,4-dimethylacetophenone, the structure assigned long ago by Baur. Musk ambrette is 1-methyl-2:6-dinitro-3-methoxy-4-*tertiary*-butyl benzene. In spite of their formidable odours, both indole and skatole are used to a limited extent in certain perfumes, and other β -substituted derivatives of indole do possess odours of potential value in perfumery (T. F. West, H. J. Strausz and D. H. R. Barton, *Synthetic Perfumes—their Chemistry and Preparation*, London, Edward Arnold & Co., in press). 6-Methylquinoline and its tetrahydroderivative and 7- and 8-methylquinolines also find a limited use.

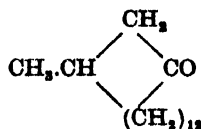
MISCELLANEOUS

*iso*Eugenol has a carnation odour and is obtained by isomerising the phenol eugenol (main constituent of oil of cloves) by fusion with potash. The compound is used in perfumery and as an intermediate in the preparation of vanillin from eugenol, so that optimum conditions for effecting the isomerisation have been closely studied. Coumarin, which possesses an odour recalling new-mown hay, is utilised widely in perfume mixtures and can be prepared from salicylaldehyde by the Perkin reaction (*e.g.* P. May, *Perf. and Essent. Oil Rec.*, 1925, **16**, 45), and 6-methyl coumarin obtainable from *p*-methylsalicylaldehyde (*e.g.* K. S. Chang and P. P. T. Sah, *J. Chinese Chem. Soc.*, 1936, **4**, 84) is also used to a limited extent. Diphenyl methane has a pleasant geranium odour and has a perfumery use. As it is a dyestuff intermediate many methods have been devised for synthesising the compound.

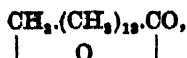
Civet, which has been used in perfumery for centuries, is found in a peculiar sac situated underneath the anus of the civet cat. This natural excretion is collected at nine-day intervals from both male and female animals specially bred for the purpose, the average yield being about 25 grams at a time. It is a yellowish to light brown paste with a characteristic odour that is due mainly to two compounds, viz. skatole and the ketone, civetone. Ruzicka and his collaborators (*Helv. Chim. Acta*, 1926, 9, 230; 1942, 25, 604) showed that civetone possessed the structure



and, although the "natural" ketone does not appear to have been synthesised, Hunsdiecker (*Ber.*, 1943, 76, 142) has recently synthesised a stereoisomer. 1-Muscoone (3-methyl-cyclopentadecanone)

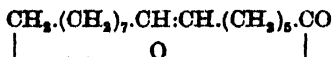


is the odorous principle of natural musk (the secretion obtained from the sac situated beneath the abdomen of the male musk deer), which has been used in perfumery from biblical times. The *dl*-form of the ketone has been prepared by synthetic routes by Ziegler and coworkers (*Annalen*, 1933, 504, 94; 1934, 512, 164), by Ruzicka and Stoll (*Helv. Chim. Acta*, 1934, 17, 1308) and more recently by Hunsdiecker (*Ber.*, 1942, 75, 1190). The ketone, cyclopentadecanone (Ruzicka *et al.*, *Helv. Chim. Acta*, 1926, 9, 249, 260; *B.P.* 235,540/1925) is used in perfumery under the name "exaltone" and the preparation of this compound has been studied in detail by Ziegler and coworkers (*Annalen*, 1933, 504, 122; *B.P.* 438,291/1934). Kerschbaum (*Ber.*, 1927, 60, 902) found that the lactone of 14-hydroxy-tetradecane-1-carboxylic acid,



possessed a characteristic musk odour and synthesised the compound (*G.P.* 449,217/1926). Ruzicka and Stoll devised alternative methods of preparation (*Helv. Chim. Acta*, 1928, 11, 1159; *B.P.* 294,602/1928) and called the lactone "exaltolide." At the same

time Kerschbaum showed that the musk odour of musk seed oil was due to the presence of the lactone ambrettolide



and described a synthetic method of preparation (*G.P.* 449,217/1926).

In this brief review an attempt has been made to survey some of the materials of the perfumer. The approach has been chemical, whereas the main preoccupation of the perfumer is presumably with the appropriate human senses and it is his artifice that effects the complex odour sensations induced by the finished perfume mixture.

Hast thou not learn'd me how
To make perfumes ? distil ? preserve ? yea, so
That our great king himself doth woo me oft
For my confections ?

(*Cymbeline*, Act I, Scene 5)

SOME APPLICATIONS OF ELECTRONICS IN METROLOGY

BY E. J. B. WILLEY, D.Sc.

FOLLOWING the excellent advice of the King of Hearts, we shall begin at the beginning and define metrology, according to the restricted sense wherein the term is generally used nowadays, as that branch of the art of measurement having most application in the engineering world. Although in the less progressive and far too numerous sections of British industry there has been undisguised joy at the demise of so much of the A.I.D., there has also been *some* recognition, even among the barbarians, of two facts that for years have been quite well known to what may be termed the Rolls-Royce type of organisation, *i.e.* that a good design is useless if not backed by sheer quality in materials and workmanship, and that the older technique of gauging and inspection has to be supplemented by devices giving detailed information as to local dimensions and other variations that averaging methods do not always reveal. All this reduces simply to the necessity for adopting precision metrology in fabrication and inspection departments alike, which, taken along with the requirement for speed of production, has led to some interesting developments wherein electronics is without doubt going to replace a good deal of the purely mechanical gadgetry that now inhabits the Gauge Room.

In work of the kind wherein we are here interested, the principal necessity is to measure quantities having the dimensions of length, more usually in comparison with a standard than in absolute value; that is, although we can deduce, say, the diameter of a mandrel if we know the amount Δl by which it deviates from a known reference value, l , what we are more concerned with is the value of Δl itself, with especial regard to the ratio $\Delta l/l$, or $\Delta l/l'$, where l' is the tolerance or the amount by which the finished article can be allowed to deviate from dimensions specified on the drawing. Instruments are, therefore, best designed to measure small changes relative to a reference value that must itself be changeable at will to provide for adaptability in use.

The dial-gauge comparator (or comparometer) is a typical instrument of this class. A spring-controlled plunger of hardened steel (sometimes tipped with tungsten carbide) engages with the specimen under examination, movement of this point being magnified by a gear train and read on a scale graduated, say, in ten-thousandths

of an inch. The scale itself can be rotated and the zero brought to coincide with the pointer, no matter where this may be, so that if the assembly is set up to read zero, with the reference standard in position, any new reading with the work will give the deviation directly. For relatively large changes this is quite satisfactory, but the mechanism is liable to show stickiness, and reproducibility of results is none too good when only small readings are concerned, since the effects of backlash, etc., are then so serious; about

± 0.00005 in. is the usual working limit, even with the best of these "clocks."

In a more recent device (P.G. Comparometer, Cooke Opticator, etc.), the moving plunger tilts a small mirror, which by an optical lever—sometimes using multiple reflexions—gives in effect a light path some 20 ft. long in a cylinder only 10 in. high, final readings being taken on a scale built into a low power eyepiece or projector head; this is much better than the mechanical arrangement alone, since there is in effect no mechanism, and with the average commercial unit results are easily reproducible to ± 50 micro-inches (Fig. 1). The same elimination of an internal gear train is secured in the

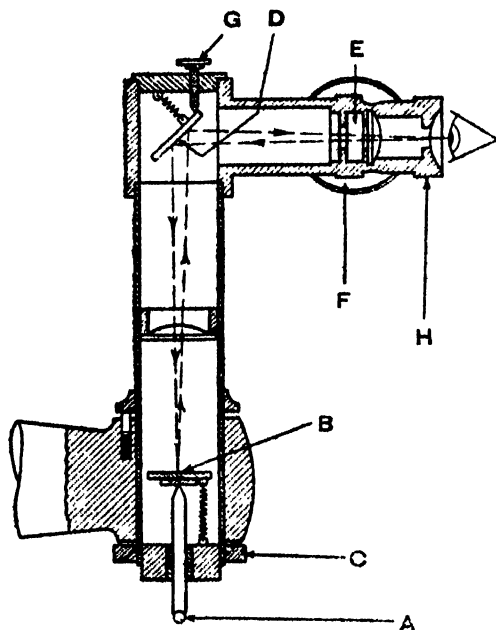


FIG. 1—Optical comparometer.
(Precision Gauges & Tools, Ltd.)

A = plunger; B = tilting mirror; C = retaining ring; D = zero adjustment controlled by screw G. E and F = half-silvered mirror and scale system; H = ocular.

(Reproduced from "Engineering Precision Measurements," by A. W. Judge, by kind permission of Messrs. Chapman & Hall, Ltd.)

Sheffield gauge by use of an ingenious system of differential reeds.

For measurement of angles, we can use a comparator in conjunction with a sine bar, or similar device, determining the height of a reference point above a surface, the hypotenuse of the resultant right-angled triangle being known. The Solex pneumatic gauges are especially useful for internal measurements, since they depend upon the pressure drop set up when air at constant pressure flows to atmosphere through the space between the surface under examina-

tion and a reference plunger by which the air enters. In this case, we can of course obtain only mean values for the element of volume over which the measurement is taken.

The possibility of extreme accuracy of measurement in length, and very conveniently in angles as well, together with other physical quantities such as dielectric constant, was first indicated, so far as modern electronic methods are concerned, by Whiddington (*Phil. Mag.*, 1920, 40, 634). By a well-known principle of wave theory, if frequencies f_1 and f_2 are applied to an appropriate system, a resultant will develop composed of the two applied frequencies, their sum and their difference. In electrical systems, filters can be devised to remove f_1 , f_2 and $(f_1 + f_2)$, only $(f_1 - f_2)$ being transmitted. This is, of course, the principle of the heterodyne receiver, beat frequency oscillator, and many other devices. Suppose now that we have a tuned circuit composed of fixed inductance L and variable capacity C , resonating at frequency $f = 1/2\pi\sqrt{LC}$ c/s. If C is a parallel equiplate condenser, then $C = A/4\pi x$, where A is the area of the plate and x the gap, so that $f = (x/\pi LA)^{1/2}$. Differentiating, $df/dx = 1/2(\pi LAx)^{1/2} = f/2x$. If we have two similar oscillators, tuned initially to, say, 1 megacycle, feeding into a mixing circuit where sensitivity is great enough to enable a beat frequency of 1 c./s. or less to be detected and determined, then, with $df = 1$, dx can be made as little as $5 \cdot 10^{-9}$ cm. Correspondingly, by using a rotary condenser with linear relation between capacity and angle of fixed relative to moving vanes, an extremely small change of rotation can be measured. Mounting the condenser so that its spindle can be rotated by a lever moving in a vertical plane, we have obviously a very sensitive angle-comparator. With later developments (see, e.g., Andrews, *Physics*, 1931, 1, 366, for use of this method in determination of the dielectric constants of gases), this "ultra-micrometer" has been made an excellent laboratory instrument, although experimental precautions in connection with shielding, prevention of "pulling" between fixed and variable frequency circuits when $(f_1 - f_2)$ is small, and stabilisation of power supplies are all severe. Very good accounts of the technique for such beat frequency circuits are given by Kirke (*Wireless Engineer*, 1927, 4, 67) and Gilbert (*Television and Short Wave World*, 1940, 13, 548; 1941, 14, 15).

Attempts have also been made to measure small changes of length by changes of resistance in carbon and certain ceramic materials when they are compressed (cf. McCullum and Peters, *Tech. Paper Bur. Standards*, No. 247 (1924); Bleakney, *Bur. Stand. J. Res. Wash.*, 1937, 18, 723). A bridge circuit is used, but out-of-

balance e.m.f. is not always linear with load, while temperature coefficients and difficulties in securing good contacts make experimental difficulties very troublesome, so that the method is not really practical. The piezoelectric effect, using crystals such as Rochelle salt that develop relatively large e.m.f.s, has, however, been used with success. The spring-loaded plunger of the usually mechanically-operated comparator head is applied to a cartridge composed of up to six crystals in series, the e.m.f. being passed to a multistage amplifier using electrometer triodes and an exceptionally well-stabilised power unit. Use of a larger number of crystals defeats its own purpose, since self-capacity is then so large that the e.m.f. at output is actually less than with a smaller bank giving *per se*

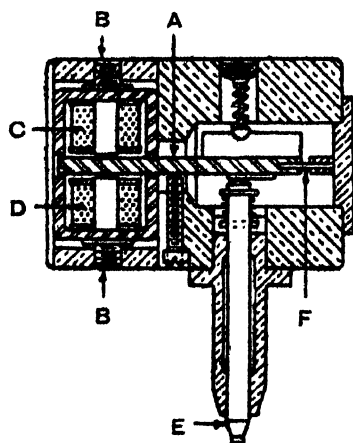


FIG. 2.—Electrolimit gauge head.

A = armature; B = adjusting screws for coils C and D; E = plunger; F = hinge.

(Reproduced from "Engineering Precision Measurements," by A. W. Judge, by kind permission of Messrs. Chapman & Hall, Ltd.)

is very useful indeed. The working head contains an a.c. bridge of four coils wired in two pairs, between which lies a steel armature connected to the usual plunger for exploration of the work (Fig. 2). Coils, etc., are so dimensioned that the bridge is balanced when this armature is at some point about midway between the two coils, any displacement giving rise to an out-of-balance e.m.f. that is rectified and passed to a moving coil microammeter—with scale calibrated in microinches—and the usual range shunts. Since all coils are equally affected, no special precautions are necessary in regard to power supplies, and, even without amplification, displacements of one microinch can be measured quite easily if the coil assembly is well designed. The working head is mounted on a stand

a smaller signal. The output meter can be calibrated directly in microinches; this is the principle of the Brush surface analyser, to which reference will be made later on. In the writer's experience, piezoelectric apparatus is better for use in the laboratory rather than in the tool-room, even when the more robust quartz crystals are used in place of Rochelle salt; the trouble is, of course, the very small signal e.m.f. for any reasonable loading. (Kluge and Linckh, *Z. Ver. deutsch. Ing.*, 1929, 73, 1311; 1930, 74, 887; Sigrist and Meyer, *Helv. Phys. Acta*, 1936, 9, 646).

The Electrolimit type of gauge (Messrs. Taylor, Taylor & Hobson)

and used with slips according to standard technique. In practice, the device is very good tempered and rarely gives trouble.

The change of capacity in a condenser has also been used somewhat differently by comparison with the ultramicrometer. Suppose a tuned circuit, made up of condenser and inductance as before, to be coupled inductively to a local oscillator. The curve of induced e.m.f. against condenser capacity will be of the general form of an inverted V with somewhat flat top, so that if we make our condenser in two parts, one in association with the variable to be studied, we can use the main tuning control to set the circuit to some reference point at the middle of the straight part of one of the "legs" of this resonance curve, and then run up and down the curve in accordance with changes of capacity in the other part. It may quite simply be shown (*cf.* Willey, *J. Sci. Instrum.*, 1946, **23**, 264) that with a parallel plate condenser the change of capacity dC can be made linear with change of gap dx over a considerable range, provided that x itself is large. By an obvious adaptation, any parallel plate condenser or the like can be made into a device for measurement of any displacement as required in the work here concerned (Cornelius' gauge). Sensitivity can be as high as with the heterodyne ultramicrometer, the determining factors here being the sharpness of tuning and the number of stages of amplification used. With a steeply-sided resonance curve, any given small change of capacity will clearly lead to a much larger change of induced e.m.f. than will the same capacity increment with a flatter curve; there is, however, a limit beyond which the circuit becomes unstable. The amplifier is, of course, the analogue of the gear train in the mechanical recorder.

With this arrangement, frequently termed the capacity micro-meter, there is, however, another most useful property, *i.e.* ability to respond to, and act as recorder for, high-speed phenomena. Suppose (i) the polarising frequency to be high by comparison with that of any variable load, such as a fluctuating pressure, applied to the plates of the condenser, and (ii) the tuned circuit to be set to some reference point on the resonance curve where a linear relation exists between dC and dx . On application of this load, we shall then bring about amplitude modulation of the induced e.m.f., and, by rectification and further amplification as desired, obtain an output proportional to pressure at any instant. Applying this to a cathode ray oscillograph, using a linear time base, we draw out a graph of pressure against time, while if we are, for example, taking the pressure load from a reciprocating machine, we can use as x -deflection another voltage reproducing movement of the piston, the resultant envelope being an indicator diagram. A full account of this tech-

nique has recently been given by the present author (*loc. cit.*). The Electrolimit type arrangement can similarly be used in the same way. The steel diaphragm can, of course, be used as in a telephone, inducing in a neighbouring coil an e.m.f. proportional to rate of change of load. To convert this into one of load against time, it must be integrated, which can be done to quite a good order of accuracy in a resistance-capacity network, followed by at least a two-stage amplifier (with ample negative feedback) to compensate for the severe attenuation entailed. Considered as a means of obtaining an indicator diagram, this pressure capacitor is greatly superior to the older mechanical devices. It can be made very small, be placed in any position, involves no complicated lever movements or other devices for magnifying displacement of the diaphragm, has no hysteresis worth mentioning, and is quite free

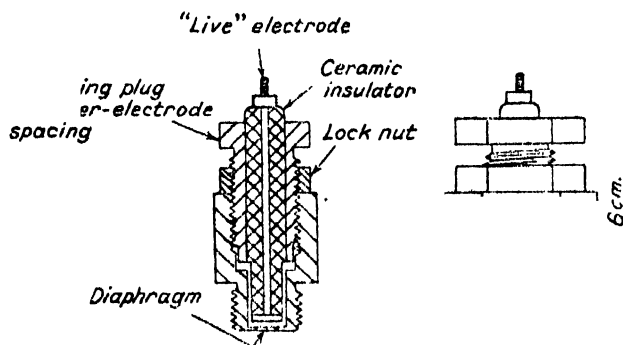


FIG. 3.—Capacity micrometer as used for pressure measurements.

(Reproduced by permission from "J. Sci. Instrum.," 1946, 23, 264.)

from liability to resonances up to the natural frequency of the diaphragm, which can of course be calculated quite easily. A typical unit is shown in Fig. 3. The electrical circuit consists simply of a local oscillator, a rectifier stage, and amplifier designed to cope with the very low frequencies likely to be encountered.

The Talysurf meter (Messrs. Taylor, Taylor & Hobson) is a precision instrument for exploration of surfaces, rendered necessary by the increasing emphasis upon quality control in connection with aero engines and the like where extreme accuracy of fits, bedding between surfaces, etc., is essential. Whatever the merits of the various arguments as to slightly roughened surfaces holding their lubricants better than those of optimum smoothness—a perennial controversy among engineers—it is fairly generally conceded that consistency as to texture is most desirable. In any case, it is obvious

that if, for example, we can manufacture an engine with the same surface texture characteristics in cylinders, piston rings, gudgeon pins, big ends, etc., as obtain after running in, an important time advantage is obtained.

A diamond stylus of point radius 0.0001 in. is caused to traverse the surface under investigation at constant speed, the up and down movements of this point being amplified electrically in a magnetic balance arrangement of the Electrolimit variety, the output being switched at will to an integrating circuit, so that the average height of surface indentations can be measured as well as the actual profile, which is generally traced out on a recorder. The load on the surface being only 0.1 gm., there is clearly no danger of obtaining spurious readings due to pressure by the stylus itself; soft materials can, therefore, be handled with assurance. Standard vertical amplification is 40,000, but can, of course, be varied as desired by a selector switch. With the normal arrangement of sliding surfaces, the assembly carrying the measuring point would itself be moving over a bed having its own quota of irregularities, so that the resultant profile would in effect be a composition of that due to the instrument and that due to the surface under examination. This is very neatly obviated by mounting the stylus, traversing gear and magnetic head upon a sprung cradle so that in the actual instrument there is no sliding or rotation of one surface over another. A simple analogue is the usual laboratory "shaker," where the tray holding the solutions is supported by four vertical members of spring steel, so that it can be pulled to and fro by a string connected to a pin on the periphery of a motor-driven wheel. Horizontal movement is thus obtained without bearing surfaces, simply through flexure of the supports. All vertical movement of the stylus is due in consequence to the test surface and to nothing else. Although it was possible with simpler arrangements, such as the Tomlinson surface analyser—using movement of a stylus to trace out upon a smoked glass screen a pattern that could then be magnified by optical projection—to obtain a good deal of information as to the actual physical results of the various surface finishing processes, these more recent devices have greatly extended knowledge in this direction.

In their treatise on surface measurement, the staff of Messrs. Taylor, Taylor & Hobson (Reason *et al.*, 1944) point out (p. 12) that the texture of a machined surface can frequently be resolved into wave-forms representing :

- (1) General curvature of the whole surface, or irregularity of long wavelength, due perhaps to flexure of the work in the machine, or to lack of straightness in the ways.

- (2) Surface texture of medium wavelength, due to bad conditions or bad setting up of the machine ; vibration, untruth, and bad squaring on of the cutter are common causes.
- (3) Surface texture of comparatively short wavelength, due to tool-cutting action during the machining process in a rigid and well-set-up unit, and roughly divisible into two sub-classes according to whether it may or may not be explored by a stylus of radius 0.0001 in.

Species (1) and (2) not infrequently combine, with additional inaccuracy due to resonances set up between work and tool or grind-

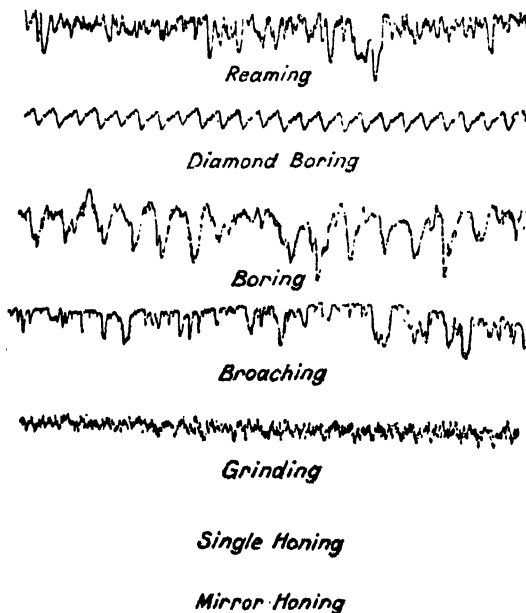


FIG. 4.—Surface contours due to various finishes (Schlesinger, *Machinery*, Mar. 28, 1940). Magnification, horizontal $\times 30$, vertical $\times 1000$.

(Reproduced by kind permission of the Institution of Production Engineers.)

ing wheel, bad errors developing at nodal or antinodal points where amplitude of vibrations can be excessive. These effects may be one of the reasons for anomalies such as constant-diameter figures, which when measured by an ordinary two-point contact, as with a micrometer, appear to be circular, but are in reality quite otherwise (cf. Shaw & Shaw, *Machinery*, 1932, 40, 777).

Readings taken with this instrument or its equivalent are highly illuminating as to the exact nature of the finishes produced by various means (Fig. 4 ; cf. Schlesinger, *Machinery*, March 28, 1940), and

considerable entertainment can often be derived from observation of their impact upon the mixture of prejudice and sheer folklore that sometimes replaces precise knowledge in engineering practice. The Brush surface analyser, used in America, is designed for the same class of measurement as the Talysurf gauge, but uses a piezo-electric pressure head in place of the magnetic bridge of the British instrument (*cf.* Graveley, *Electronics*, Nov. 1942, p. 70).

Studies upon vibration, acceleration, etc., are also made by electronic devices. A representative example of instruments used herein is described by Stansfield (*Proc. Inst. Mech. Eng.*, 1943, **148**, 175) in connection with the recording of torsional vibrations in shafts and the like. A bipolar armature is wound with a coil of wire to be integral, and thus to oscillate with the shaft under examination. Floating on the spindle of this armature is an inertia mass containing a permanent magnet. Above a certain critical speed, this mass rotates at constant angular velocity, so that oscillations of the armature shaft, reproducing those of the engine shaft, cause the windings of the coil to cut the magnetic field, giving a voltage directly proportional to the angular velocity of the shaft vibrations. This primary signal is then amplified and fed to a cathode ray oscillograph. Provided that there is at hand some means to hold the body steady, so that the only movements recorded are those due to travel of a stylus in contact with the work under examination, the versatile capacity micrometer can also be used in this, as in other measurements; but provision of the rigid base is very difficult, so that experimental troubles are formidable. (*Cf.* Frommer, *Electronics*, July 1943, p. 105, for description of a capacity micrometer used in physiological work.)

Measurements upon strains at all frequencies up to c. 1000 c./s. can be made quite well with the special resistance elements developed, for example, by Messrs. Tinsley (*cf.* Aughtie, *Proc. Inst. Mech. Eng.*, 1945, **152**, 213; Curtis, *Electronics*, July 1945, p. 114). The working part consists of a grid of fine nichrome or modified eureka wire, fixed to a paper backing with non-conducting cement, and provided with ends that are specially reinforced and spot-welded to eureka connecting pieces. The unit is attached by an adhesive to the region where strains are to be measured, and lightly covered with protective felt to keep temperature as constant as is reasonably possible, any variations of resistance due to fluctuations herein being in consequence very slow. The relations between strain and change of resistance being known from calibrations, it is only necessary to incorporate the unit in a sensitive bridge circuit, using valve amplification where strains are small, and feeding to an

oscillograph where short-period phenomena are under examination. As measured, the strains are longitudinal, corresponding to a change in length of the wire, but, by appropriate siting of the elements, torsional strains, etc., can be measured as well. Such equipment is an absolute essential to the aircraft designer, and, since the resistance elements themselves can be made so small, a considerable number of measurements can be made simultaneously at different places and results made visible by means of an oscillograph and electronic switch circuit (*cf.* Russell, *Electronic Engineering*, Dec. 1942; Moerman, *Electronics*, Aug. 1945, p. 112).

The most recent development in the use of electronic appliances in metrology has been through introduction of ultrasonics. For measurement of thickness, for example, the test material is set into vibration in the thickness direction by a variable-frequency ultrasonic signal, a quartz crystal in contact with the surface being itself made to vibrate at the resonance points and generate an e.m.f. that is amplified and fed to an output meter. Since these resonance frequencies are very sharply defined, accuracy in their location can be correspondingly high and in one representative commercial instrument the maximum error of measurement—which in practice is rarely obtained—does not exceed 3 per cent. over a range of thicknesses 0.02 to 0.12 in. (Messrs. Dawe Instrument Co.).

The ultrasonic flaw detector (Messrs. Henry Hughes) works somewhat differently. A narrow pulse of ultrasonic radiation is sent out by a crystal circuit, traversing the test medium and being reflected from the other boundary back to a receiver crystal, mounted integrally with the test head, in a manner dependent upon the length of path and the uniformity or otherwise of the intervening material. Fractions of both pulses are amplified and applied to a cathode ray oscillograph, outgoing and returning signals being recognised as "blips" at each end of the trace, with inhomogeneities showing as small peaks between them. An approximate spatial location of flaws is thus possible, and the instrument has many uses in general testing work, although there cannot be any high accuracy in measurement. A certain apprenticeship is also desirable as regards interpretation of results.

We may conclude by reference to a device that has interesting potentialities apart from the purpose for which it was originally designed, *i.e.* the photoelectric polarimeter (*J. Sci. Instrum.*, 1943, 20, 74). In this, the usual nicols are replaced by "Polaroid," permitting the use of large apertures and very much more light than is usually possible. Intensity of transmission is measured upon a cell of the barrier-layer type (Messrs. Evans Electroselenium,

et al.)—which gives its own e.m.f. without any polarising battery—and therefore non-subjectively, to any desired order of accuracy. Since intensity I is proportional to $\cos^2 \theta$, where θ is the angle between axes of polariser and analyser, the relation between I and θ will be a periodic curve displaced from the zero position by an amount equal to the rotation induced by any optically active material placed in the path of the light. To measure this rotation, we have then to find any two angles, one on each side of the zero, where equal photocurrents are obtained, take their mean, and deduct 90° plus any zero error there may be, repeating the observation at as many points as may be desired. Bringing the Polaroid discs into closer apposition, and setting them so that their axes are inclined at *c.* 45° , where $dI/d\theta$ is greatest, we have a very sensitive angle comparator, and the writer has found that, with a good scale and vernier, the device is equal in veracity to a good sine bar.

With further developments in the art of electronics, many more instruments will no doubt appear ; only a few of the most interesting have been described here. Whether there will be any solution to some of the perennial problems of metrology remains to be seen ; we still don't really know how to measure up a hole !

FIFTY YEARS OF THE INERT GASES

By M. SCHOFIELD, M.A., F.R.I.C.

Lecturer in Chemistry at the Wolverhampton and Staffordshire Technical College

WHEN Ramsay and Travers just half a century ago succeeded in conjuring forth neon, krypton and xenon, the tale of the discovery of the inert gases was completed while that of possible applications in the laboratory and the arts was begun. Chemists and physicists would be hard put to it to find a more absorbing series of incidents than those concerned with the capture of these drones among the elements. As we turn back the pages of chemical history, there is no surprise that such inert gases had literally been under the noses of enquiring scientists through the centuries. It was simply that these drones, as with the human types today, remained well hidden, since there were no means of identification. A hint from Cavendish, of course, should have sent forth the hue and cry. That classic experiment of a savant deemed somewhat eccentric had first exposed a bubble of the gases remaining after Cavendish had passed sparks through air plus excess oxygen; removed the nitrous oxides with potash and oxygen with liver of sulphur; and then noted the magic bubble—"certainly not more than one hundred-and-twentieth of the bulk of the phlogisticated air let up into the tube" as he remarked, but a hundred-and-twentieth which should have attracted someone within a whole century to try and find a reason for it.

It was left, then, to Rayleigh to open up that chapter in the history of science which gave us the inert gases, or "noble gases," as the Americans term them. His work was remarkable in that on his Terling estate this gifted amateur could "make do" with any odd pieces of apparatus. In rising to eminence as physicist Rayleigh could adapt the wires of an old piano, odd jars and bottles from pantries, glass tubing and sealing wax and so on for his fruitful studies of sound vibrations. And so, too, when he came to investigate the reason for slight differences in the density of atmospheric nitrogen and nitrogen from other sources, when he came to repeat the Cavendish experiment on a more ambitious scale, he was not put off when his Cambridge tutor would never tell him where his apparatus could be purchased ("the man he bought it from was dead"). As laboratory, Rayleigh used an old brewhouse at Terling. He borrowed his 3 h.p. gas-engine, normally used to work a cream separator, and contained his caustic soda, required for absorbing oxides of nitrogen, in odd dairy vessels. In puzzling over the slightly denser nitrogen from air—after inviting explanation for this

in the columns of *Nature* and getting none—Rayleigh visualised the possible causes. His nitrogen from atmospheric sources might still contain oxygen, or even some N_3 molecules analogous to ozone; the nitrogen from ammonia decomposition might be lighter because of a little hydrogen left in it; or some dissociation of nitrogen molecules may have taken place.

The critic in Rayleigh's early career who attributed "Mr. Strutt's success to his perseverance rather than to his brilliant talent" was certainly on the mark with the first attribute. Our chemical histories make much of the eccentric yet fruitful career of Cavendish working in his London mansion converted to laboratory. But how often they turn from this to Ramsay's work, how rarely if ever do they tell of that remarkable intermediate phase in which a first-rate physicist, a man who had read Odling's *Chemistry* as well as Paley's *Evidences*, Butler's *Sermons*, and Todhunter's classics while at Trinity, made one venture into Chemistry House and won success before returning to his own sphere—"because the second-rate men know their places much better," as he somewhat surprisingly put it. Rayleigh's striking experiments at Terling proved, moreover, a connecting-link between nitrogen fixation by Cavendish's rough experiment in merely sparking air and the now obsolete large-scale industrial operation carried on at Notodden, in which a widely-spread arc was used. He used his brewhouse for the crucial experiment with the 50-litre glass globe, a transformer discharge of 6000–8000 volts, and a fountain of caustic soda spraying over the inner surface, and, since he knew the vigil would be a long one, he had a telephone connecting to the library while he dozed, to assure him that his transformer was still functioning. The result was a thimbleful of a new gas which Rayleigh first intended to call "aeron" to denote its derivation. But tiring of the sceptics with their facetious query as to "When may Moses be expected?", he decided to name it "argon," the idle or lazy gas, a name suggested from his reading in a Greek testament how "Some of the labourers from the vineyard stood *idle* in the market place."

Ramsay became co-discoverer of argon, since he asked Rayleigh's permission to experiment on the problem of nitrogen from different sources, and proceeded to "fix" his nitrogen differently, that is, by combining with magnesium. The two collaborators working independently had "no suspicion or sense of injustice." Ramsay was the first to note the group of red and green lines in the spectrum, lines which banished any idea of a modification of nitrogen being present. In a letter to Rayleigh he also showed he suspected the presence of other inert elements: "Has it ever occurred to you that

there is room for gaseous elements at the end of the first column of the Periodic Table ? ” And it was Ramsay, too, who in his address to the British Association just fifty years ago expressed the belief that other inert gases remained to be discovered. (Lecoq de Boisbaudran, after hearing of argon, had hinted at the possibility of a “ family ” of inert elements in a paper in *Compte rendu* in 1895. But then, so had Ramsay a year previously suggested to Rayleigh the term “ Q or Quid ? ” since “ there are two other X’s.”)

The identification of helium as a constituent of the atmosphere and not solely an element of the sun, as indicated by Janssen and Lockyer with their spectroscopes, came from a number of observations. Hillebrand, eminent American chemist and mineralogist, had noted how uraninite by the action of a dilute acid evolved an inert gas considered at that time to be nitrogen. But Ramsay was not satisfied. He took the uranium mineral cleveite, named after Theodor Cleve, added an acid, found some nitrogen and argon to be evolved, but also another inert gas proved different by lines in the spectrum. So he sent a specimen to Lockyer and Crookes, and was told how “ a glorious yellow effulgence . . . a sight to see ” was the result of a high-tension discharge. “ I suppose it is the sought-for krypton, which should accompany argon ” was his first reaction. But it proved to be helium, as Ramsay delightedly telegraphed Berthelot : “ Gas obtained by me cleveite mixture of argon and helium. Crookes identifies spectrum. Communicate Academy Monday—Ramsay.” And yet Cleve and his associate Langlet must be credited for the independent bringing of helium to earth, for in Sweden they also had obtained a rather purer specimen of helium from cleveite.

From this point there followed a veritable orgy of heating rare minerals in the hunt for the remaining members of the group of inert gases. Minerals were found to evolve carbon monoxide, carbon dioxide, hydrogen, helium, and, in the case of malacone, helium and argon. Mineral waters also were tried, but no new spectral lines were forthcoming. (Mineral waters were a likely source : apart from helium there are records of nitrogen and argon at Buxton, nitrogen, argon, and traces of helium and neon at Bath, argon and helium in the effervescent Canterets waters, and up to 1.14 per cent. argon in gases from Icelandic springs.) Success came to Ramsay and Travers when they turned to the possibility of separating fractions of some other gas from crude argon. Argon had been liquefied in 1895, and Hampson provided a litre of liquid air for Ramsay and Travers to practise the technique of diffusion before risking their precious 15 litres of gaseous argon. From the residue

left after evaporation of most of the liquid air, final traces of oxygen were removed with hot copper, nitrogen with magnesium as before, and the remaining gas examined spectroscopically within a Plücker tube. Thus was krypton identified fifty years ago by its green line. The hunt was by now in full cry, with Travers so excited that he almost forgot the day of his examination for the doctorate degree. Since krypton was a denser gas than the one expected from the more volatile fraction from argon, further search using a new weapon—liquid air boiling under reduced pressure for liquefying argon—revealed the expected lighter fraction with a “magnificent spectrum with many lines in the red, a number of faint green ones, and some in the violet.” Neon was thus identified in June 1898. Finally, with the aid of a liquefaction machine from the Brin Oxygen Company, Ramsay and Travers fractionally distilled krypton to yield xenon, the stranger, with its striking blue glow in the Plücker tube.

The harvest from all this endeavour half a century ago shows how that bubble of gas, which Cavendish first saw, proved indeed a magic bubble. Five new members, taking their places at the chemist's Table, attracted considerable interest by virtue of their complete inactivity, by demonstrating in a practical way the kind of properties forecasted for elements with their outer shells satisfied with electrons, elements bridging the gap between the strong electro-positive members of the first group of the Table and the strong negative elements of Group Seven. The physicist is happy, too, to welcome these new members because of their value in low-temperature research, in studies of emission spectra, and especially in discharge lamps. On the technical scale such discharge lamps for internal and external illumination, together with the use of argon in gas-filled filament lamps, provide the final touch, the “surprise ending” which fact as well as fiction so often introduces. “To everything its use.” Those “rare” gases well illustrated the old saying, in their provision of the garish advertisements which glowed in our cities and towns by night, literal signs that “progress” has gone far since the time of Cavendish. Before a Ministry of Fuel introduced a new law of conservation of energy by placing a veto on neon signs, one saw this new industry in full swing: the skilled manipulators working S-bends and intricate patterns on glass tubing often of wide bore, then fusing on side branches and fixing electrodes; next the complete evacuation of each shaped tube by “bombardment” with high-tension discharge during pumping; then the introduction of a few millimetres pressure of neon for common orange-red, beckoning from every tavern-door; of helium within yellow glass; of mercury vapour with neon or argon for blues, and other combinations designed to meet the urge for chromatic display.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By A. HUNTER, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

THE 200-IN. TELESCOPE.—The first World War did not interrupt the completion of the 100-in. reflecting telescope, the mirror of which was installed in its mounting on Mt. Wilson in 1917. War evolves more rapidly than astronomy, however, and the magnitude of the effort required from the U.S. when she entered the second global conflict made it necessary to postpone *sine die* plans for finishing the grinding and polishing of the 200-in. mirror, then nearly completed in the Pasadena workshops. The dome and mounting, already finished and waiting on Mt. Palomar, lay idle for nearly four years whilst the technicians turned to optical work of more immediate importance, and only after the capitulation of Japan were operations resumed on the mirror.

In his 1947 Morrison lecture (*Publ. Astr. Soc. Pacific*, 59, 153, 1947) Edwin Hubble reports that in April 1947, departures of the new mirror from the paraboloid had been reduced to about a quarter of a wave-length of light. When the residual errors are reduced to a tenth wave-length, figuring will be stopped, and the mirror will be aluminised and installed in the mounting on Mt. Palomar ready for the final tests on star-fields under working conditions. "We like to believe," says Hubble, "that some research may be started during the early months of 1948," and he goes on to outline some typical problems that are scheduled for attack with the new instrument.

The waiting-list of conceivable programmes is so long that use of the telescope will be limited to problems utilising its aperture to the full, either for light-grasp, for resolution, or for both—problems which cannot be effectively attacked with existing equipment. Since the completion of the 100-in. telescope, four large reflectors of apertures 60 in. to 84 in. have been erected, another 100-in. is planned for Great Britain and a 120-in. for the Lick Observatory in California. Observing time with the 200-in. will be so much in demand that it will not be allowed to compete with these. Hubble selects for discussion three typical problems, each representing a particular direction in which the new telescope's great aperture will make it uniquely suitable: first, the existence of Martian "canals"; second, the relative abundance of the elements in stars; and third, the structure of the universe.

Hubble's first topic, one suspects, looms far less important in his mind than the other two, but the completion of the 200-in. telescope does afford a chance of settling, once for all, the Martian controversy, which has erupted at varying intervals over the past three-quarters of a century. The facts are these. Schiaparelli was the first to observe on Mars, in addition to the large, easily recognised markings on the planet's surface, some fine dark lines, at the limit of visibility, which seemed to follow great circles, which sometimes appeared double, which seemed to show seasonal variations, and which crossed one another at dark spots since described by Lowell's suggestive (perhaps too suggestive) name "oases." Since Schiaparelli's time the existence of these "canals" has been vehemently denied by other first-class visual observers, and as strenuously reaffirmed by still others, no less experienced. Interpretation of the observations lies on the borderline between psychology and physiology, and attempts have been made to resolve the conflict by investigating experimentally the mechanism of vision at low illuminations and at the limit of visual acuity. To the extent to which these experiments failed to reproduce the rather specialised conditions at the telescope, their results are inconclusive. From the other direction, the problem has been attacked by photography. Here it can only be admitted that with present apertures photography cannot furnish an objective test of the reality of the canals.

The human eye, peering as it does through the ill-mixed turbid sea that constitutes our terrestrial atmosphere, can nevertheless follow the gyrations and distortions of a planetary image and, rejecting those many moments when surface detail is lost, can transmit to the consciousness records only from those few instants when seeing is momentarily steady. The facility is one which is possessed in some small measure even by the inexperienced eye: that it can be immeasurably improved by practice is affirmed by all assiduous visual observers. But the photographic plate, though building up, in a way no eye can do, an image on the emulsion, must perforce do so soullessly, obliterating the fine detail available during the steadiest moments with images of coarser structure which are the rule even on nights of good general definition. The limiting factor is simply light intensity. The best visual work has been done with telescopes of quite moderate apertures—10 or 20 in. At the prime focus of (say) a 20-in. reflector working at $f/5$, the image of Mars will never, even at the most favourable oppositions, reach a diameter of one-eightieth of an inch. Even at the Cassegrain focus of the 100-in. telescope it is always less than a fifth of an inch across. To utilise the optical resolving power of the telescope to the full,

an enlarging lens must be used, otherwise the photographic grain size will be too big to record the fine detail required. The image, already rendered faint by this enlargement, must then be photographed through a red filter so as to use light which best penetrates the Martian atmosphere. As a result, exposures of some seconds are usually required even with big telescopes, and atmospheric shimmer smears out much of the detail observed visually.

Perhaps the best celestial photographs in existence are those obtained by Ritchey, who took tremendous pains in guiding with a plate-holder movable in two directions at right angles by hand-operated screws and in exposing (by means of a shutter operated by mouth) only at moments of exceptional definition. His results have certainly never been surpassed—perhaps never even equalled—since. Yet even so, with the apertures available to him no definitive answer to the Martian problem could be given by photography. But with 200 in. the problem may be solved—perhaps, as Hubble suggests, by cinematography, on the principle that a series of short exposures will include a few frames that catch those moments of good seeing which the eye selects instinctively; perhaps, in these days of electronic devices, by photoelectric guidance of a light plate-holder, if this can achieve sufficiently speedy and sensitive following of a rapidly wandering image. Hubble is clearly convinced that present equipment is nearly fast enough to produce photographs capable of showing the canals if they exist, for he claims “with some confidence” that the 200-in. telescope may settle the long-standing controversy.

The second matter he selects as typical of the problems the new telescope will be set concerns the distribution of the chemical elements in different stars. Adequate spectroscopic dispersion for detailed measurement of relative abundances is as yet obtainable only with the sun, which is bright enough for its visible spectrum to be spread out over about 50 ft. With three or four of the brightest stars the highest attainable dispersion is less than this by a factor of over 10, which does not really leave enough for the needs of theoretical astrophysics. For a multitude of fainter stars, including many of more intrinsic interest than those that are apparently brightest, little more than an indication of abundances can be obtained with present instrumental equipment. The data of immediate interest concern the relative abundance of hydrogen in stars of different spectral type and luminosity class, which are presumably at different stages in their evolutionary history. At present the theorist is faced with an embarrassingly wide choice of hypotheses regarding hydrogen content, all of which can be said to be con-

sistent, or at least not inconsistent, with present observations, but which entail widely different cosmogonic consequences. Only higher dispersion can give him the observational discriminants he wants.

As to the other elements, the basic hypothesis up to the present has assumed a constant composition from star to star, observed spectral differences being attributed to different physical conditions, such as surface temperature and gravity. With minor exceptions, exemplified by the two divergent classes of Wolf-Rayet stars exhibiting spectra dominated by carbon and nitrogen respectively, this hypothesis has held the field with fair success, but its implications in cosmogony are profound, and only high-dispersion spectra of widely different stellar objects can verify it. Theoretical deductions regarding the genesis of the heavy elements are already being made, on observational grounds that are less than adequate; and they must remain speculative until higher dispersion can be obtained in stellar spectra. The results the 200-in. telescope may bring to light here are likely to affect greatly our present views not only on the origin of the elements but also on the source of stellar energy in different classes of star and on the past history and future evolution of the universe.

Hubble's third typical problem is peculiarly his own: one whose solution will react as deeply on cosmology as his second does on cosmogony. The effective range of the 100-in. telescope, as defined by the faintest extra-galactic nebulae it will photograph, is about 2×10^8 parsecs. The new telescope, in doubling this, will make a sample of space eight times as large available to observation. This is in itself a desirable thing, for we shall be the less apprehensive that our corner of space may not be a fair sample of the whole. But the matter goes deeper than this. It will be of the greatest interest to find whether the observed large-scale homogeneity in distribution of the 10^8 galaxies now known extends also to the greater number detectable with the 200-in. telescope; and of even more interest to discover whether the linear relation between red-shift and distance holds as far out as twice the present limit. Results obtained by the 100-in. telescope have already put out of court some earlier cosmologies which were inconsistent with uniform nebular distribution or with linear red-shifts; but we are left with several theories, each with first-order effects consistent with the observations. These latter are of insufficient extent and are insufficiently precise to distinguish between the rival theories. The hope is that the 200-in. telescope will be able to sort them out by their second-order effects, which are expected to differ appreciably at larger distances. Hubble has already, indeed, suspected the existence of

such second-order effects in sample surveys made at Mt. Wilson. Counts to successive apparent-magnitude limits seem to indicate the presence of a general radial density gradient which corresponds to an increase in density of rather less than 20 per cent. in 10^6 parsecs. But photometric work at apparent magnitudes of 18 or so is so difficult and so liable to systematic error that mere indications are not enough to establish the presence of such an effect. It is early yet, then, to deduce from present observations a strong positive spatial curvature and a small, dense universe; or to react against this and abandon the velocity interpretation of the red-shifts. These ambiguities have arisen because the present observational material is being over-pressed: they will not, it is hoped, long survive the inauguration of the new telescope. The 200-in. observers should shortly be able to state whether in Shapley's Metagalaxy the red-shift is due to recessional velocity or to Hubble's "new principle of nature." The theorists will then have something to bite on.

Altogether, the first ten years of operation of the 200-in. telescope look like being as epoch-making as the first ten years of the 100-in. To appreciate the full significance of that it is enough to remember that when the latter was erected it could still be maintained that the sun was liquid, that the spiral nebulae were near-by galactic objects, and that the solar system was central in a Galaxy only 10,000 parsecs across. Perhaps in 1958 we shall regard some of our 1948 ideas with the same rather surprised amusement with which we now look back at these.

DAYLIGHT METEORS.—An astronomical discovery is in these days usually the hard-won result of painstaking measurements undertaken over periods of years. In this category must be placed, for example, the war-time discovery of non-solar planetary systems associated with two of the nearest stars. That even the sun's planetary system is still not fully explored, however, was demonstrated as recently as 1938, when Nicholson discovered two new satellites of Jupiter. These bodies could hardly be described as major features of the solar system, though, and one might have been excused for assuming that Herschel's discovery of Uranus a century and a half ago marked the last unexpected find of any importance in our immediate cosmic neighbourhood. But this view reckons without the development of astronomical radar recently described in these pages (*SCIENCE PROGRESS*, 35, 77, Jan. 1947). Keats's watcher of the skies now has a more powerful probe at his command.

B.A.A. Circular No. 282 (1947) announces that the Manchester

team of radio physicists, led by Dr. A. C. B. Lovell, has recently discovered a major meteor shower hitherto unknown because it occurs in daylight. In common with the Operational Research Group of the Ministry of Supply, the Manchester physicists have been studying for some months the ionospheric ionisation caused by meteors, and particularly by meteor showers. A new method (as yet unpublished), due to J. A. Clegg, enables accurate radiants to be determined even with the low angular resolution which is a necessary feature of the technique (the beam used is 12° wide). Throughout last autumn and winter the method has been tested on meteor streams which have long been observed by visual means. The results obtained, though interesting, have not been unexpected: indeed, the fact that they could have been predicted with fair accuracy beforehand has been a welcome indication that all was well with the new technique. The summer of 1947, however, brought a new feature to light. Investigation of the radiant near η Aquarii showed that the visible shower was only the beginning of a much wider stream stretching towards the sun, with a main radiant in Pisces only about an hour and a half in advance of the sun. Visual observations of a radiant so close to the sun are of course impracticable, and the new stream, which has turned out to be of unprecedented extent and duration, would have remained undetected but for radar. The intensity is very high: the hourly rate at meridian passage remained at over 20 for weeks, a value comparable with the peak rate observed for the Geminids with this apparatus.

The first-fruits of this application of radar to astronomy have thus turned out to be unexpectedly rich. Refinements already made in the technique make the method a close rival to visual observation in sensitivity, and if its angular resolving power cannot yet compete with that of the human eye, it has the compensating advantage that it is unaffected by daylight and by cloud. With its speedy development and extended application we may look for rapid advances in our knowledge of meteor streams and their connection with cometary orbits.

TIME FROM THE TRANSIT CIRCLE.—The introduction of the quartz crystal clock in place of precision pendulum clocks in observatories concerned with maintaining a public time service has been due largely to the demand by radio engineers for time signals capable of giving accurate intervals of 24 hours for control of radio frequency-standards. It has also incidentally thrown into relief the inadequacy of the standard transit circle for determining clock errors from night to night. The short-period erratics of quartz clocks are very small,

the mean scatter of daily rate being of the order of ± 0.05 milliseconds/day. The much larger erratics of Shortt clocks had previously made it very difficult to determine how much of the clock error observed nightly was due to the clocks and how much to the transit instrument. It now appears, however (Symms and Penny, *Monthly Notices Roy. Astr. Soc.*, **106**, 390, 1946), that the probable error of a single time determination, based on observations of 10 stars, is about ± 32 millisec. Yet it has long been apparent that the internal agreement between star and star is much better than this would suggest. This indicates that uncertainty in measuring the instrumental corrections—collimation and level errors—may contribute heavily to the external probable error. In a careful investigation of results obtained over five years with the Edinburgh transit circle, Symms and Penny confirm this surmise quantitatively and conclude that the scatter in measurements of clock error is largely due to the scatter in measurements of collimation error, the probable error of the actual transits being relatively small.

The transits were in fact recorded against a Shortt clock, but a comparison between this and a neighbouring quartz clock before, during, and after each observation gives observed errors of the latter. Through a radio time-signal link these transits are transferred so as to give errors of another quartz clock in London, selected as giving the best long-period performance over the interval considered. After removal of a term in t^2 representing the post-ageing frequency drift of the quartz crystal, the clock errors are plotted against time. The transits were observed so as to eliminate azimuth error, and were reduced using the collimation and level errors measured at the time by the observer actually making the transit observations. Perfect observations of stars whose places are perfectly known, made on a perfect instrument in perfect adjustment against a perfect clock should give zero clock errors at all times. The actual clock errors are not, of course, zero and are functions of time. Errors of measurement of collimation, errors of nadir settings, changes of clock rate, etc., will combine with actual errors of transit observation to produce these non-zero clock errors, and Symms and Penny proceed to correlate the observed clock errors with the instrumental corrections. Since their object is to investigate the day-to-day variations of these quantities, they first eliminate the systematic secular shifts in collimation error, nadir reading and clock rate by computing residuals from datum lines which take up these drifts. Different sets of datum lines are adopted for different observers, so as to allow similarly both for personal equation

itself and for its secular variation. The residuals which are then left form the numerical basis of the work—they are the accidental parts of the quantities involved.

Over a period of five months in which the collimation error shows no major discontinuities, the two sets of residuals, clock-error residuals and collimation residuals, are examined for any correlation which would indicate the extent to which accidental errors of measurement of collimation contribute to the scatter in the observed clock errors. For the four observers concerned, the mean correlation coefficient found is 0.59 ± 0.07 . This strong correlation indicates a material contribution to the total error by errors of collimation. If in fact the whole of the collimation residuals arise from errors of measurement, i.e. if there are no *real* fluctuations about the assumed datum lines, it is possible to calculate how large is that part of the probable error of a time determination which is not due to errors of measurement of collimation. The probable error of the original observations is ± 32 millisecon. Correcting for personal equation and for collimation on the above assumption gives an upper limit of ± 17 millisecon. for the probable error of the transit observations themselves.

A similar procedure carried out for level errors, or strictly (since level errors involve collimator readings) for nadir readings, gives a correlation coefficient between nadir residuals and clock-error residuals corrected for collimation of 0.10 ± 0.10 , an insignificant quantity.

An entirely different approach to the problem can be made by examining the internal consistency of a night's observations. The probable errors obtained by considering the residuals of each separate star are practically independent of personal equation and of errors in measuring the instrumental corrections. The mean probable error for four observers turns out to be ± 9 millisecon., and for each observer the probable error is systematically smaller than the value found by correcting for erroneous measures of collimation. This is not unexpected, for "night errors" due, for example, to lateral refraction affecting all the observations made on one night are eliminated from the internal probable error but not from the other estimate. But even if the larger value be accepted, the probable error of a time determination corrected for measurement errors in collimation is ± 17 millisecon., a very different figure from the ± 32 millisecon. which appears from uncritical examination of the crude clock errors. And this is an upper limit, including contributions due to any neglected real variation of collimation about the steady drift, and also contributions due to errors of star places and clock erratics.

It is very satisfactory to have this numerical confirmation of our suspicions that the major uncertainty in time determination with a transit circle lies in the values of the instrumental constants and not in the actual transit observations ; for fundamental star places, on which depends so much of positional astronomy and with it of stellar dynamics, are determined with meridian circles similar in principle and operation to the transit circle with which this investigation was carried out. If bisection with the hand-driven impersonal micrometer fitted to the Edinburgh transit circle can be acquitted of major error, we shall be safe with the motor-driven type common on the larger and more modern meridian instruments. The conclusions may react, too, on time determinations themselves. Clearly, if attention can be concentrated on improving methods of measuring the instrumental constants, now we know the weak link is there, the transit circle may take on a new lease of life. The present tendency is to pronounce it dead for time determinations, and to replace it by the photographic reflex zenith tube. It is no criticism of this latter instrument to say that we can afford to neglect no opportunity to improve its rivals to such a pitch that the observational side of the time service once again approaches a short-term accuracy comparable with that attained by the horological side.

PHYSICS. By F. A. VICK, O.B.E., Ph.D., F.Inst.P., The University, Manchester.

RECENT WORK WITH STABLE ISOTOPES AND MASS SPECTROGRAPHS.—Before the late war mass spectrographs were used mainly as instruments of high precision to determine, as accurately as possible, the masses of the various isotopes in relation to that of O_{16} , and also the relative abundances of the isotopes in the elements as they occur naturally. These two measurements combined enable molecular weights to be determined with an accuracy probably exceeding that of any other method. A knowledge of the masses of the isotopes, together with measurements of the energies of all the particles (including the photons) before and after a nuclear reaction, gave the first real verification of the Einstein relation

$$E = mc^2$$

where E is the energy resulting from the transformation of a mass m , and c is the velocity of light. Knowledge of isotope masses has been of the greatest importance in the development of "nuclear

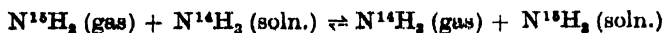
chemistry." Furthermore, accurate measurements make it clear that the mass of a stable nucleus is less than the sum of the masses of the constituent neutrons and protons. The mass "lost" is converted into the nuclear binding energy, the magnitude of which is a measure of the stability of the nucleus.

During the past decade, the scope of work with stable isotopes and mass spectrographs has widened considerably as a result of a number of factors. As has been admirably described in the Smyth Report (*Atomic Energy*, pp. 92-123, 1945) the separations of the isotopes of uranium, and of deuterium from hydrogen, were major steps in the development of the atomic bomb. The methods devised or improved are now available for other applications. One application is the use of stable isotopes as "tracer elements" in cases where radioactive isotopes are not available or are inconvenient. The mass spectrograph is no longer confined to a few academic laboratories, but is to be found in various forms in research organisations of all kinds, and is even becoming an important aid to production.

By definition, the isotopes of an element have the same chemical properties but differ in nuclear mass, so that to separate isotopes physical methods depending on mass differences must be employed. In a few cases the dependent physical properties differ sufficiently for methods of separation to be comparatively simple. For example, heavy water, D_2O , has a freezing point of $3.82^\circ C.$, boiling point $101.42^\circ C.$, and density 1.1074, so that separation of D_2O from H_2O by fractional distillation (for example) is feasible. For most elements, however, the percentage differences in mass and dependent physical properties of the isotopes are small, and more complex methods of separation must be used.

A useful review of separation methods tried up to the outbreak of the late war has been given by H. C. Urey (*Phys. Soc. Rep. Progr. Phys.*, 6, 48, 1940), so only brief mention of work before that date will be made here. Strangely enough, some of the methods are, after all, chemical, but this is because interatomic and intermolecular forces depend to a small extent on nuclear masses. For example, the equilibrium constant in exchange reactions between compounds of two different isotopes is not exactly unity, and as the reaction continues partial separation results. The method has been used successfully for the concentration of the isotopes of nitrogen and carbon, using a counter-current system in a fractionating column. Details of such methods would take us too far into the realms of chemistry, but it is of interest to observe that a mass spectrometer has been used to measure the relative abundances of the isotopes

present when an exchange reaction such as the following has reached equilibrium :



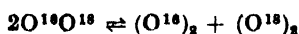
and so to determine the equilibrium constant for the reaction (H. G. Thode, R. L. Graham and J. A. Ziegler, *Canad. J. Res.*, **B.23**, 40, Jan. 1945). The exchange reaction between water and gaseous hydrogen, in the presence of a catalyst, was the basis of one of the methods of manufacturing heavy water used for atomic bomb work, the other method being the distillation process already mentioned (see H. G. Thode, S. R. Smith and F. O. Walkling, *Canad. J. Res.*, **B.22**, 127, July 1944).

It was observed some time ago that water removed from commercial electrolytic cells used for the production of hydrogen and oxygen contained enhanced concentrations of deuterium. This was the first method used on a large scale for the commercial production of deuterium, especially in Norway where cheap electric power is available. A typical figure for a pre-war plant was 35,000 ampere-hours for one gram of heavy water. A brief survey of electrolytic processes is given in the review by Urey (*loc. cit.*).

It is well known that the rate of diffusion of a gas through a porous barrier is inversely proportional to its molecular weight. Hence if a gas consisting of a mixture of two isotopes diffuses into a vacuum through a porous barrier, the concentration of the lighter isotope on the far side of the barrier will for a time be greater than that of the heavier, though if the process is continued until equilibrium is reached the concentrations of the isotopes will be the same on each side of the barrier. To separate isotopes, many stages of diffusion processes are necessary, and before the war the method had been used with success for the separation of Ne^{22} from Ne^{20} , and similar mixtures. Cascade processes were evolved in which only half the gas which has passed through one barrier is allowed to diffuse through the next, the other half being returned to an earlier stage. One of the methods of separating U^{235} from U^{238} for the atomic bomb was a cascade diffusion process with uranium hexafluoride.

If a temperature gradient is set up along a cylinder containing gases originally uniformly mixed, theory and experiment show that the heavier molecules tend to flow in the direction of decreasing temperature, and the lighter molecules in the reverse direction, until the resulting inequality of composition causes ordinary diffusion in the opposite direction sufficient to prevent further change in composition. The phenomenon of "thermal diffusion" is a complex

one, since it depends not only on a difference in molecular weights but also on the nature of the intermolecular forces. The effect disappears completely if one molecule repels the other with a force falling off as the inverse fifth power. Experimentally, however, the separation which can result from thermal diffusion of some mixtures is quite marked. K. Clusius and G. Dickel (*Naturwiss.*, **26**, 546, 1938) introduced a most interesting and efficient method of isotope separation based on thermal diffusion. In principle, a gas mixture is placed between two vertical parallel walls, one hotter than the other. By thermal diffusion the heavier molecules will concentrate at the cooler wall, and will then flow downwards by ordinary thermal convection, there being a similar upward flow of the lighter molecules at the hot wall. The result is, therefore, a relative concentration of lighter molecules at the top and heavier ones at the bottom. This can be shown as a lecture experiment with coal gas in a vertical glass tube fitted with a central wire through which an electric current can be passed to make it dull red. After five minutes or so, the top of the wire looks much duller than the bottom, since more heat is carried away through gas enriched with hydrogen. Clusius and Dickel used the method with success for the separation of the chlorine isotopes, and the principle is also applicable to liquids. The theory of the separation of isotopes by thermal diffusion has been discussed in great detail in a valuable paper by R. Clark Jones and W. H. Furry (*Rev. Mod. Phys.*, **18**, 151, April 1946). It is interesting to note that the hot-wire form of the apparatus facilitates catalytically useful exchange reactions such as



Other practical details are given in the paper, including numerical examples relating to the concentration of $\text{C}^{13}(\text{H}^1)_4$ and of He^3 . The latter is of considerable interest, since it might be used as a bombarding particle for nuclear disintegrations in a cyclotron. He^3 is a stable isotope present in a concentration of about 10^{-7} or 10^{-8} in natural helium. Jones and Furry conclude their paper with a critical review of previous papers on experimental work with the method, and a bibliography. Details of a Clusius-Dickel separator for the concentration of Cl^{37} have been given by E. F. Schrader (*Phys. Rev.*, **69**, 432, 1946).

An obvious method of separation based directly on difference of mass is the centrifuge, but early attempts met with little success. Urey suggested the use of tall cylindrical centrifuges with a counter-current flow, and several applications of such a device have been reported recently. In principle, there is a downward flow of vapour

or liquid in the outer part of the rotating cylinder and an upward flow near the centre. Across the interface between the two currents there is diffusion of molecules, and that of the heavier molecules outwards is helped by the radial force of the centrifugal motion, so that the concentration of heavier molecules increases in the peripheral region (*see* S. Goldwasser and H. S. Taylor, *J. Amer. Chem. Soc.*, **61**, 1751, 1939).

It is evident that the mass spectrograph itself could be used to separate isotopes by allowing the appropriate ion beams to fall in suitable collectors. The most spectacular use of this method was the separation of uranium isotopes for the atomic bomb in mass spectrographs constructed in cyclotron magnets in the U.S.A. during the war. The work is so well known that there is no need to discuss it further here.

As has been indicated above, stable isotopes find application as "tracer elements" in biological, chemical and physical researches, especially where for one reason or another radioactive isotopes cannot be used. Important stable isotopes in this connection are deuterium, C^{13} , N^{15} and O^{18} . Care must be taken, of course, that physical processes investigated are not themselves influenced by the substitution of a tracer isotope of different mass. The relative abundances of the isotope before and after being subjected to the processes studied are determined by use of a mass spectrograph. C^{13} is now produced commercially (*Nature*, **157**, 71, 1946), as well as heavy water and O^{18} .

In addition to measurement of abundance of isotopes in tracer-element work, the mass spectrograph (or mass spectrometer) is finding many applications in pure and applied research. Such applications have stimulated the development of instruments suitable for more general use than those which gave such valuable results in the hands of Aston, Dempster, Bainbridge and Mattauch. It is appropriate, therefore, to review briefly some of this recent work. A short review of the development of mass spectrometers for the measurement of isotope abundance is given in a paper by E. B. Jordan and L. B. Young (*J. Appl. Phys.*, **13**, 526, 1942). In a following paper, E. B. Jordan and N. D. Coggeshall consider in more detail the principles of design of a mass spectrometer for relative abundance measurements, and the main sources of error (*J. Appl. Phys.*, **13**, 539, 1942). In such spectrographs the ion currents are recorded electrically by means of an electrometer valve and amplifier. A plot is made of the ion currents as the isotopic ion beams are swept in turn across the collector slit by (for example) varying the intensity of the deflecting magnetic field. The relative

heights of the peaks of the ion current curves indicate the relative numbers of the isotopes present in the source after various corrections have been made. It is also possible to measure in the same instrument the various ionisation potentials, and to determine the shape of the ionisation probability curve. The ions are created by an electron beam passing through the gas or vapour of the substance investigated, at a pressure of about 10^{-4} mm. Hg. The electron beam moves parallel to a local magnetic field, and the ions created are accelerated through appropriate slits by a local electric field perpendicular to the electron beam. After leaving the last "source slit", the ions pass through a magnetic field at right angles to their paths and are bent through angles depending on the ratio of charge to mass for each isotopic ion. Sometimes, instead of varying the deflecting magnetic field in the manner described above, the ion beams are moved across the collector slit by varying the electrical potential differences accelerating the ions. There are two main types of mass spectrometer in general use. In the first the whole path of the ion beams is in a uniform magnetic field, and the total beam deflection is 180° . In the second the magnetic deflecting field is confined to a small portion of the ion paths, and the total deflection is less than 180° —e.g. 60° or 90° .

J. A. Hipple (*J. Appl. Phys.*, **13**, 551, 1942) describes the application of the second type of mass spectrometer, with deflection angle 90° , to gas analysis. The paper contains a useful bibliography of previous work on gas analysis with such an instrument. Since the heights of the peaks on the ion-current curve depend not only on isotopic abundance but also on efficiencies of ionisation, it is usual to calibrate the spectrometer by observing the curves obtained with known mixtures of the gases studied. Care must be taken not to dissociate thermally any of the molecules, particularly hydrocarbons, investigated by, e.g., contact with the hot filament supplying the ionising electron beam. A mass spectrograph of the type considered is most valuable for looking for impurities in gases used as sources of bombarding particles in nuclear disintegration experiments, or for investigating complicated mixtures of hydrocarbons when only small quantities are available. A review of further applications to chemistry, and also of some uses of stable isotopes, is provided by D. Rittenberg (*J. Appl. Phys.*, **13**, 561, 1942).

A kindred application is the use of the instrument to detect leaks in large metal vacuum systems (H. A. Thomas, T. W. Williams and J. A. Hipple, *Rev. Sci. Instr.*, **17**, 368, 1946). In principle helium gas is sprayed on to the region where a leak is suspected, and entry of the gas into the vacuum system is detected by a mass

spectrometer prefocussed for helium. The authors describe an instrument capable of detecting 1 part of helium in 400,000 parts of air at 10^{-4} mm. Hg. pressure.

A. T. Forrester and W. B. Whalley (*Rev. Sci. Instr.*, **17**, 549, Dec. 1946) describe a method whereby, at a constant magnetic deflecting field, the ion energies may be varied by a small periodic variation of ion accelerating potential. The voltage produced by passing the ion current reaching the collector through a high resistance is applied, after appropriate amplification, to give a vertical deflection of a cathode-ray tube spot, the horizontal deflection being synchronised to the changing potential which varies the ion energies. The resulting trace on the screen gives a picture of the variation of ion current with e/m , where e and m are the charge and mass of an ion. This forms a very convenient and rapid method of obtaining an estimate of the kinds and numbers of ions present, for the whole mass spectrum can be traversed across the cathode-ray tube screen by increasing or decreasing the magnetic field of the mass spectrograph.

The spectrographs described so far are suitable for investigating the isotopes present in a gas or vapour. A. E. Shaw and W. Rall (*Rev. Sci. Instr.*, **18**, 278, May 1947) describe one intended for the analysis of solids over a wide mass range, based on a design by Mattauch, in which the ions traverse first a radial electrostatic field and then a homogeneous magnetic field. This arrangement can be designed to give both direction and velocity focussing over a wide range of e/m . The various ion beams are received on a photographic plate, as in Aston's instrument. The ions are produced by a Tesla discharge in a hollow cathode, consisting of an outer metal cylinder with an inner concentric metal cylinder of the metal to be examined, or packed with the solid under investigation. The resolution $\Delta m/m$ achieved was $1/250$.

In a following paper (*Rev. Sci. Instr.*, **18**, 294, May 1947) A. O. Nier, E. P. Ney and M. G. Inghram describe a method of determining more precisely the relative abundance of two given ions. Each ion beam is received in a different collector, and the two resulting currents are compared by changing one in a known manner until it is equal and opposite to the other. The method is thus essentially a null one, and the circuits employed are designed to eliminate errors due to voltage fluctuations, etc. The method has obvious applications to production control.

A spectrometer designed for isotope abundance measurements in connection with the atomic bomb project is described by R. L. Graham, A. L. Harkness and H. G. Thode (*J. Sci. Instr.*, **24**, 119,

May 1947). The instrument is used for isotopic analysis of the heavier elements and various fission products, and the design is similar to that described by Hipple (*loc. cit.*). The main tube is of copper, with glass ends containing the ion source and Faraday cage collector respectively. The design of spectrometer and associated stabilised electrical control circuits are described in detail, and the paper is thus very valuable to those contemplating using such an instrument. An isotope present in a proportion of 1 in 100,000 can be detected. Another useful paper is that by A. O. Nier (*Rev. Sci. Instr.*, **18**, 398, June 1947), describing a spectrometer of the 60° deflection type for isotope and gas analysis, employing the null method mentioned above. These last two papers give a very good idea of what has been achieved by attention to design details.

A very compact and simple design of 180° deflection mass spectrometer for analysis in the low mass range has been described recently by W. Siri (*Rev. Sci. Instr.*, **18**, 540, Aug. 1947). The instrument is not intended to have the resolution and sensitivity of those described in the previous paragraph, but is designed for quantitative analysis with an accuracy of 2.5 per cent. in the mass range 4 to 65 to one mass unit, which is adequate for most biological and medical research involving stable isotopes and gas mixtures. A permanent magnet is used, and the ions are swept past the collector slit by an oscillating accelerating potential. The mechanical design is very simple and ingenious. The mass spectra are observed on a cathode-ray tube screen, as described by Forrester and Whalley and reviewed above.

Finally we have a recent paper describing a very different method of separating two isotopes (J. G. Daunt, R. E. Probst, H. L. Johnston, L. T. Aldrich and A. O. Nier, *Phys. Rev.*, **72**, 502, Sept. 1947). In an earlier article in this series (*SCIENCE PROGRESS*, **33**, 517, 1939) some of the then recently discovered properties of liquid helium were described. Among them was the property of liquid helium, II of "super-fluid flow" over a surface from one vessel to another at a slightly different temperature. In the paper now before us a similar experiment is described with liquid helium below the λ -point. The abundance ratio of He^3/He^4 was measured by a mass spectrograph for the liquid before and after the super-fluid flow over a surface, and the results showed that, within the accuracy of measurement, the He^3 isotope did not take part in the flow. The experiment indicates a possible method of separating He^3 from He^4 , and work on this is in progress. The phenomenon is also a challenge to theoretical physicists.

METEOROLOGY. By M. K. MILES, M.Sc., A.R.C.S., Imperial College of Science and Technology, London.

THE STRUCTURE OF THE TROPICAL ATMOSPHERE : ANALYTICAL AND PREDICTION METHODS

THE tropical atmosphere is considered to be that broad region of predominantly easterly winds lying between the subtropical high-pressure belts of the two hemispheres. In this simplest picture pressure would decrease towards the equator from both hemispheres with a belt of low pressure encircling the earth at the equator. This is the "Doldrums" of the mariner, with its light winds and frequent calms.

In fact, the subtropical high-pressure belt consists of four or five large anticyclones, and these, like the belt of minimum pressure, have a seasonal movement following the sun. Much of the interest and variety of tropical weather are due to this.

The region of easterly winds on the equatorial margins of the subtropical anticyclones is characterised by quiet weather and the small cumulus clouds typical of the trade winds. It presents less immediate problems and has naturally attracted less attention than the belt of so-called deep easterlies which lie on its equatorial side. These extend meridionally to the doldrums, when these are on the equator or in the same hemisphere, and are a region of small but fairly uniform pressure gradients. The tropical easterlies are in general overlain by westerly winds. The boundary is quite low—a few thousand feet—near the subtropical anticyclones, rises equatorwards, at first gradually and then fairly steeply, and in equatorial regions easterlies are sometimes found extending to the tropopause.

The region of deep easterlies is the abode of tropical storms, but only when the doldrums are displaced into the same hemisphere. The seasonal movement of the doldrums has long been regarded as a controlling factor in tropical meteorology, and extended observations by aircraft and radio-sonde during the war years have emphasised and clarified this view. A northward displacement of the pressure minimum results in the winds on the south side having a westerly component, and there is then cyclonic shear in the velocity field of the trough. When it is on the equator with easterly winds on both sides there is likely to be little or no shear. This meeting zone of winds from the two hemispheres is referred to in the literature as the Intertropical Front (I.T.F.) or the Intertropical Convergence Zone (I.C.Z.). In this article we shall use the abbreviation (I.C.Z.) because, while it is not invariably a region

of convergence, it seldom shows a strong temperature gradient, which is the *sine qua non* of a front.

CONVECTION IN THE TROPICS.—It is now fairly well agreed that, not only is convection vastly more important in the tropics than elsewhere, but it has several characteristics strange to a meteorologist who knows only extra-tropical convection.

There is two to three times as much water vapour in the air as in temperate latitudes and tropical convection releases large amounts of rain and, what is perhaps more important, latent heat energy; here, most meteorologists would say, lies the source of the tropical cyclone's energy.

Cumulonimbus clouds are frequently observed closely packed together over a horizontal area tens of miles across in the I.C.Z. and layers of alto- and cirro-cloud drawn from their upper regions produce complete overcast conditions. Observers of U.S.A.A.F. weather reconnaissance aircraft in the south-western Pacific have reported that the cloud mass in a tropical cyclone consists of many individual clouds closely packed together.

Convection in the tropics shows the familiar increase in intensity by day overland, but full maturity does not seem to be reached till after sundown, and the cloud masses are sometimes still existent by dawn, though no longer active. There is a night or early morning maximum of convective activity over the sea, which is a great deal more marked than in extra-tropical regions.

It is the rule rather than the exception for heavy precipitation to occur from tropical cumulus clouds when the top is far below the freezing level, and there is further evidence of the tendency of tropical convection clouds to early colloidal instability in a note from W. J. Kotsch in the *Bulletin of the American Meteorological Society* of February 1947 of heavy showers occurring from a cloud less than 5000 ft. thick over Guam.

STRUCTURE OF THE EQUATORIAL EASTERLIES.—Between the region of trade winds and up to a few hundred miles from the I.C.Z. the easterlies have been found to consist of a moist lower layer with drier and warmer air aloft. Convection normally takes place readily enough in the moist lower layer, but the inversion (generally called the T_s inversion from the French name *Tropical Supérieur* for the dry air aloft) effectively checks it. The height of the T_s inversion has been found to vary from place to place by 15,000 ft. to 20,000 ft. and day-to-day variations at any one place from 4000 ft. up to 15,000 ft. were reported by Dunn (*Bull. American Met. Soc.*, June 1940) in an investigation into the association between anallobaric and katallobaric waves and hurricanes in the Caribbean.

Regions of persistently high T_s inversion appear to be associated with regions of convergence in the general flow, but the day-to-day variations are difficult to account for. Dunn (*loc. cit.*) suggested that the height of the T_s inversion increased as or just before a katalobaric system passed over the Virgin Islands, but one can find occasions from the graphs he gives where the height of the T_s inversion increased markedly during and after a pressure rise.

Riehl, from a similar study in the Caribbean, postulated the existence of "waves in the easterlies" (*Misc. Reports* 17, Dept. of Meteorology, University of Chicago, January 1945) which move westwards and have bad weather on their east side. There are slight wind changes at the ground, with more marked changes at 10,000 ft., but no appreciable temperature gradients. The bad weather, it will be noted, occurs after the surface pressure minimum, and Riehl says that the deep, moist layer behind is associated with velocity convergence behind the wave. This association of rising pressure with high value of T_s inversion appears at variance with Dunn's earlier findings, but, since many of these waves take less than 12 hours to pass over a station, a slight forward tilt to the wave could lead to the impression that the falling pressure associated with the approach of the wave was accompanied by an increasing depth of the moist layer. Further, there is reason to believe that Riehl chose for analysis only the well-marked cases, whereas Dunn examined all isallobaric systems during August 1935.

Waves in the easterlies have been detected by Bryson in the south-western Pacific, but are generally much smaller than those Riehl found in the Caribbean. Bryson claims a 12-day cycle, during which the interval between successive waves decreases from 38 hours at the beginning and end of the cycle to a minimum of 28 hours at the middle (*Report of Present Methods of Tropical Forecasting*, Naval Meteorological Branch, May 1947).*

During late summer when the thermal equator is at its maximum displacement from the geographical equator there will be occasions when the temperature will increase polewards in the easterlies, leading to an increase in the strength of these winds with height. This condition has been shown by J. Bjerknes and Holmboe (*J. Meteorol.*, 1, 1944) to be a state of dynamical instability favouring the intensification of any small waves that may occur in the easterlies, and they suggest that this may be a significant circumstance for development of tropical cyclones. A strong thermal gradient polewards results in a low level for the occurrence of westerlies,

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METEOROLOGY

and this is recognised as unfavourable to the development of tropical cyclones.

A local strengthening of thermal gradient polewards occurs in the easterlies when a polar cold front moves southwards round the subtropical anticyclone. A lowering of the base of the westerlies results as shown in Fig. 1, and when this is travelling more or less eastward along the poleward side of the deep easterlies the system is referred to as a polar trough. The sequence of winds at, say, 20,000 ft. would then be E., S.E., S.W. through W. as the trough line passes, to N.W., and later returning to E. again. Further equatorward the passage of a polar trough may cause only a slight cyclonic ripple on the easterly current. Weather may show some deterioration, mainly an increase of high and medium cloud moving eastward ahead of the polar trough.

Palmer and others (*Report on Extended Range Tropical Forecasting*, Met. Office, Wellington, New Zealand, 1945), from a study of streamline charts in the southern Pacific, have found that belts of flow convergence are closely associated with intense convection in the easterlies.

INTER-TROPICAL CONVERGENCE ZONE.—This is normally found in the latitude of maximum convective instability, as shown by a graph of latitude distribution of wet bulb potential temperature due to K. N. Rao and Sir Charles Normand (*Q.J. Met. Soc.*, 72, 1946) and is frequently characterised by violent and widespread convection.

A series of war-time flights made through the zone in the eastern Pacific (*Bull. Amer. Met. Soc.*, Dec. 1945, Jan. 1946, Feb. 1946) indicated that the severity of convection increased with the displacement of the I.C.Z. from the equator. It is now generally accepted that a displacement of more than 10° from the equator over the sea provides a favourable state of affairs for the formation of tropical cyclones, especially if the zone is inclined to the parallels of latitude. Where it is overland, as in West Africa and the Sudan in July, the monsoon air from the southern hemisphere is markedly cooler than the easterly current on the north side of the zone which

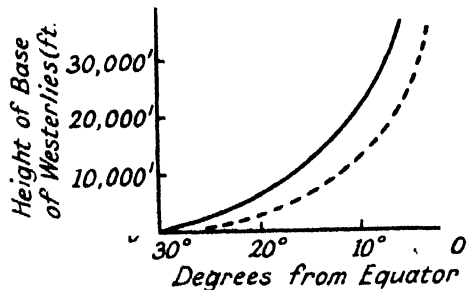


FIG. 1.

— Normal Distribution.

- - - - - During Passage of Polar Trough.

overruns it (Hamilton and Archbold, *Q.J. Roy. Met. Soc.*, **71**, July 1945, Fig. 2). Under these circumstances there is little or no weather near the zone, since the easterly is a dry current. About 200 miles south of the zone, where the westerlies are about 6000 feet deep, the easterlies aloft are much moister and convection is often intense. Hamilton and Archbold (*loc. cit.*) in their discussion of West African tornadoes show that they occur in this region and distinguish the moist easterlies aloft from the dry Saharan current found near the I.C.Z. by calling them equatorial easterlies.

At sea, attempts to find the temperature gradient across the I.C.Z. have shown conclusively that this is variable and usually extremely small. An analysis of southerly winds at Panama showed that they were cooler than the north-easterlies on the north side

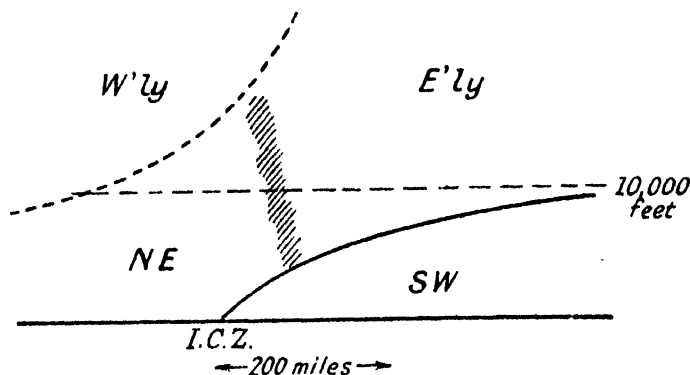


FIG. 2.

of the I.C.Z., but Kidson (*Q.J. Roy. Met. Soc.*, **63**, pp. 339-40) describes a situation in which the northerlies were markedly cooler.

The day-to-day variations in its position are sometimes so large as to suggest it is not a material surface, but in general it should be remembered that there is a good deal of latitude in assigning a position to it. There is normally no strong wind discontinuity in the zone: winds decrease in strength gradually towards the centre of the zone.

The flights mentioned earlier through the I.C.Z. in the eastern Pacific have revealed that, with rare exceptions, it is north of the equator there throughout the year. Mean values of its position given for two years' observations are: January, February and March, 2-5° N.; May-October, 8-11° N. Similarly the zone suffers very little displacement into the southern hemisphere over the southern Atlantic. It is noteworthy that this ocean *alone* among tropical seas is free from intense tropical storms.

TROPICAL STORMS.—These comprise the hurricanes of the West Indies, the cyclones of the northern and southern Indian Ocean, the typhoons of the China Seas and the cyclones of the southern Pacific, north-east and north-west of Australia. They are all characterised by an oceanic origin, steep pressure gradients and a central core of clear air of varying diameter. Most of them start off travelling in a westerly direction at about 10 m.p.h., eventually recurve away from the equator into extra-tropical latitudes where they lose their intensity. Flights into and round tropical storms by H. Wexler and F. B. Wood (*Bull. Amer. Met. Soc.*, May 1945) have confirmed that the central clear core increases in diameter with height, and revealed the existence of a cloud-free, turbulence-free region underneath the inverted cone-shaped cloud mass of the storm. The only cloud reported under this inclined cloud mass was flat soud or small cumulus clouds.

Flights by U.S.A.A.F. hurricane recco aircraft from the Marianas (*Bull. Amer. Met. Soc.*, Nov. 1946) have shown that weather on the south side of typhoons is frequently worse than that on the north and west sides. They confirm the existence of thickening cirrus layers about 300 miles from the centre of the storm.

There have been few direct observations of the vertical extent of tropical storms, but the general view is that they extend to at least 10 km., and many well-developed storms are deeper than this. There is some evidence (E. J. Schacht, *Bull. Amer. Met. Soc.*, June 1946) that temperatures within a radius of about 200 miles of Caribbean hurricanes are higher by some 3° F. between 4 and 14 km. than the normal undisturbed environment. These results are based on about a score of hurricane soundings made over the years 1941-44 and they cannot be considered entirely conclusive. A more detailed study of the soundings has been made by H. Riehl, it is reported, and will appear shortly as part of a report *On the Development of Tropical Storms*.

Evidence for a ring of strong upward flow about 17 km. wide has been discussed by C. E. Depperman (*Bull. Amer. Met. Soc.*, Jan. 1946). The reports of the hurricane recco observers mentioned above suggest that the strong upward flow is fairly concentrated around the calm central core.

It is generally agreed that tropical storms are most likely to develop where there is abundant convective instability. Several forecasters in the south-western Pacific have pointed out that storms are most likely to develop where there is a closed low-pressure area on the I.C.Z. It is thought that the pre-existent, though weak, cyclonic wind circulation brings together the various large cumulo-

nimbus cloud masses in the area and concentrates the available latent heat energy in a smaller area. Certainly it has long been known that an increase of showeriness and squalliness is a prelude to the formation of a tropical storm.

Depperman and others have claimed that fronts occur in tropical storms and have suggested that the storm derives its energy from frontal processes after the manner of extra-tropical depressions. The arrival of an extra-tropical cold front in the I.C.Z. forming a "triple point" was regarded by many meteorologists (see Schofield, *Bull. Amer. Met. Soc.*, June 1938) as a favourable situation for the formation of a storm. Australian meteorologists (*Report of Present Methods of Tropical Forecasting*, Naval Met. Branch, May 1947) consider the passage of a deep, cold air mass up the Tasman sea is almost a necessary condition for the formation of storms in the Coral Sea: of 55 cases in 1941-45, when a cold anticyclone penetrated the Tasman Sea, 29 were followed by a discernible tropical cyclone within 6 days, while only two tropical storms formed in the absence of this condition. One school of thought sees in this conditions leading to formation of a "triple-point" and claims that only when the isobars around the triple point are markedly cyclonic does a storm develop. Another interpretation is in terms of the trough that occurs aloft ahead of a cold front moving into low latitudes. It is suggested that it may lead to a wave-like disturbance on the I.C.Z. This is equivalent to the interpretation in terms of polar troughs of one group of American meteorologists (see later).

Opinion generally in recent years has been inclining away from frontal interpretations, and, accepting convective instability as an essential condition, has sought to discover what conditions favour the development of a strong circular vortex from a region of weak circulation and strong convection.

The presence of deep easterlies is generally regarded as essential (Riehl and Shafer, *J. Meteorol.*, 1, Sept. 1944); when the base of the westerlies is down to middle cloud level, say 15,000 ft. to 20,000 ft., hurricanes do not develop over the West Indies. The passage of a polar trough far enough north to give the easterly current a cyclonic deformation, but not bring the base of the westerlies down so far, is held to be effective in leading to intensification of a weak low level circulation. This, however, does not explain how sufficient divergence occurs aloft to overcompensate for frictional inflow in the lower levels and lead to the deep minimum of surface pressure observed in a fully developed storm.

The theories of upper divergence fall into two main classes

which may be called, shortly, circularly symmetrical divergence in the upper levels of a vortex, and divergence associated with a lagging wave motion aloft. Durst and Sutcliffe (*Q.J. Roy. Met. Soc.*, 64, 1938, No. 273) showed how the first type could occur in a vortex which was warmer than its environment by reason of an ageostrophic radial wind v , given by

$$v_r = - \frac{w \frac{\partial V}{\partial z}}{\lambda + V/r + \partial V/\partial r}$$

where w is the vertical velocity, V is the tangential velocity and λ the coriolis parameter; $\lambda + V/r + \partial V/\partial r$ represents the total absolute vorticity of the air and can be regarded as a positive quantity everywhere in the storm, so that for ascending motion, $w + ve$, there will be an outflow v_r positive if $\partial V/\partial z$ is negative. This is the condition that the tangential velocity decreases with height, which it will do if the storm is warmer than its environment. What observational evidence there is goes to show that the cyclonic rotation of tropical storms decreases with height, and that air which rises near the centre is flung outwards radially as it ascends. The outward radial motion can occur over the wide range of height where the radial pressure gradient is decreasing and air is ascending, and will be of sufficient magnitude to cause deepening for $w = 5$ cm./sec.—a very modest value even at high levels.

The other view is developed by J. Bjerknes and J. Holmboe (*J. Meteorol.*, 1, Sept. 1944) who show that for a sinusoidal wave there will be divergence ahead of the trough if the mean zonal wind exceeds the translation speed of the wave by an amount

$$\frac{2\Omega a \cos^2 \bar{\phi}}{n^2},$$

referred to as the critical velocity,

where Ω is the angular velocity of the earth,

a is the radius of the earth,

$\bar{\phi}$ is the mean latitude of the wave,

and n is the number of waves along the total circumference of the latitude circle.

In low latitudes and for fairly short waves ($n = 10$) v_c is about 5 m./sec. Thus for slow-moving troughs in the easterlies there will normally be divergence ahead of the trough. An almost stationary upper trough lying to the east of an incipient tropical storm would

be expected to lead to a fall of pressure over the area and a growth of cyclonic circulation. On this expectation W. Krasner and H. London (*Bull. Amer. Met. Soc.*, Sept. 1944) examined the tilt of the axis in the early stages of thirteen tropical storms. They used wind shear with height to determine the direction and amount of tilt, and claimed that in eight cases eastward tilt was followed by development or westward tilt by no development; there were four cases of no tilt and one case that went against the rule. One cannot, however, feel very sure that these cases really demonstrate the reality of the process, since Bjerknes' and Holmboe's treatment requires an open wave aloft, and it is not certain whether a wind shear with height implies this or merely a tilt of the axis of the closed circulation with height. The necessary temperature gradient to give an eastward tilt requires appropriate distortion of an easterly current which is originally cooler on its equatorial side. This is a state of dynamic instability in which the easterlies increase with height (Bjerknes and Holmboe, *loc. cit.*).

In a recent paper J. S. Sawyer (*Q.J. Roy. Met. Soc.*, 73, 1947) has considered the circumstances under which circularly symmetrical divergence will occur in the very early stages of a tropical storm before there is sufficient cyclonic rotation in the upper or, indeed, in the lower layers for the Durst-Sutcliffe effect to exist. He considers the isobaric and isallobaric highs which must occur at upper levels of a convective column in an environment with a lapse rate greater than the wet adiabatic rate of potential wet bulb 80° F.—the usual value of surface wet bulb temperature over tropical oceans. The rise of pressure aloft as the convection develops will lead to an approximately radial motion away from the convective column given by $-\frac{1}{\lambda^2 \rho} \frac{d}{dt} \left(\frac{\partial p}{\partial r} \right)$

where λ is the coriolis parameter,

ρ is the air density,

$\frac{\partial p}{\partial r}$ is the radial pressure gradient.

So long as $\frac{d}{dt} \left(\frac{\partial p}{\partial r} \right)$ is negative, i.e. central pressure rising, the outflow will continue. If this isallobaric term is the only one operative, Sawyer states, a balanced vortex anticyclonic aloft and cyclonic below will have been established at all levels by the time pressure changes cease and there will be no further outflow. He considers under what dynamical conditions outflow will not stop

at this stage. He writes the equation of motion for any time while pressure is rising over the centre of the column as

$$\lambda v_0 + \frac{v_0^2}{r} = \frac{1}{\rho} \left(\frac{\partial p}{\partial r} \right)$$

and considers the effect on the radial motion u of an increase of tangential motion from v_0 to $v_0 + v^1$ resulting from an increase in the radial pressure gradient by Δ . By ignoring a term $w \frac{\partial u}{\partial z}$, a term $u \frac{\partial u}{\partial r}$ and all frictional terms he derives the following two equations for the motion

$$\frac{\partial u}{\partial t} = \frac{\Delta}{\rho} + v^1 \left(\frac{2v_0}{r} + \lambda \right)$$

and

$$\frac{\partial v^1}{\partial t} = -u \left(\lambda + \frac{\partial v_0}{\partial r} + \frac{v_0}{r} \right) - u' \frac{\partial v_0}{\partial z}$$

Eliminating v^1 and $\frac{\partial v^1}{\partial t}$ between these two equations yields the following equation for the radial motion

$$\frac{\partial^2 u}{\partial t^2} = -B \left\{ u - \frac{1}{B} \frac{\partial}{\partial t} \left(\frac{\Delta}{\rho} \right) \right\} - w \frac{\partial v_0}{\partial z} \left(\frac{2v_0}{r} + \lambda \right)$$

where

$$B = \left(\lambda + \frac{2v_0}{r} \right) \left(\lambda + \frac{\partial v_0}{\partial r} + \frac{v_0}{r} \right)$$

Ignoring the term involving $w \frac{\partial v_0}{\partial z}$ (the Durst-Sutcliffe term) the condition for u to increase with time at any distance r from the centre is that $B < 0$. He investigates the two possibilities and concludes that the more likely one is that

$$\frac{v_0}{r} > -\frac{\lambda}{2} \text{ and } \left(\lambda + \frac{\partial v_0}{\partial r} + \frac{v_0}{r} \right) < 0$$

This means an upper vortex which may have cyclonic or anti-cyclonic rotation up to a limit of $-\frac{\lambda}{2}$ and absolute vorticity anti-cyclonic, and such a vortex is dynamically unstable, in that after a small outward displacement it will continue to move outwards. By rewriting the shear and rotation terms of the vorticity as a gradient of circulation $\frac{1}{r} \frac{\partial}{\partial r} (v_0 r)$, he expresses the condition for instability

as a vortex in which circulation decreases outward at a greater rate than λ . Conservation of angular momentum requires that the value of $\frac{1}{r} \frac{\partial}{\partial r}(vr)$ in an expanding vortex be equal to $-\lambda$, so that

expansion in an environment where $\frac{1}{r} \frac{\partial}{\partial r}(vr) < -\lambda$ would result in growing unbalance of the centrifugal force.

Sawyer points out that the mean March winds at 6 km. over Rangoon and Port Blair show an anticyclonic shear of greater magnitude than $\frac{\lambda}{2}$. He concludes that such a shear may lead to

the formation of anticyclonic vortices between the two currents, whose vorticity may occasionally be greater than $-\lambda$, and so provide an environment in which deep convection would grow into a storm circulation. In the absence of any rotation in the region between the polar westerlies and the tropical easterlies a sheer $\frac{\partial v_0}{\partial r} < -\lambda$ might, it seems, lead to instability to the north and south of the convective column, but the stability over the rest of the ring would probably check development of this kind. Examination of shear and rotation at levels between, say, 6 and 10 km. over the I.C.Z. appears to be the only way of establishing the validity of the criterion, in view of the neglect of the $w \frac{\partial v_0}{\partial z}$ term.

Sawyer also discusses a relatively late stage in the development, when air which has gained cyclonic rotation by convergence to the centre of the storm at low levels ascends to the level we have been considering, leading to a cyclonic vortex within the original anticyclonic vortex. By making an assumption about the tangential velocity at the inside of this cyclonic vortex and computing the amount of expansion required to reduce this to zero at a rate $\frac{1}{r} \frac{\partial}{\partial r}(vr) = -\lambda$, he is able to compute the fall of pressure at this level over the centre of the storm. The effect of this at the surface (about 10 millibars) plus the thermal pressure deficit, which he estimates at some 20-30 millibars compared with the value of 80 millibars for convection to 15 km. given by Sir Napier Shaw in *Geophysical Memoirs of the Met. Office*, No. 19, 1920, represents the total pressure fall associated with the storm. While it is reasonable to suppose that air of strong cyclonic rotation will be lifted to high levels at a fairly mature stage of the storm, it is not so clear how pressure aloft will have fallen to accommodate this rotation.

It may be, of course, that the region of anticyclonic flow has moved to still higher levels and produced the necessary divergence to account for the pressure fall. The paper, which also includes a discussion of the effect of horizontal turbulence on the vertical structure of the eye, tackles the fundamental problems of the tropical storm in a highly original way.

The kinematics of the tropical storm do not appear to have received any formal study since Horiguiti ("On the Typhoon of the Far East," *Memoirs of the Kobe Imperial Marine Observatory*, Parts I, II, III and IV, 1926 and 1928) from surface wind observations at a number of islands in the China Sea concluded that the relation of tangential wind speed to distance from the centre was best represented by $vr^{\frac{1}{2}} = \text{const.}$ This indicates a loss of angular momentum, since convergence to a point under conservation of angular momentum leads to the distribution $vr = \text{const.}$ Horiguiti found an average angle of inflow of 40° for the Okinawa typhoon, so that clearly frictional forces are large enough to account for this loss. K. P. Kidd and C. K. Reed (*Bull. Amer. Met. Soc.*, June 1946) say that in many cases in the western Pacific strongest winds are not immediately outside the eye, but many miles from the centre. In two cases the strongest winds approached the centre and the eye decreased in diameter as the storm developed.

It has long been believed that tropical storms are steered by high-level winds, but a formal statistical study does not yet seem to have been attempted, principally of course because upper wind observations in the vicinity of tropical storms are not very plentiful. The general view seems to be that in the early stages the 10,000-ft. winds are likely to provide the best indication of the direction of motion, but for a well-developed storm 15,000-ft. winds are more useful; the Australian forecasters say 20,000 ft. for deep storms (*Report on Present Methods of Tropical Forecasting*, Naval Met. Branch, May 1947).

Recurvature has always been rather difficult to explain satisfactorily and still cannot be considered to be fully understood. A statistical study carried out on West-Indian hurricanes by Riehl and Shafer (*J. Meteorol.*, 1, Sept. 1944) suggested that upper wind distribution was closely connected with recurvature. They showed that when the base of the westerlies falls below about 14,000 ft. to the westward of a tropical storm recurvature is extremely likely. This will normally be with the passage of a polar trough. They point out that the trough must be intense enough or sufficiently southerly to bring down the westerlies over a fairly broad region, otherwise a slight cyclonic deformation of the easterlies is all that

will occur, and the storm may make a small movement northwards and then resume its west or west-north-west path without any real recurvature having taken place. They discuss the special cases in which deep easterlies reform quickly behind a polar trough, and claim that even using 14,000-ft. charts for wind distribution they encountered only three failures out of sixty cases examined.

MICROSEISMS AND TROPICAL STORMS.—Sensitive seismographs reveal the existence of many earth waves of small amplitude, and it has long been known that these become more intense when there are intense storms raging at sea. There are two main views about the origin of the microseisms, as these small earth waves are called. On the one side it was held that swell waves pounding on steep rocky coasts produced them, while a number of tropical meteorologists believed that there exists an even closer connection than this between storms and certain microseisms (Macelwane, *Annales de Géophysique*, 2, 1946, Fasc. 4, and Gutenberg, *J. Meteorol.*, 4, Feb. 1947).

Following many inconclusive experiments to determine the direction of propagation of the microseismic waves, a determination by Ramirez (*Bull. Seismol. Soc. Amer.*, 30, 1940), using three sensitive seismographs at the vertices of a triangle, provided strong evidence that the storm microseisms of period about 4 secs. originated from the centre of storms and not from the nearest coasts. A series of determinations using a similar arrangement of seismographs at Guantanamo Bay, Cuba, by the U.S. Navy in 1944, confirmed Ramirez's conclusion that the microseisms come from the centre of hurricanes moving along the Caribbean. See M. H. Gilmore, *Bull. Amer. Met. Soc.*, 28, Feb. 1947, or *Bull. Seismol. Soc. Amer.*, 36, pp. 89–119, 1946, for an account of this work and extension of it in the hurricane seasons of 1945 and 1946. Gilmore concludes that, with experience, microseisms caused by tropical storms can generally be distinguished from those caused by strong winds associated with other types of steep pressure gradient. Banerji in 1929 (*Nature*, 123, pp. 163–4) had observed that microseisms associated with the Indian south-west monsoon are steady and have a period between 4 and 10 seconds, while those with tropical storms are of period between 4 and 6 seconds and show irregular variations in amplitude. The amplitude of microseisms is seriously modified by the geological structure of the earth between station and storm, but, once the principal fault lines are known, there seems to be little doubt that the position of a tropical storm can be located on many occasions to within 50 miles and frequently before it appears on the synoptic charts.

The exact cause of the microseisms is not yet understood, but it is certain that the depth of water under the central region of the storm is important. Microseisms from quite intense storms show a marked fall in amplitude while the storm is passing over shallow water or land.

PRESENT FORECASTING METHODS.—While an isobaric chart remains an essential tool in tropical forecasting, war-time experience has shown that an analysis of the wind field independently of the pressure field is of great value. New Zealand meteorologists have been doing this by means of flow lines giving the direction of the velocity vector and speed lines giving its magnitude. Some tropical forecasters have found it profitable to draw streamlines whose separation varies inversely with wind speed. Additional streamlines have then to be introduced in regions of divergence, and disappearing streamlines occur in regions of convergence. Palmer and others (*Report on Extended Range Tropical Forecasting Project*, Sept. 1944–Feb. 1945, Met. Office, Wellington, New Zealand, May 1945) have found that the streamline synoptic models of V. Bjerknes (*Dynamical Meteorology and Hydrology, Pt. II. Kinematics*, 1911) are of help in analysis of the wind observations. There is also a close connection between weather and the singular lines and points of the streamline patterns. The singular lines move relatively slowly and some of them can be considered quasi-stationary systems. A number of empirical rules about the relation of streamlines to isobars are being tried out.

The importance of keeping track, by wind analysis, of cold fronts moving into the tropics is now generally realised both in forecasting convection and the development and motion of tropical storms. See "Polar Trough and Tropical Storms."

Forecasting the development of tropical storms is still a highly uncertain matter. The position and inclination of the I.C.Z. give some indications of likely areas of formation. Regions where the I.C.Z. is deformed and lies within a weak closed isobaric low are regarded with suspicion, especially if other signs such as an increase in convective activity or the passage of a polar trough to the north occur. Microseismic data and reports from hurricane recco aircraft are of great help at such a stage. The use of time meteorograms of pressure in a cyclogenetic area is occasionally of value. A general fall of pressure of about 2 millibars is considered significant within a weak isobaric low on the I.C.Z. in these regions.

Once a storm has formed the forecasting of its path and speed is mainly done by extrapolation from its path history, with the possibility of recurvature being kept more and more in mind as

the storm moves towards latitude 25° . Where adequate upper winds are available they will normally help with direction of motion and especially in forecasting recurvature.

An effort has been made by Moore (*Bull. Amer. Met. Soc.*, Sept. 1946) to apply the results of Bjerknes and Holmboe (*J. Meteorol.*, 1, Sept. 1944) on the critical eccentricities of circular isobaric patterns extending through the entire depth of the atmosphere to the motion of tropical storms. They use two very simple formulæ

$$C_E = -\frac{v_c}{2} + \frac{V_S - V_N}{2} \text{ and } C_N = \frac{V_E - V_W}{2}$$

where C_E and C_N are the components of the storm's motion to east and north respectively, v_c is the critical velocity (given by $4wa \cos \phi \sigma_p^2$ where w , a and $\cos \phi$ have the meanings given earlier), and $2\sigma_p$ is the latitudinal extent of the outer isobaric ring, in which V_S , V_E , V_N and V_W are the wind velocities to south, east, north and west of the centre respectively. Strictly speaking, one should deal with all isobaric rings separately, but the author claims that using only the ring between the outermost two isobars gives the best results. Indeed, he claims a correlation of 0.94 for east to west motion and 0.82 for north to south motion between observed and forecast values. In latitude 15° and for a radius of 5° , i.e. just under 350 miles, $\frac{v_c}{2} \approx 7$ m./sec., so that for the usual westward speeds of

tropical storms, say 5 m./sec., very little eccentricity is required. When the low has recurved and C_E has a positive value of say 10 m./sec., values of $V_S - V_N$ of the order of 30 m./sec. are required. Such eccentricity is rarely observed and the conclusion is that storms moving eastward at 10 m./sec. certainly cannot extend throughout the entire atmosphere. On the face of it, it is surprising that a formula based on this assumption and an assumption of non-divergent flow should give reliable results at all. The choice of the appropriate isobaric channel to use is clearly a somewhat subjective matter and with small intense storms it will often be difficult to obtain representative values of V_S , V_N , V_E and V_W .

Priestley (*Q.J. Roy. Met. Soc.*, 73, 74, 1947) has treated gradient flow in circular isobaric patterns which extend unchanged throughout the atmosphere and derives the following expression for C_E :

$$C_E \approx -\frac{2w}{r_0 \bar{K}_s} \cos \phi + \frac{1}{\bar{K}_s} \left(\frac{\partial G}{\partial s} \right)$$

where r_0 is the radius of the earth, $\bar{K}_s$ is the mean curvature of the

streamlines and $\frac{\partial G}{\partial s}$ is the rate of variation of the gradient wind G round the streamlines.

This expression can be rewritten

$$C_E = -2wr_0 \cos \phi \sigma_p^2 + \frac{1}{K_s} \left(\frac{\partial G}{\partial s} \right)$$

by putting $\frac{1}{K_s} = r_0 \sigma_p$, i.e. supposing isobars and streamlines to coincide. The first term on the right is the quantity $v_c/2$ of Bjerknes and Holmboe's equation while, if the variation from maximum to minimum value of G takes place smoothly round the circumference, the second term can be written $\frac{V_s - V_N}{\pi}$. In a footnote Priestley

says that consideration of what seems to be a second order term leads to a factor 1 in place of π , so that the formula for east to west translation velocity becomes

$$C_E = -\frac{v_c}{2} + V_s - V_N$$

Thus for a given eccentricity westward motion will be smaller than that given by the equation Moore used, as it must be for the necessary divergence to occur to the west of the low.

An attempt has been made in New Zealand (*Report on Extended Range Tropical Forecasting*, Met. Office, Wellington, New Zealand, May 1945) to achieve forecasts for five days ahead. The method was based essentially on the relation between large-scale pressure systems and the strength of the main zonal wind systems—the polar westerlies and tropical easterlies—postulated by C. G. Rossby (*Sears Foundation, Journal of Marine Research*, 2, No. 1, 38–55, June 1939), on the basis of constant vorticity trajectories. The attempt proved unsuccessful, mainly owing to the obscurity of the relation between tropical convection and pressure systems. The authors recommend that research on the pressure fields required to maintain the flow patterns of the tropics should be intensified if the prospect of even five-day forecasts is to be realised.

GENERAL AND PHYSICAL CHEMISTRY. By J. W. SMITH, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic.

TRITIUM (RADIOACTIVE HYDROGEN).—After the detection in 1931 of the hydrogen isotope of mass 2, which was called deuterium (D), it was natural to investigate the possibility of there being isotopes of

higher atomic weight, particularly one of mass 3, which by analogy was called tritium and assigned the symbol T. This problem, however, passed through several phases before the true nature of this isotope was appreciated, so it is of interest to review briefly the published work on the subject.

The first recorded attempt to detect this isotope was that of Lewis and Spedding (*Physical Rev.*, 1933, **43**, 464), who, using a direct spectroscopic method, failed to detect any line corresponding to an atom of mass 3 in the hydrogen released from a rich sample of heavy water. Consequently they inferred that, if tritium were present in ordinary water, its concentration must be less than one part in 6×10^6 as compared with ordinary hydrogen.

Soon afterwards Latimer and Young (*ibid.*, 1933, **44**, 690), whilst studying solutions of hydrochloric and hydrobromic acids in water containing 2-4 per cent. of deuterium by the magneto-optical method, obtained new minima which they ascribed to the hydrogen isotope 3, but their method and the conclusions drawn from it were later subjected to severe criticism.

The same problem was attacked by means of the mass spectrograph by Bleakney and his collaborators. Using a sensitive form of mass spectrograph, into which they introduced deuterium which was almost free from ordinary light hydrogen (protium), and which they evolved from water which had been reduced by electrolysis to one part in 225,000, they observed the presence of charged particles of masses 6, 5 and 4. Of these the first varied in concentration with the square of the pressure, whilst the last two varied directly with the pressure. Hence they inferred that the first was a triatomic molecule DDD^+ , whilst the others were the diatomic DT^+ and DD^+ , respectively. Also, from the plot of the intensity-pressure ratio for these lines they inferred that the tritium content in ordinary water cannot exceed one part in 10^9 (Lozier, Smith, and Bleakney, *Physical Rev.*, 1934, **45**, 655). Following up this work, Selwood, Taylor, Lozier, and Bleakney (*J. Amer. Chem. Soc.*, 1935, **57**, 780) claimed to have increased the concentration of tritium to one part in 10^4 by electrolysing 75 metric tons of water down to 0.5 c.c., i.e. to one part in 150,000,000 of its original volume. Further, from the total amount of tritium evolved during electrolysis, they deduced that the discharge ratio of deuterium to tritium α_T^D is 2.0, in accord with the theoretical predictions which had been made by Eyring (*Scientific Monthly*, 1935, **39**, 415). Since the discharge ratio α_D^H of hydrogen to deuterium is about 6, α_T^H should be about 12, and the relative abundance in ordinary water was calculated as 7 parts in 10^{10} .

At almost the same time Tuve, Hafstad, and Dahl (*Physical Rev.*, 1934, **45**, 840) also thought that they had detected particles of mass 3 whilst using a sample containing 98 per cent. of deuterium and a tube operating at 1100 kilovolts. They estimated the abundance ratio as one in 10^6 as compared with deuterium, in reasonable accord with the previous estimates.

The existence of small amounts of tritium in ordinary hydrogen and hydrogen-containing compounds appeared, therefore, to be quite definitely established when investigations were commenced at Cambridge upon material which was derived from 43.4 kg. of water of average D_2O content of 99.2 per cent., originating from no less than 13,000 tons of ordinary water. This heavy water was electrolysed down to 11 c.c. under circumstances which should favour the concentration of tritium, so that altogether the water had been reduced by electrolysis to one part in more than 10^6 of its original volume. Aston introduced some of the hydrogen derived from this water into his precision mass spectrograph under conditions such that the intensities of the diatomic lines were about 1000 times those of the triatomic lines. His measurements showed, however, that the line corresponding to mass 5 was unquestionably DDH^+ , and that there was no trace of a line corresponding to DT^+ which should have been 1 mm. away on the record. Aston concluded that the concentration of tritium in this water was less than two parts in 10^6 of the deuterium. Disintegration experiments by Kempton and Miss Maasdorp also failed to indicate the presence of tritium in the samples (*cf.* Rutherford, *Nature*, 1937, **140**, 303).

In 1938, after re-examining their results, Sherr, Smith and Bleakney (*Physical Rev.*, 1938, **54**, 388) showed that the previous interpretation of their data was erroneous and that, in fact, there was no evidence of the existence of tritium in ordinary water.

Meanwhile, however, Oliphant, Harteck, and Lord Rutherford (*Nature*, 1934, **133**, 413; *Proc. Roy. Soc.*, 1934, **A134**, 692) had observed that when ammonium chloride, ammonium sulphate, or phosphoric acid, in which the hydrogen had been largely replaced by deuterium, were bombarded with deuterons, two groups of particles were formed in large numbers. The first comprised protons, whilst the second, equal in number to the first, consisted of singly charged particles of 1.6 cm. range. They concluded that the latter group consisted of nuclei of the hydrogen isotope of mass 3.0151, produced through the nuclear reaction



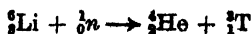
The reason for assigning this mechanism in preference to the direct

one was that, since the deuteron is not broken up by either proton or α -particle bombardment, it is unlikely that it should be broken directly by deuterons. On the other hand, it was thought to be more probable that union might occur to form helium of mass 4.0272 and charge 2, which cannot get rid of its excess energy above that of the ordinary helium nucleus of mass 4.0022, and therefore breaks into two fragments. The yield calculated for a pure deuteron beam hitting a pure deuterium target was calculated to be one in 10^6 at 10^5 volts. It was also observed at the same time that neutrons were also emitted which indicated the simultaneous but less-frequent dissociation of the unstable helium ion to form ${}^3_2\text{He}$ and ${}^1_0\text{n}$.

Following the same reaction by means of a cloud chamber Dee (*Nature*, 1934, 133, 564; *Proc. Roy. Soc.*, 1935, A148, 623) showed that the particles of range 14.3 cm. and 1.6 cm. actually originated from the same disintegration, thereby confirming the interpretation which had been previously suggested.

In an attempted extension of this process, Harnwell, Smyth, van Voorhis, and Kuper (*Physical Rev.*, 1934, 45, 655) thought that they had accumulated an appreciable amount of tritium by running a high-voltage discharge in deuterium at low pressure and passing the canal rays from it into deuterium at higher pressure. Their mass spectrographic analysis indicated that the circulation of the gas for three hours had increased the tritium content from an initial value of one part in 2×10^5 to one part in 5000 parts of the deuterium. Doubtless, however, this measurement was vitiated by the same factors as the other mass spectrographic observations discussed previously.

The fact that deuterium on bombardment with deuterons yields tritium is now, however, firmly established, whilst other nuclear reactions have also been shown to produce it. The earliest of these from an historical point of view, and also the one which has so far proved the most important, is the neutron bombardment of lithium salts, in which the lithium isotope of mass 6 is converted into tritium and helium



This was first detected by Taylor and Goldhaber (*Nature*, 1935, 135, 341), who found that when a photographic plate was soaked in a solution of a lithium salt and then exposed to neutron bombardment from a radon-beryllium source, tracks were obtained with an air length of 6.9 ± 0.2 cm., representing the sum of the ranges of the two resultant particles. Similar results were obtained by Budnizki, Kurtschatov, and Latishev (*Physikal. Z. Sovietunion*, 1935, 7, 474), using the cloud chamber method, when they found that the helium

and tritium nuclei were ejected in opposite directions with air ranges of 2 and 6.5 cm., respectively. It was also shown that the bombardment of beryllium with deuterons leads to the production of tritium through the reaction



(Williams, Haxby, and Shepherd, *Physical Rev.*, 1937, **52**, 1031).

The existence of tritium acquired a new interest in 1939, when Alvarez and Cornog (*Physical Rev.*, 1939, (ii), **56**, 613) found that it was radioactive. They bombarded deuterium gas with deuterons and then passed it into an ionisation chamber connected to an amplifier, when the gas was found to have a β -ray activity of long half-life period. As the radioactive material diffused through hot palladium it was evidently the hydrogen isotope, and the decay process must yield helium isotope of mass 3. As the decay constant was found to be too small for it to be determined from the decay curve, O'Neal and Goldhaber (*ibid.*, 1940, (ii), **58**, 574) deduced it indirectly. They produced a known number of tritium nuclei by exposing lithium for a definite period to a known density of slow neutrons, and then determined the number of nuclei decaying per second by dissolving the lithium in water and introducing a known fraction of the hydrogen evolved into a tube counter. The velocity constant of the decay process was evaluated as 7×10^{-10} per second ± 25 per cent., from which the half-life period was calculated to be 31 ± 8 years.

This radioactivity of tritium has been an invaluable weapon in its study, because the quantities produced are always small, as is evidenced by the fact that it was shown that at a potential of 10 kilovolts the yield of tritium is only about one atom for 10^{13} deuterons passing through a thick screen of deuterium, whilst at 20 kilovolts the yield on a lithium target is about one tritium atom per 10^{14} deuterons (Burhop, *Proc. Camb. Phil. Soc.*, 1936, **32**, 643). It has been of use, for instance, in detecting other nuclear reactions giving rise to tritium and also in investigating its presence or otherwise in ordinary hydrogen. Thus Cornog and Libby found that tritium could also be produced by the bombardment of boron and of nitrogen with neutrons (*Physical Rev.*, 1941, (ii), **59**, 1046). They employed saturated solutions containing an excess of boric acid or of ammonium nitrate, which they bombarded for several thousand milliampere-hours with fast neutrons produced from beryllium with 16.5 Mev. deuterons. After this the water, as vapour in a Geiger counter, showed an activity of about 10^4 counts per gram-mole per second.

Radioactive technique has also been applied by Eidinoff (*J. Chem.*

Physics, 1947, **15**, 416) to a determination of the upper limit of the possible tritium content in ordinary hydrogen. He found that the introduction of deuterium gas of a deuterium content greater than 95 per cent. did not change the counting rate of a Geiger counter by more than 0.03 counts per second from the normal background count of 2.85 per second. Combining this result with a tritium half-life period of thirty-one years he inferred that the tritium atom fraction in his deuterium must be less than 1.5 parts in 10^{13} . From the separation factors of the isotopes he deduced that the tritium content must have been enriched about 15,000 times during the production of the heavy water from ordinary water, so the tritium atom fraction in ordinary water must be less than one part in 10^{17} . This upper limit, which may be still further decreased through measurements on heavy water which has been reduced still more exhaustively by electrolysis is of great interest in view of the fact that tritium is a possible reaction product of nuclear processes taking place in the earth's atmosphere (Libby, *Physical Rev.*, 1946, **69**, 671). It also confirms the fact that the estimates made by earlier workers as a result of mass spectrograph measurements were much too high.

The radioactivity of tritium also permits the determination of the physical properties of this isotope and of its compounds, even though it cannot be isolated in the pure state or, indeed, prepared in any considerable quantity. Thus, for instance, the method of distillation, followed by measurements of the radioactivity of the distillate, has been utilised by Libby and Barter (*J. Chem. Physics*, 1942, **10**, 184) to compare the vapour pressures of the various molecular species present in hydrogen gas. By using hydrogen and deuterium which were about 10^{-9} molar in HT and DT respectively, and assuming hydrogen and deuterium to form almost ideal solutions, so that Henry's law could be applied, they deduced that the vapour pressures of the various kinds of hydrogen molecules at 20.4° K. are as follows: H_2 , 760 mm., D_2 , 256 mm., HD 438 mm., HT 254 ± 16 mm., DT 123 ± 6 mm., and T_2 , 45 ± 10 mm. Therefore, as would be anticipated, the boiling-point of tritium would be appreciably higher than that of deuterium, which in turn is known to be more than 3° higher than that of hydrogen.

Similar technique has also been used to study the equilibrium of tritium hydride with ordinary water $HT + H_2O \rightleftharpoons H_2 + HTO$ over a platinum on charcoal catalyst at $289 - 576^{\circ}$ K., and to derive the thermodynamic relationships for this reaction (Black and Taylor, *J. Chem. Physics*, 1943, **11**, 395). At 20.2° C. the equilibrium constant of this reaction was found to be 6.47 ± 0.12 , as compared with the value of 6.24 at 20° deduced theoretically by Libby.

The experimental precautions necessary, and the degree of accuracy which can be attained in measurements of this type may be illustrated by Eidinoff's preliminary report on his determination of the separation factor for hydrogen and deuterium during the electrolysis of a 10 per cent. solution of sodium hydroxide at 20° C. with a smooth platinum foil cathode (*J. Amer. Chem. Soc.*, 1947, 69, 977). He began with a solution of normal deuterium content and in which the atomic fraction of tritium was about one in 10^{11} . The tritium activity was measured by using a counting gas mixture consisting of hydrogen, alcohol, and argon at partial pressures of 25, 20, and 20 mm., respectively, at 25° in a glass envelope copper gauze Geiger counter of effective counting volume of about 200 c.c. In each electrolytic run about two millimoles of hydrogen were produced, the isotopic concentration of the medium remaining almost constant, so the separation factor was given by the ratio of the relative concentrations of tritium to protium in the solution to those in the gas. The gas evolved was dried through liquid air, and oxygen was removed over a hot nichrome wire before being introduced into the counter. The tritium content of the liquid was determined by evolving the hydrogen quantitatively, in 0.05 gram samples of the solution, over magnesium at 630° in an analytical train "seasoned" at this activity. The tritium activity as measured in counts per min. per mm. of hydrogen gas were

Alkali solution	574, 568 (average 571)
Cathode Gas 1	38.9
" " 2	42.7

These figures lead to separation factors of 14.7 and 13.4, respectively. These values may be compared with the hydrogen-deuterium separation factor of 6-8 at smooth platinum surfaces in both acid and alkaline solutions, and are in remarkable agreement with the value of 2 for the deuterium-tritium factor determined by Bleakney and his co-workers twelve years ago by the mass spectrograph method later regarded as unsound.

Comparison of the separation factor observed with Black and Taylor's equilibrium data shows that the protium to tritium ratio for the hydrogen gas in the electrolysis is slightly greater than twice that corresponding to the thermodynamical equilibrium conditions prevailing at the electrode interface.

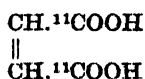
It will be very apparent that the existence of a radioactive isotope of hydrogen has a very important potential value as a weapon for use in following the course of chemical reactions. Since its chemical properties must be almost identical with those of the

ordinary isotope of hydrogen; it should be possible, through its aid, to trace whether hydrogen atoms are transferred during a chemical reaction or not. Such technique has been developed very considerably with the radioactive isotopes of other elements, particularly ^{11}C , in recent years, and such radioactive isotopic indicators have contributed considerably to our knowledge of the mechanisms of organic chemical and metabolic processes.

As compared with ^{11}C and the other usual radioactive tracer isotopes, tritium possesses the advantages that it has the long half-life period of about thirty years, and that samples of high specific radioactivity can be prepared, so that measurements need not be so hurried as in some other cases. On the other hand the chief difficulty with it lies in the fact that it emits only very soft β -radiation of energy not exceeding 10-15 Kev., and hence it must be counted in the gaseous form, either as the element or as water vapour, inside the counter.

Deuterium has also been used as a tracer isotope in reactions involving hydrogen, but, not being radioactive, investigations with its aid demand accurate indirect technique for its determination, for instance precision density measurements on the water produced by the combustion of each product.

The first application of tritium to such problems appears to have been by Allen and Ruben during studies of the synthesis and the oxidation of fumaric acid (*J. Amer. Chem. Soc.*, 1942, **64**, 948). They had converted radioactive carbon (^{11}C) successively into $^{11}\text{CO}_2$, K^{11}CN , and $(\text{CH}_2^{11}\text{CN})_2$, and thence into fumaric acid



in which the carbon of the carboxyl group only was radioactive. On oxidation with permanganate and sulphuric acid the formic acid formed was non-radioactive, showing that the carbon atom of this molecule had originated in one of the CH groups of the fumaric acid. They then showed that when non-radioactive fumaric acid was oxidised in a solution containing HTO and the formic acid produced was neutralised with solid sodium bicarbonate and the water distilled off in a vacuum, the water produced by the combustion of the dry salt showed only about 2 per cent. of the activity of the original radioactive water, whilst the water distilled off had retained its former activity. Therefore the extent to which isotope exchange could have occurred was small, and as this had most probably taken place after the conversion into formic acid it was

inferred that the CH linkage of the fumaric acid remains intact during the oxidation. Hence this permitted the building up of a complete picture of the mechanism of the oxidation process.

Again, in a study of the Menschutkin reaction, Harman, Stewart, and Ruben (*ibid.*, 1942, 64, 2293, 2294) used methyl iodide with the methyl group labelled by the inclusion of radioactive hydrogen. This was prepared by the catalytic reduction of methyl formate with hydrogen containing tritium over a copper oxide-chromic oxide catalyst at 160° C. to give methyl alcohol, which, with hydrogen iodide, then gave methyl iodide. When an excess of triethylamine or of dimethylaniline in alcohol or benzene solution was converted by this methyl iodide into the quaternary ammonium salt, all the radioactivity was found in the salt and none in the residual amine. It was concluded, therefore, that in these cases there is no reaction intermediate which is reversibly formed and in which the iodide ion loses its identity with the methyl group to which it was originally linked. In accordance with this view no transference of methyl groups between tetramethylammonium iodide or chloride and trimethylamine molecules could be detected during eight hours in alcohol solution at room temperature or during three days in benzene suspension at room temperature.

Similar methods were used by Fontana (*ibid.*, 1942, 64, 2503) to study the hydrogen exchange of dyes or dye intermediates in which certain hydrogen atoms should be activated by the presence of amino groups in positions *ortho*- or *para*- to them. In this investigation 1 gram of the compound under investigation was mixed with 1 c.c. of water containing HTO, and, excepting where hydrochlorides were used, 0.01 c.c. of 36N. sulphuric acid, and were heated at 100° for seven days. The water was then distilled off in a vacuum, purified, and its radioactivity measured. From a consideration of the number (n) of replaceable hydrogen atoms, i.e. the number of unoccupied nuclear positions *ortho*- or *para*- to the activating groups, and the observed decrease in the radioactivity of the water, the partition ratio k_T between tritium and protium was deduced. The results were :

	n	nk_T	k_T
Crystal violet	6	9.18	1.53
Methylene blue	4	5.21	1.30
Benzidine	8	2.86	0.33
Benzidine in 50 per cent. alcohol	8	0.90	0.11
Benzidine monohydrochloride in 50 per cent. alcohol	8	0.60	0.08
Benzidine dihydrochloride	8	2.90	0.36

From these data and similar observations on the exchange reaction of deuterium with these compounds it was inferred that the partition ratios of tritium and deuterium is about 1.2 and of tritium and hydrogen about 2.0. The tritium-substituted crystal violet lost only about 2 per cent. of its activity on standing in 2 per cent. aqueous solution for three months, but 60 per cent. was lost in the presence of sulphuric acid in seven days at 100° C. The lower values of k_T found in the case of benzidine and its salts were attributed to the fact that exchange activation occurs through the appearance of a negative charge on the *ortho*- and *para*-positions through the quinonoid resonance forms; in the case of benzidine there will be interference between the resonance of the two groups with the nucleus and this activation effect will be diminished.

Another problem of extreme theoretical and practical interest to both physical and organic chemists is the isomerisation of paraffin hydrocarbons. It is known that, in the presence of a little hydrogen chloride, *n*-butane can be converted into an equilibrium mixture of *n*- and *iso*-butanes over an aluminium chloride catalyst, but the mechanism of this reaction, involving the rupture and re-formation of a C-C linkage has been the subject of some conjecture. Taking advantage of the fact that during the catalytic isomerisation an exchange of hydrogen atoms from both elementary hydrogen and hydrogen chloride with the hydrogen of the butane occurs, Powell and Reid (*J. Amer. Chem. Soc.*, 1945, 67, 1020) have used tritium as a tracer in attempting to determine the steps involved in this reaction. The presence of hydrogen does not alter the progress of the reaction, whilst the presence of hydrogen chloride is essential, so the investigations were carried out with the tritium initially present successively in the hydrogen, hydrogen chloride, and in known positions in the butane molecule. Tritium was derived from deutron-deutron bombardment in a cyclotron and stored in the form of HTO in radioactive water. Radioactive hydrogen chloride was produced by the interaction of electrolytic radioactive hydrogen with elementary chlorine over an activated carbon catalyst at 300° C., and also by dropping radioactive sulphuric acid on sodium chloride. Butanes with selectively placed tritium atoms were prepared by hydrolysis of the corresponding butyl magnesium bromides with acid solutions containing radioactive water. The isomerisation reactions were carried out at atmospheric pressure and 107–128° C., over aluminium chloride on alumina and charcoal catalysts. Butane itself proved to be a good counter gas, and so the resulting butane could be introduced directly into the Geiger counter, whilst the hydrogen could be analysed for tritium by hydrogenating butane with it.

Study of the reaction between HT and butane showed that the isomerised product always contained the higher tritium concentration, and that the amount of exchange was approximately proportional to the amount of isomerisation, but varied with different catalysts. These results were in accord with the supposition that a butane- AlCl_3 complex is formed during the life of which exchange occurs, and which decomposes subsequently into the equilibrium mixture.

Exchange occurred to a much greater extent, however, in the case of the reaction of TCl with butane, and this was taken as evidence that the hydrogen chloride plays an integral part in the isomerisation. As the exchange occurred much more freely after the catalyst had been flushed with hydrogen after its preliminary treatment with inactive hydrogen chloride, it was inferred that the compound HAlCl_4 is probably formed and is the catalytically active substance. The higher tritium content was again found in the isomerisation product and the results accorded with the formation of a complex.

The most significant results, however, came from the study of the reaction between tritium-containing butanes and hydrogen, which showed that, when the tritium was on either the terminal or secondary carbon atoms of the *n*-butane molecule, the exchange was much less than would be predicted from the data for the reverse reaction, and that it is the hydrogen on the secondary carbon atoms which participates principally in the exchange. On the other hand, with isobutane the amount of exchange was greater from a primary than from the tertiary carbon atom, in fact the exchange of hydrogen for tritium on the primary carbon atom was more rapid than the reverse reaction.

These observations were used to build up a tentative mechanism for the isomerisation process. This includes an initial interaction between the β carbon atom and the HAlCl_4 molecules, so that the hydrogen atom of the latter comes into the field of force of this carbon atom, with resultant weakening of its linkage to the α carbon owing to the presence of three hydrogen and two carbon atoms about the same bonding volume. The general instability leads to the original α carbon atom either linking itself to the γ carbon or re-linking to the β carbon atom with introduction of the hydrogen from the HAlCl_4 .

Finally tritium has been applied to the study of at least one problem of biological interest. Norris, Ruben, and Allen (*J. Amer. Chem. Soc.*, 1942, 64, 3037) used it in an attempt to find whether or not chlorophyll participates in photosynthesis as a hydrogen

donor. This theory demands the alternating oxidation and reduction of the pigment, a process involving dehydrogenation and hydrogenation, so if photosynthesis is allowed to proceed for a sufficiently long time in water containing HTO, radioactive chlorophyll containing tritium should be formed if this theory is correct. It was actually found that when chlorophyll was kept in the light in 80 per cent. alcohol containing HTO, less than 5 per cent. of the theoretical exchange of hydrogen for tritium occurred, whilst during the photosynthesis of *Chlorella pyrenoidosa* in water containing HTO no formation of chlorophyll containing tritium could be detected. From this it was inferred that either no isotope separation accompanies the hydrogen donation process, a very improbable state of affairs, since even with deuterium the difference in activation energies is such that there is a considerable tendency for the reaction with one isotope to occur more rapidly than with the other, or else that the hypothesis that chlorophyll acts as a hydrogen donor in photosynthesis should be abandoned.

ORGANIC CHEMISTRY. By A. W. JOHNSON, Ph.D., A.R.C.S., The University, Cambridge.

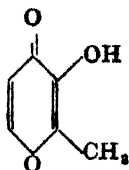
THE CHEMISTRY OF STREPTOMYCIN.—A very great deal of research has been carried out during the last few years on the chemical structures of the antibiotics, penicillin and streptomycin, and, as a result, the constitution of each has been determined, and penicillin has been synthesised. Very little of the considerable literature on penicillin has been published as yet, but the research which has led to the establishment of the structure of streptomycin represents a brilliant achievement of degradative organic chemistry which should serve as a model for many years to come.

Streptomycin was first obtained in 1944 (Schatz, Bugie and Waksman, *Proc. Soc. Exp. Biol. and Med.*, 1944, 66, 55), when it was prepared as a crude concentrate from cultures of *Streptomyces griseus*. It is beyond the scope of this article to discuss the biological properties of streptomycin, but it will be recalled that its considerable therapeutic interest lies in the treatment of infections resistant to penicillin, e.g. typhoid fever and tuberculosis. Isolation of streptomycin from surface culture filtrates was accomplished in the earliest papers (Schatz, Bugie and Waksman, *loc. cit.*; Carter, Clark, Dickman, Loo, Skell and Strong, *J. Biol. Chem.*, 1945, 160, 337; Le Page and Campbell, *ibid.*, 1946, 162, 163) by a preliminary absorption on carbon, followed by elution with methanolic hydrogen chloride and precipitation of the crude streptomycin hydrochloride

with ether. The hydrochloride could be further purified by chromatography on alumina at pH 6.3 and elution with 80 per cent. methanol. The eluate was concentrated *in vacuo* and dried by the lyophile process. Another process for the isolation of streptomycin was given by Kuehl, Peck, Hoffhine, Graber and Folkers (*J. Amer. Chem. Soc.*, 1946, **68**, 1460), which comprised charcoal adsorption, elution with methanolic formic acid, precipitation as picrate, conversion to hydrochloride, chromatography and finally conversion to the helianthate.

In the course of this work on purification, several crystalline derivatives of streptomycin were obtained, *e.g.* hydrochloride, hydrobromide, sulphate, double salt of the hydrochloride with calcium chloride, reineckate, helianthate, *p*(2-hydroxy-1-naphthylazo)-benzenesulphonate (with Orange II) (Carter *et al.*, *loc. cit.*; Kuehl *et al.*, *loc. cit.*; Fried and Wintersteiner, *Science*, 1945, **101**, 613; 1946, **104**, 273; Kuehl, Peck, Walti and Folkers, *ibid.*, 1945, **102**, 34; *J. Amer. Chem. Soc.*, 1945, **67**, 1866), and from the analysis of these derivatives the formula of streptomycin was finally fixed as $C_{21}H_{39}O_{12}N_7$.

General observations on the reactions of streptomycin had included the important discoveries that it was deactivated by the action of carbonyl reagents (Donovick, Rake and Fried, *J. Biol. Chem.*, 1946, **164**, 173) and indeed formed carbonyl derivatives, *e.g.* oxime-hydrochloride and semicarbazone-hydrochloride (Brink, Kuehl, Flynn and Folkers, *J. Amer. Chem. Soc.*, 1946, **68**, 2557), and that it gave a positive Sakaguchi test for guanidine groups (Carter *et al.*, *loc. cit.*). Alkaline degradation of streptomycin with *N* sodium hydroxide at 100° for 3 minutes gave maltol (I) by a molecular rearrangement (Schenk and Spielmann, *J. Amer. Chem. Soc.*, 1945, **67**, 2276).



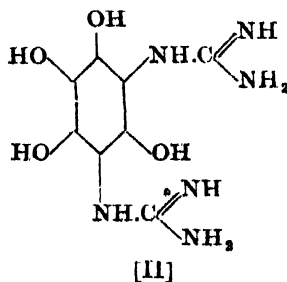
[I]

Furthermore, the molecule was shown to contain one C-methyl group (Polglase, Wolfram *et al.*, *J. Amer. Chem. Soc.*, 1946, **68**, 2120, 2747).

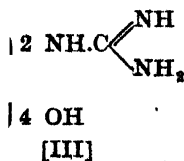
A result of prime importance was obtained from the acid hydrolysis of streptomycin hydrochloride in methanol solution, when the molecule was split into an optically inactive hydroxylated base,

streptidine, and a nitrogen-containing disaccharide-like molecule, methyl streptobiosaminide dimethylacetal hydrochloride (Brink, Kuehl, Folkers *et al.*, *Science*, 1945, **102**, 506; *J. Amer. Chem. Soc.* 1946, **68**, 2557; Peck, Graber, Walti, Peel, Hoffhine and Folkers, *ibid.*, 1946, **68**, 29). This indicated that streptomycin probably contained these two portions attached by a glycosidic linkage.

The Structure of Streptidine.—Streptidine could also be obtained as the crystalline sulphate by cleavage of streptomycin in dilute aqueous or methanolic sulphuric acid, and the base was characterized by the formation of a number of salts, the analyses of which led to the formula $C_8H_{18}O_4N_6$ for streptidine. Further investigation showed that it contained one or more hydroxyl groups, two basic groups, 8 acetyltable $—OH$ or $—NH$ groups, but no $—NH_2$, $—COOH$, $—OCH_3$ or $>CO$ groups. Following up on this evidence (Peck, Hoffhine, Peel, Graber, Holly, Mozingo and Folkers, *J. Amer. Chem. Soc.*, 1946, **68**, 776), the structure of streptidine was established as one of the meso forms of 1:3-diguanido-2:4:5:6-tetrahydroxycyclohexane (II):

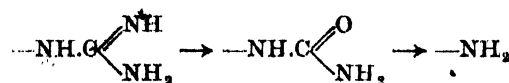


Oxidation with permanganate gave guanidine and the yield showed that streptidine contained two guanido groups, which accounted for 4 of the 8 acetyltable groups and all of the nitrogen atoms. The remaining 4 acetyltable groups, therefore, appeared to be hydroxy groups. As streptidine contained no double bonds the formula indicated the presence of an alicyclic ring and the formula thus became (III)

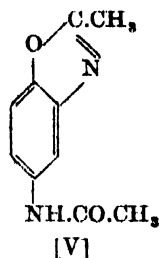
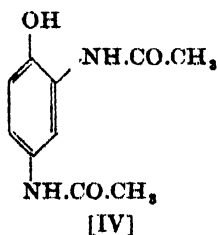


Further information was obtained by alkaline hydrolysis of streptidine, when it was converted successively to the neutral strepture

and then the basic streptamine, and the analysis of derivatives indicated that the changes had involved only the guanido groups, i.e.



In the quantitative periodate oxidation of diacetylstreptamine, two equivalents of oxygen were taken up, indicating that the amino groups could not be in the 1 : 2 position, as this compound would have been expected to have consumed 3 equivalents of oxygen and, furthermore, streptamine did not yield a cyclic ureide on treatment with phosgene. These conclusions had also been reached by other workers (Carter, Clark, Dickman, Loo, Meek, Skell, Strong, Alberi, Bartz, Binkley, Crooks, Hooper and Rebstock, *Science*, 1946, 103, 52; Fried, Boyack and Wintersteiner, *J. Biol. Chem.*, 1946, 162, 391). The elimination of the 1 : 4- in favour of the 1 : 3-diamino-2 : 4 : 5 : 6-tetrahydroxycyclohexane structure for streptamine was the result of the pyrolysis of hexa-acetylstreptamine at 350° for one hour, when 3.5 moles of acetic acid was produced and two crystalline products obtained, one of which proved to be 2 : 4-diacetylaminophenol (iv) and the other 5-acetylamino-2-methylbenzoxazole (v) :



The yield of the crystalline mixture of these compounds was about 95 per cent. theoretical and, on the basis of this evidence, the accepted structure (II) was advanced for streptidine.

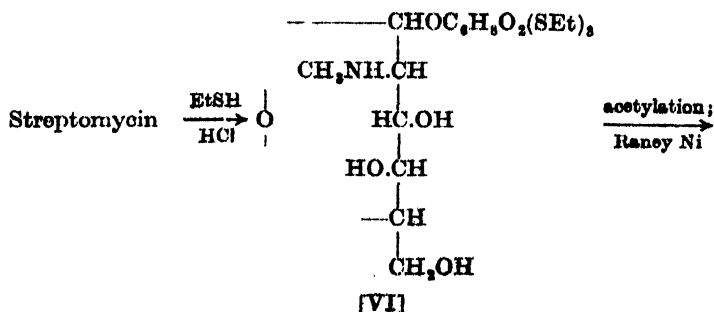
The Structure of Streptobiosamine.—Experiments on the structure of streptobiosamine, the other portion of the streptomycin molecule, were being carried out simultaneously. Streptobiosamine contains one atom of nitrogen and is very rich in oxygen :

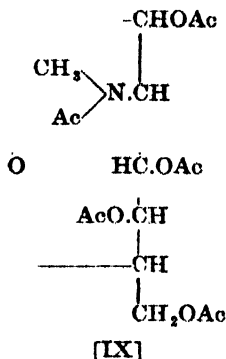
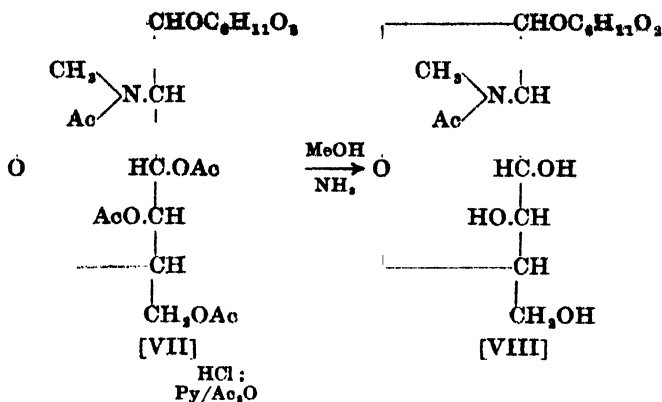


The compound obtained by the hydrolysis of streptomycin with methanolic hydrogen chloride was methyl streptobiosaminide dimethylacetal hydrochloride, $\text{C}_{11}\text{H}_{22}\text{O}_7\text{N}(\text{OCH}_3)_2\cdot\text{HCl}$ (in which a carbonyl group has been converted to its dimethylacetal and the

third methoxy group is a methyl glycoside), which contained a C-methyl group and gave a tetra-acetyl derivative in which three acetyl groups were attached to oxygen and one to nitrogen. As primary amino groups were absent, it was concluded that the nitrogen was present as a secondary amino group, and this was confirmed by alkaline hydrolysis when methylamine was obtained. The presence of a methyl group on the nitrogen signified a residual C_{12} disaccharide structure, in which the methylamino group was probably attached to the 2-position of one hexose fragment, in common with all other known naturally occurring amino-sugars.

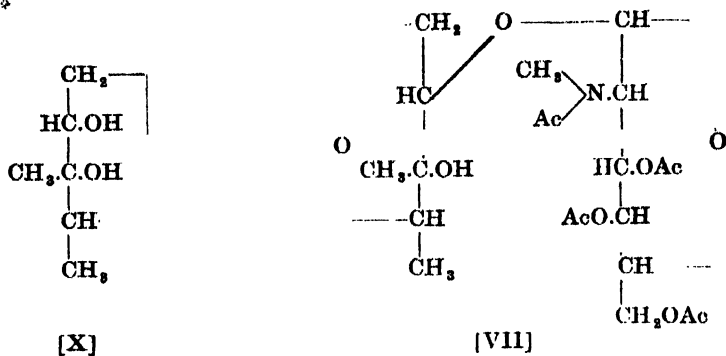
Acid hydrolysis of methyl streptobiosaminide dimethylacetal hydrochloride, followed by acetylation, yielded the pentacetyl derivative of N-methyl-*l*-glucosamine (IX), and thus revealed the nature of one of the hexose portions of streptobiosamine (Kuehl, Flynn, Holly, Mazingo and Folkers, *J. Amer. Chem. Soc.*, 1946, **68**, 536). Degradation of streptomycin with ethyl mercaptan and hydrogen chloride gave crystalline ethyl thiostreptobiosaminide diethylmercaptal hydrochloride (VI) in 80 per cent. yield, which was the sulphur analogue of methyl streptobiosaminide dimethylacetal hydrochloride, as shown by the conversion of the latter to (VI) by the action of ethyl mercaptan and hydrogen chloride (Kuehl, Flynn, Brink and Folkers, *J. Amer. Chem. Soc.*, 1946, **68**, 2096). On refluxing the tetra-acetyl derivative of (VI) with Raney nickel in 70 per cent. ethanol, a crystalline sulphur-free compound, $C_{18}H_{21}O_7N(CO.CH_3)_4$, was obtained, in which each -SEt group had been replaced by hydrogen. As two oxygen atoms had been removed from the original disaccharide by these reactions, the product was named tetra-acetylbisdesoxystreptobiosamine (VII). Partial deacetylation of (VII) with saturated methanolic ammonia gave N-acetylbisdesoxystreptobiosamine (VIII), which did not reduce Fehling's solution, and hence the linkage between the 2 hexose portions of the disaccharide was considered as involving C_1 of the N-methyl-*l*-glucosamine :



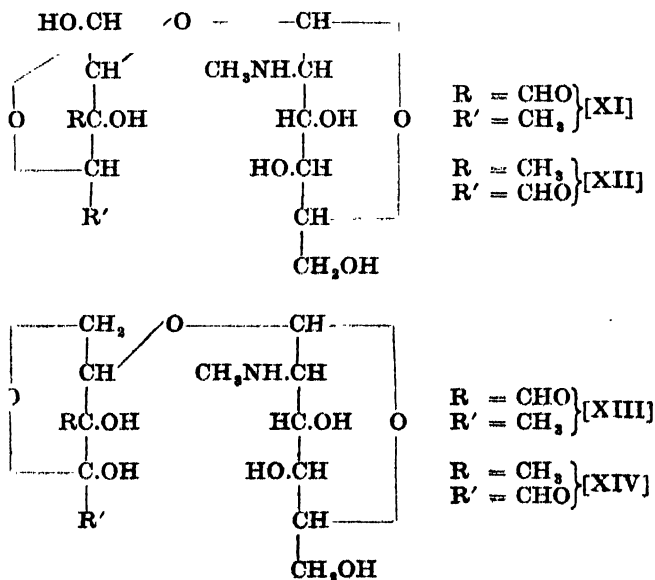


Periodate oxidation of (VIII) gave no formaldehyde or formic acid and hence the N-Me-*l*-glucosamine contains a pyranose ring. Hydrolysis of (VII) with concentrated hydrochloric acid gave N-methyl-*l*-glucosamine, which established that the two carbonyl groups which react with the ethyl mercaptan are in the streptose (nitrogen-free hexose) portion of streptobiosamine and also that the glycosidic linkage between streptidine and streptobiosamine is through the streptose moiety. Essentially similar conclusions were reached by Hooper, Klemm, Polglase and Wolfram (*J. Amer. Chem. Soc.*, 1946, 68, 2120). The complete structure of (VII) was obtained from the results of acid hydrolysis, when N-methyl-*l*-glucosamine was obtained together with bisdesoxystreptose (X), C₈H₁₀O₈, which proved to be 3:4-dihydroxy-2:3-dimethyltetrahydrofuran. Oxidation with periodate, followed by acid hydrolysis, gave diacetyl, and furthermore bisdesoxystreptose formed an acidic complex with boric acid, indicating that the hydroxy groups are in the *cis* position

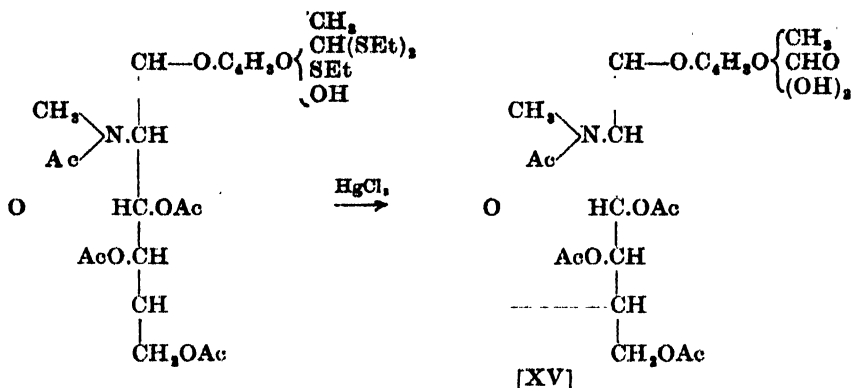
(Brink, Kuehl, Flynn and Folkers, *J. Amer. Chem. Soc.*, 1946, **68**, 2405) :



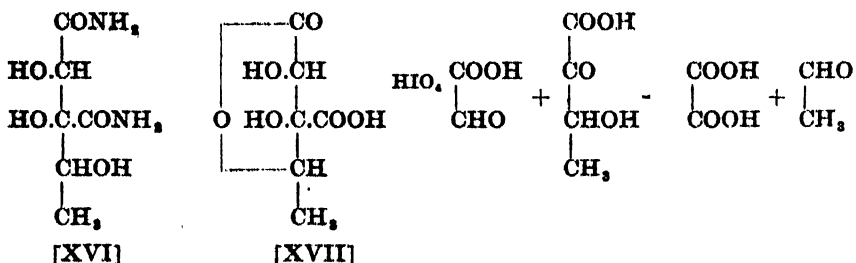
On the basis of this and earlier work, four possible formulæ were advanced for streptobiosamine, viz. (XI, XII, XIII and XIV) :



Further evidence led to the establishment of (XI) as the correct formulation. It was found that the ethylthio-groups of ethyl thiostreptobiosaminide diethylmercaptal hydrochloride (vi) could be removed by treatment with mercuric chloride in the presence of strontium carbonate to give tetra-acetylstreptobiosamine (xv), (Kuehl, Flynn, Brink and Folkers, *J. Amer. Chem. Soc.*, 1946, **68**, 2679) :



(xv), on oxidation with bromine and re-acetylation, yielded pentacetylstreptobiosamic acid monolactone, $\text{C}_{18}\text{H}_{21}\text{O}_{10}\text{Nac}_5$, which on acid hydrolysis gave streptosonic acid monolactone, $\text{C}_8\text{H}_8\text{O}_6$, a crystalline compound derived from the streptose portion of the molecule, together with N-methyl-l-glucosamine. Streptosonic acid monolactone was shown to be the monolactone of a dibasic acid and to contain one C-methyl group, and a quantitative periodate oxidation indicated the presence of three adjacent hydroxyl groups. These properties eliminate formulæ (xiii) and (xiv) for streptobiosamine, as neither of these could yield a C_8 dibasic acid from the streptose portion. Oxidation of the diamide of streptosonic acid with periodate failed to yield any volatile acids, and this result, coupled with the above degradations and the accepted structure of bisdesoxystreptose, led to the formulation of the diamide as (xvi), as all other possibilities had been eliminated. This was confirmed by the periodate oxidation of streptosonic acid monolactone (xvii), when acetaldehyde, glyoxylic acid and oxalic acid were obtained.



Only formula (xi) for streptobiosamine is consistent with the formation of streptosonic acid of the above structure.

Further supporting evidence for the structure of streptose came from the work of Fried, Walz and Wintersteiner (*J. Amer. Chem. Soc.*, 1946, **68**, 2746), who showed that the action of phenylhydrazine

amine. Comparison of the quantitative periodate oxidations of this compound and N : N-dibenzoylstreptamine showed that one and two equivalents of periodate were taken up respectively, a result which can only be interpreted if the attachment of streptidine to streptobiosamine is at C₄ in streptidine. The complete formula for streptomycin is, therefore, shown at the foot of previous page. Definite indications of the existence of compounds structurally related to streptomycin in certain concentrates have been obtained (Fried and Titus, *J. Biol. Chem.*, 1947, **168**, 391, 393).

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

REGIONAL AND TECTONIC GEOLOGY.—C. E. Wegmann of Schaffhausen and his collaborators have published an excellent series of papers on the geology of Greenland, largely based on their own researches. There are twenty contributions in all (230 pp.), of which a selection of the most important is given below : C. E. Wegmann, Einleitung zur Vortragsreihe über die Geologie von Grönland, *Mitt. Naturf. Ges. Schaffhausen (Schweiz)*, XVI, Jahrg. 1940, 29–46 ; C. E. Wegmann, Übersicht über das Kaledonikum Ostgrönlands, *ibid.*, 82–104 ; H. Butler, Übersicht der devonischen Bildungen nördlich des Davy-Sundes in Ostgrönlands, *ibid.*, 105–31 ; A. Rittmann, Der jungpaläozoische Vulkanismus in Ostgrönland, *ibid.*, 146–51 ; C. E. Wegmann, Übersicht über die Geologie Südgrönlands, *ibid.*, 188–212. The comprehensive memoir on “ Die Triasablagerungen von Ostgrönland,” by H. Stauber (*Medd. om Grönland*, **132**, No. 1, 1942, 325 pp.) also comes from the Swiss school.

In a memoir on “ The Structure of the Keiser Franz Josefs Fjord Region, Northeast Greenland,” N. E. Odell (*Medd. om Grönland*, **119**, No. 6, 1939, 52 pp.) states that a continuous stratigraphical succession extends downwards from the low-grade metamorphic Petermann Series of quartzites and slates into the high-grade rocks of the so-called Central Metamorphic Complex. The Petermann Series was found to be strongly overfolded and even overthrust, showing that the Caledonian deformation extended much farther west than had hitherto been thought. Granite and some basic sheet intrusions accompanied the Caledonian folding, but were not responsible for more than local metamorphism. The author thinks that Backlund's theory of wholesale granitisation and syntexis cannot be applied in general to this region.

The memoir “ A Geological Reconnaissance of the Southern Part of the Svartenhuk Peninsula, West Greenland,” by A. Rosenkrantz,

A. Noe-Nygaard, H. Gry, S. Munck, and D. Laursen (*Medd. om Grönland*, 135, No. 3, 1942, 72 pp.), shows that the region consists of a basement of Pre-Cambrian phyllites, followed by marine Cretaceous and Tertiary sediments, a formation of basaltic breccias, and finally the Tertiary plateau basalts. The Cretaceous and Early Tertiary sediments, a kilometre or more thick, were deposited upon the folded Pre-Cambrian basement, then faulted and eroded, so that an uneven surface was produced upon which the basaltic breccia was deposited sub-aqueously, followed by outpourings of basaltic lavas several kilometres thick. The final event was a tilting to the south-west.

D. N. Wadia's Presidential Address to the 29th Indian Science Congress, Baroda, 1942 (21 pp.), dealt with "The Making of India" as a review of some aspects of the geological structure of the sub-continent. He emphasised the three main structural divisions: the Himalayan belt of folded younger rocks; the rigid Archæan shield of the Deccan; and the intervening North Indian plains which cover a trough produced in front (south of) the Himalayas by the interaction of the Himalayan and Deccan Blocks. Dr. Wadia is evidently not impressed by the Wegener theory which regards Peninsular India as a north-eastward-drifting fragment of Africa which, by its impact with Asia, has formed the Himalayan folds. He does not regard the Deccan as a "foreign" block, but as being always integral and adjacent to the greatly contrasted Himalayan Block. This Address is most valuable, not only for Indian, but also for foreign geologists who will find therein the best modern conspectus of the structure of India.

C. S. Pichamuthu's Presidential Address to the Section of Geology and Geography, 34th Indian Science Congress, Delhi, 1947 (16 pp.), dealt with "Some Aspects of Dharwar Geology with special reference to Mysore State." The Dharwars represent the most ancient Pre-Cambrian and, like the oldest Archæan rocks of most areas, consist dominantly of acid and basic volcanic rocks. In the latter pillow-structures have been recognised, and there are thin sedimentary intercalations. Dr. Pichamuthu deals with the sedimentation, cyclic or otherwise, volcanism, tectonics, climate and life of Dharwar times; but much more research is needed before satisfactory conclusions can be reached.

"The Archæan Complex of Mysore" is dealt with in a comprehensive memoir by B. Rama Rao (*Mysore Geol. Dept. Bull. No. 17*, 1940, 101 pp.). Granites and gneisses, including charnockites, make up the greater part of the complex. The so-called "Dharwar System" of volcanic rocks, schists, deformed sediments, and basic

and ultrabasic intrusions, forms three large separate belts, as well as smaller strips enveloped in the granites and gneisses. It is suggested that the "Dharwar facies" of southern India is to be correlated with the "Older Metamorphics" of Bihar and Orissa, and to the Aravalli system of Rajputana.

Under the leadership of Professor M. P. Billings elucidation of the complicated sedimentary, igneous and metamorphic geology of New Hampshire proceeds apace. For the igneous geology of this region see SCIENCE PROGRESS, April 1947, 308-9. Recent papers dealing mainly with the sedimentary and metamorphic geology are the following:

C. A. Chapman, "Geology of the Mascoma Quadrangle, New Hampshire" (*Bull. Geol. Soc. Amer.*, 50, 1939, 127-80). Half of the Mascoma Quadrangle is composed of middle- and high-grade metamorphic rocks derived from sediments and volcanics which range from Middle Ordovician to Lower Devonian. The other half consists of igneous gneisses belonging to the Oliverian and New Hampshire magma series. The outstanding structural features are the domes of metamorphic rocks around cores consisting of laccolithic igneous bodies of the Oliverian Series. There have been two periods of metamorphism, one imposed by the igneous intrusions, and a later schistosity impressed on sediments and igneous rocks alike.

M. P. Billings, "Structure and Metamorphism in the Mount Washington Area, New Hampshire" (*Bull. Geol. Soc. Amer.*, 52, 1941, 863-936). M. P. Billings *et alia*, "Geology of the Mount Washington Quadrangle, New Hampshire" (*ibid.*, 57, 1946, 261-74). The geology of the Mount Washington Quadrangle follows the same general pattern as the above. High-grade metamorphic rocks belong to four stratigraphical units ranging from Ordovician to Devonian. The area lies on the south-eastern flank of a great north-east-trending dome raised by the intrusion of an Oliverian magmatic body. The second reference (1946) is a description of a new geological map of the area, the appearance of which has been delayed by the war.

F. C. Kruger, "Structure and Metamorphism of the Bellows Falls Quadrangle of New Hampshire" (*Bull. Geol. Soc. Amer.*, 57, 1946, 161-206). J. B. Hadley, "Stratigraphy, Structure and Petrology of the Mount Cube Area, New Hampshire" (*ibid.*, 53, 1942, 113-76).

The paper by P. Evans and W. Crompton on "Geological Factors in Gravity Interpretation illustrated by evidence from India and Burma" (*Quart. Journ. Geol. Soc.*, CII, Pt. 3, 1946, 211-49) is the result of an extensive gravity survey of Burma and north-eastern

India in which gravity anomalies were determined at over 6000 stations. The attempt was then made to resolve the anomalies into the portions due to known geological causes, and the residual (regional) portions due to deep-seated variations in density, allowing especially for the effects of relatively light sedimentary rocks in diminishing the observed values of gravity.

A pronounced gravity minimum was found to follow the lines of maximum uplift between Burma, Assam and Bengal, and a gravity maximum almost coincided with the nearly parallel volcanic belt of Burma. Both these lines can be correlated with those determined by Vening Meinesz in the Netherland East Indies. The great deltas are areas of excess gravity. All these major regional anomalies are attributable to variations in the depths to the basic and ultrabasic crustal zones.

In a paper on "Different Types of Island-Arcs in the Pacific," J. H. F. Umbgrove (*Geogr. Journ.*, CVI, 1945, 198-209) distinguishes three types of island arcs, double, pseudo-single, and single. He reviews favourably P. Lake's theory of a deep-reaching thrust-plane, or zone of thrust-planes, dipping continent-ward from the Pacific, to account for the shapes of the arcs. These probably reach a depth of several hundred miles; and the prevalence of deep-focus earthquakes in these localities serves to confirm this view. The narrow deeps along the outer convex margin of the arcs, and the deep basins on their inner concave sides, are brought into consonance with the theory. It follows from the considerations advanced that the different aspects presented by such mountain belts as the Alps, Himalayas, the structural belts of Burma, and the island festoons of the East Indies, do not represent evolutionary stages in a continuous process of mountain-building.

The same author has also discussed "The Origin of Continents and Ocean Floors" (*Journ. Geol.*, LIV, 1946, 169-78). Starting from the concept of permanence three groups of theories are investigated: (1) theories postulating a primary surface layer of basaltic composition; (2) theories starting with the resonance theory of the moon's origin and considering the result of such an event on a sialic layer of continental thickness; and (3) theories based on the effects of convection currents on a surface layer of sialic composition. The author favours the third of these groups. He believes that continents and oceans arise from a world-encircling sialic layer by a process of thickening in some places, thinning and disruption in others, due to a combination of folding and drifting induced by the action of rising and sinking convection currents in the earth's interior. This view is reinforced, and the tetrahedral aspect of the earth

explained, by a geophysical investigation of Vening Meinesz's on the probable distribution and effects of convection currents.

The "enigma of the Pacific" is discussed by A. L. du Toit in a paper on "Observations on the Evolution of the Pacific Ocean" (*Proc. Sixth Pacific Sci. Congr.*, 1939, 175-83). He poses the question whether this ocean conceals the sites of various Palaeozoic and Mesozoic land masses or land bridges, which were repeatedly built up and destroyed (Gregory), or whether it was, during the Mesozoic, a far larger basin upon which the encircling lands have encroached by a process of continental drift. Du Toit favours the latter alternative on a review of the tectonic and geophysical evidence.

H. H. Hess has described and discussed the curious flat-topped peaks which, to the number of 160, have been discovered by soundings in the Pacific basin between Hawaii and the Marianas ("Drowned Ancient Islands of the Pacific Basin," *Amer. Journ. Sci.*, 244, 1946, 772-91). They are oval in plan, their slopes suggest volcanic cones, and they are truncated by a level surface which now stands at about 4500 ft. below sea level. Relations to atolls indicate that the surface is older than the atoll formation. A tentative hypothesis is advanced that "guyots," as they have been called, are extremely old, and represent marine planation surfaces within a Pre-Cambrian ocean in which reef-building organisms did not exist. The present depth of the surfaces might be accounted for by the relative rise of the ocean surface due to sedimentation.

A. L. du Toit adduces strong evidence for "The Origin of the Atlantic-Arctic Ocean" by continental drift (*Geol. Rundsch.*, XXX, 1938, 138-47). It was initiated in the Mesozoic and progressively enlarged during the Tertiary. Its outlines were determined by north-east and north-west tensional rifting within a zone extending from Alaska to Antarctica. Widespread volcanicity accompanied the crustal stretching. The Mid-Atlantic Rise is recent, and has an isostatic basis. The oceans are bordered by fault-line coasts and down-warped shores that show the marginal entrenched, terrestrially evolved drainages known as "submarine canyons."

The last sentence of the previous paragraph gives the gist of A. L. du Toit's "Hypothesis of Submarine Canyons" (*Geol. Mag.*, LXXVII, 1940, 395-404), about which many varying and contradictory theories have been put forward. The orogenic theory favoured by the author involves elevation of the continental margins by 4000 ft., and their subsequent downwarping and trans-section. The canyons are held to be due to great rivers cutting through the raised margins in the early stages of the uplift. When downwarping occurred they were submerged and became submarine canyons.

In a paper "Zur Tektonik des Atlantischen Ozeans," R. A. Sonder (*Geol. Rundsch.*, XXX, 1939, 28-57) suggests that the continents probably possess smaller rigidity than the oceanic segments. This leads to the view that the continents represent the weak places in the earth's crust, and are the end-products of silicification in a relatively basic crust in consequence of tectonic and magmatic revolutions. The great significance of the different composition of the continental and oceanic segments in relation to the idea of permanence is emphasised. The view that the oceans are due to the breaking-up of larger continental masses is rejected. The existence of former land bridges is only admitted for special cases. Further considerations concern the character of oceanic tectonics. The oceans possess a block-fracture tectonic producing elongated horst structures as opposed to the tendency towards graben formation on the continents. This is a paradoxical view which is not easy to reconcile with obvious features of oceans and continents.

In a paper "On Rhythms in the History of the Earth," J. H. F. Umbgrove (*Geol. Mag.*, LXXVI, 1939, 116-29) briefly discusses local rhythms, the problem of worldwide transgressions and regressions, the pulsating rhythm of the oceans which is connected with epochs of diastrophism and magmatic cycles, and the origin of deep sea basins. He embodies his findings in a table which shows the rhythmic movements of sea level as a graph which is correlated with orogenic periods and with magmatic phases.

In his paper on "Deformation of the Earth's Crust," W. H. Bucher (*Bull. Geol. Soc. Amer.*, 50, 1939, 421-32) discusses basins and swells, geosynclines and orogenic zones, ocean basins and continents, and seeks to unify all these phenomena under one all-embracing hypothesis of "pulsation." The pulses are shown by the emergence and rejuvenation of continental surfaces in place of an earlier submergence and peneplanation; mountain building on the sites of former geosynclines; and evidences of compression on oceanic floors which carry signs of tensional origin. These contrasts are explainable if it is assumed that the intensity of compression in the crust does not merely change from greater to less or vice versa, but that at times it changes sign and becomes crustal tension. This means that the deformation of the crust proceeds in a great geodynamic pulse, which Bucher elaborates in the rest of the paper.

L. Picard has provided a valuable "Outline of the Tectonics of the Earth, with special emphasis upon Africa" (*Bull. Geol. Dept. Hebrew Univ. Jerusalem*, II, No. 3-4, 1939, 66 pp.), in which the structure of continents, oceans and mountain-belts, etc., is traced back to the effects of orogenesis, epeirogenesis and *tafrogenesis*

(formation of graben, horsts, etc.). Special attention is given to the hypothesis that the fold-zones are arranged concentrically around the stable Pre-Cambrian shields in succession of age, so that the youngest zones are most remote from the stable block. Tafrogenetic phenomena occur on the margins of the continents and in the connecting archipelagos. The Pre-Cambrian nuclei of the continents and the Palæozoic orogens welded on to them are now dominated by epeirogenic uplift and subsidence. The supposed differences between the Laurasian and Gondwanian crustal segments are believed to be due more to inadequate data, especially in the Gondwanian continents, than to real differences in structure and geological history.

In a paper on "Strength of the Earth," M. K. Hubbert (*Bull. Amer. Assoc. Petroleum Geologists*, 29, 1945, 1630-53) discusses the long-standing problem of how an earth whose exterior is composed of hard rocks can have undergone repeated deformations as if it were composed of weak and plastic materials. Tidal and seismic evidence shows that the earth reacts to stresses of brief duration with a rigidity of the order of that of steel, while orogenic and gravitational phenomena suggest that the earth is non-rigid and very weak. Hubbert shows that this conflict of data is more apparent than real, because the time and magnitude (scale) factors have been left out of account. When these have been evaluated "we discover that the behaviour, in response to gravitational stresses in geological space and time, of an earth composed of hard rocks, and possessing the viscosity appropriate to the Post-Glacial uplifts in Europe and North America, would be very similar to that of the weak plastic materials and viscous liquids of our everyday experience."

BOTANY. By PROFESSOR W. H. PEARSALL, D.Sc., F.R.S., University College, London.

THE perennial problems presented by the details of the fine structures of plant cell walls continue to occupy the attention of biophysicists. Of necessity, the most promising materials for study are often those, like cotton hairs, in which the wall material can be isolated from the complexities of the living cell with some hope that its fine structure can be examined. Another cell-wall type which has recently come in for noteworthy attention is the woody tissue of coniferous trees, the attractive feature here being the uniformity in size and structure of the individual tracheids of which the woody tissue is composed—as well as the scarcity of living cells.

The difficulty in dealing with material of this type arises from the fact that some of the most useful modern methods, that for example of the X-ray diagrams, are of necessity concerned with the composite results of photographs taken through a variety of cells. It is true that in the conifer wood they are mostly oriented in one general direction, but this is not necessarily or invariably the case. The interpretation of the resultant data is thus even more complex than is normal in biological material. Some of the earlier X-ray photographs of this type of material, such as those published by I. W. Bailey and E. E. Berkeley (*Amer. J. Bot.*, **29**, 231, 1942) have thus been criticised as attempting to interpret the details in too simple a manner. More recently, R. D. Preston has given a detailed analysis of the complex geometrical consideration necessary in the interpretation of X-ray diagrams of this type (*Proc. Roy. Soc.*, **B133**, 327, 1946) and in addition an account of the heterogeneity which characterises the swelling and the optical properties of the walls. It appears from this, for example, that, when the walls of a conifer tracheid are swollen in strong acid, the innermost layers may swell considerably, so much so that the lumen nearly disappears, while the comparatively slight increase in the diameter of the whole cell (as seen for example in transverse section) shows that the outermost wall layer possesses a resistance to swelling which limits the expansion of the inner layers. In this one way, among others, the existence of an outer layer of different physical properties is demonstrated. It is suggested that the change in crystalline structure most likely to explain this difference might be one in the angular dispersion of the cellulose chains.

In a subsequent paper by the same author (*Proc. Roy. Soc.*, **B134**, 202, 1947), work is described in which the tracheids of *Pinus insignis* were macerated to remove lignin by alternate chlorination and treatment with hot sodium bisulphate. They were then shaken with glass beads until a large number of damaged and fractured tracheids resulted. Among these it was at times possible to distinguish an outer (and thinner) lamella of the tracheid wall and an inner and thicker one. The outer layer was the primary wall of the cell, composed of cellulose chains lying at an angle of approximately 11° to the transverse (horizontal) plane of the cell. The secondary or inner wall had cellulose chains in steep spirals (average angle 79°) more or less uniform throughout the whole thickness. No significant correlation could be detected between the two directions of orientation. In addition, the cellulose of the primary wall, as extracted from conifer cambium, appears to differ from typical secondary wall cellulose in consisting of micelles of a some-

what smaller size as well as in differing orientation. These units could run transversely and be linked by chains running across the inter-micellar spaces. Their separation on cell expansion would be then conceived of as separating cellulose "rings"—leaving room for the deposition of additional transversely oriented micelles in the intervening spaces.

The simple and intelligible conception of the structure of the tracheid wall which results from this work is thus one in which the primary wall of the cambium initial is apparently laid down in almost transverse chains, while, when the secondary wall is deposited, the mechanism controlling the deposition of these chains either alters in direction or in character.

It is suggested that, in order to understand the mechanism of these chain orientations in the cell wall, it will be necessary to consider the states in which the cytoplasmic surface can exist, and it would seem that this may involve at least the consideration of the cytoplasmic changes which occur when vacuolation ceases—this being probably the time when secondary wall deposition starts. Many possible factors may be involved here. Some of them, like the direction of protoplasmic streaming that originally formed a fruitful basis for speculation in dealing with wall structures, seem now to have been ruled out by modern work. The cessation of cell division, possibly with resultant changes in the spiral arrangement of the protoplasm, is now suggested as a possible alternative.

Among the less accessible papers from Scandinavia which have reached England since the war is one by Börje Åberg dealing with the morphological responses of plants to light (*Symbol. Bot. Upsalienses*, VIII, 1, pp. 1-189, 1943). This memoir deals firstly with the responses of plants like Tomato, Peas and Lettuce and the aquatic *Lobelia dortmanna* to light of varying intensities and diverse quality, ranging from daylight to that of mercury, sodium and neon lamps. A critical analysis of the results of the experiments is considered to show that the responses to light are of two main sorts, those produced by smaller amounts of energy and those due to larger intensities and longer durations. The morphological effects of light appear to be strictly localised to the illuminated region, which indicates that they are primarily dependent on light effects upon the physico-chemical state of the protoplasm and the cell walls, and not on the formation of transportable substances. There is also considered to be ample evidence for the view that light reduces the sensitivity of cells to auxin, but, at the same time, the effect of light upon cell elongation is to a considerable extent conditioned by other factors than the auxin concentration.

One of these factors may be sought in the available quantity of carbohydrates. A very low quantity can, as a matter of course, limit both growth and cell extension, but there are also reasons to suppose that, at least in the shoot, the quantity may exceed the optimum and cause a retardation of cell elongation. The high carbohydrate quantity is accompanied by a high osmotical value, which causes a dehydration of the cell wall and a decrease in its plastic extensibility. The same result may also be consequent upon an accelerated deposition of cell-wall material. The effects of restricted water supply and low temperature are to some extent identical with those of high light intensity, in these respects.

These studies are finally extended to consider the variation in form of *Lobelia dortmanna* in nature at different depths below the water surface. Correlations are established between the increasing length of the leaves and decreasing light intensity, while some influence of light quality can also be detected. The changes in light energy have negligible effects upon leaf breadth. Transplantation experiments were employed to eliminate variations in the genetical properties of the plants used.

On the phytogeographical side a field of wide scope has been opened up by F. K. Rawitscher, in analysing the features of Brazilian vegetation (*Bols. Fac. Fil. Cienc. e Let., Botanica*, No. 13, 1942; No. 4, 1943). According to this work, the distinguishing features of Brazilian soils are their very great depth, a character which is associated with the high rate of rock decomposition in response to high temperature, and is therefore believed to be typical of tropical areas in general. During dry periods, although the upper layers of soil (2-3 metres) may become dry, the vegetation is not materially affected if it possesses roots long enough to pass below the dry layer. The original natural vegetation probably normally possessed this character but, in consequence of repeated burnings, a more ephemeral flora has become dominant, which cannot maintain contact with the deeper layers of soil and so suffers drying out each summer. The change in flora results in rapid soil degeneration, partly because mineral salts are no longer brought to the surface by the root activities, so that leaching becomes very pronounced in the upper layers. But the lack of humus, due to the thin vegetation, and also the very low absorption power of clays formed in hot climates both tend to accentuate these changes, as well as to produce soils lacking cohesion and accordingly offering no resistance to erosion by water.

More recently F. K. Rawitscher and M. Rached (*An. Acad. Bras. d. Cienc.*, 18, 261, 1946) have drawn attention to another feature

of the natural vegetation, the frequent presence of plants with remarkable subterranean "xylopodia," trunk-like growths in species that, though belonging to genera of tree-forming plants, show little above ground but leaves and inflorescence. The examples described in this paper are *Jacaranda decurrens* Cham (Bignoniaceæ), *Cochlospermum insigne* St. Hil. (Cochlospermaceæ) and a new species of apparently stemless palm, *Acanthococos*.

ZOOLOGY. By WILLIAM HOLMES, M.A., D.Phil., The University, Oxford.

A RECENT volume of the *Bulletin of the American Museum of Natural History* is composed of a monograph by Dr. G. G. Simpson, Curator of Fossil Mammals and Birds, on *The Principles of Classification and a Classification of the Mammals* (Vol. 85, 1945, pp. xvi + 350). The author combines an expert knowledge of the mammals, both living and extinct, with a lively interest in the current problems of genetics and ecology in their bearing on the evolutionary processes of species formation and of the segregation of the larger groups of the animal kingdom. The first part of the work contains a broad and stimulating discussion of the principles of taxonomy, with particular reference to the problem of classification: to the problem of how a classification may be an effective instrument of research on living forms and yet give an adequate indication of the probable phylogenetic relationships between the groups established by the classification. This problem is well known to all who have attempted to give a satisfactory classificatory position to a group of animals now extinct, which must have contained within it the ancestors of living forms, though these are now sharply distinguished from the parental stock. There is a lucid exposition of the Linnæan system and of the current International Rules. The second part contains a formal classification of the Mammalia which is in many particulars novel. This is presented in detail: it includes all groupings down to genera, and includes all genera, living and extinct, together with a statement of their distribution in space and time. There is a lengthy discussion of the classification, with a review of the history of the classification of each group and a wealth of valuable references. Finally there are indices of family and generic names, and of vernacular names. It is perhaps the most important contribution to the subject since Gregory's treatise on the mammalian orders published in the same Bulletin thirty-five years earlier.

THE PRINCIPLES OF CLASSIFICATION.—"It is impossible to speak of the objects of any study, or to think lucidly about them, unless

they are named. It is impossible to examine their relationships to each other . . . to treat them scientifically, without putting them into some sort of formal arrangement."

The science of classification, taxonomy, is not merely a dull process of the sorting, identifying and labelling of genera and species. It is true that no major alteration in the greater groupings of the animal kingdom has been made since 1881, when Ray Lankester demonstrated that *Limulus* is an Arachnid and not a Crustacean, but nevertheless taxonomy is not a dying science. It is constantly revived by its association with the problem of the origin of species, and the classifier must comprehend all the phenomena of variation, population size and dynamics; characters of animals that are far from the dreary certainties of the old-fashioned museum. "The definition and study of species in this more modern way have barely begun and involve, beneath the surface of apparently conservative Linnæan classification, a real revolution."

Another sort of problem confronts the taxonomist when he attempts to associate genera into larger groups such as orders and classes. The most obvious basis of classification is morphology: animals are grouped according to the number of structural characters they have in common, and according to a hierarchy in the "importance" of these characters, as assessed by the classifier. The lower the rank of a classificatory group the more characters have its occupants in common. This type of classification was adopted by Linnæus, and by most of his successors before Darwin. If a zoologist regarded non-morphological characters as more important he was at liberty to use them as the basis of his classification. Thus in 1837 Bonaparte divided the mammals into "Ineducabilia" and "Educabilia"; in 1802 Oken divided the whole animal kingdom into five classes, each one characterised by the possession of one only of the five senses that are present together in man. On the whole most systematists adhered to morphology as the basis of their scheme, but it is now believed that a classification should not merely separate animals in descending categories with increasing community of structure; it should also demonstrate the evolutionary relationship between animals at all levels, so far as our knowledge permits. The scheme adopted must not only give a "horizontal" picture of the modern living world; it must also indicate a "vertical" picture of the ancestry of living forms, including, therefore, those that are now extinct; there must often be intermediates, grading between one group and another, frustrating sharp dichotomies.

Since morphological characters are believed to be an important index of evolutionary relationship, usually the only one available,

this did not involve the wholesale abandonment of earlier classifications. There were rather merely "simple shifts in emphasis on particular characters. In non-evolutionary theory there were, for instance, few good reasons for not classifying whales as fish. Their living in water, their lack of hair, and their other fish-like peculiarities, might as well have been emphasised as their warm blood, their mammary glands, their lungs, and their other mammal-like characters. But in evolutionary theory it became apparent that whales had terrestrial ancestors and that their mammal-like characters were derived from those ancestors, and their fish-like characters were not. Hence whales are mammals phylogenetically."

"Acceptance of phylogeny as the basis for classification, not because it is the only valid basis, but because it is agreed upon as the most desirable, is merely a preliminary. It leaves two enormous, almost inconceivably complex problems to be solved. Phylogeny must itself be determined before classification can be based on it. A phylogeny, even when perfectly known and universally accepted (which none is in detail), is not a classification, and the intricate relationship between phylogeny and classification leaves more than ample room for the exercise of skill and judgment and for differences of opinion."

These are the considerations that have influenced Simpson in his reconsideration of the classification of the Mammalia.

THE CLASSIFICATION OF THE MAMMALS.—The customary primary division of living mammals into three groups of equal status, Prototheria (monotremes), Metatheria (marsupials), Eutheria (placentals), is modified. The author accepts the view that the marsupials are more closely related to the placentals than to the monotremes; hence the Prototheria are a separate major group of mammals, and the Metatheria and Eutheria form another. He thus divides the living mammals into two subclasses: Prototheria and Theria. The multituberculates, a large group of fossil forms, is regarded as distinct from all other mammals, and as showing no evidence of being ancestral to any living forms. They are, therefore, placed in a separate third subclass. He considers that the very primitive extinct group the Pantotheria represented an offshoot in the Jurassic from the common placental-marsupial stock, and therefore divides the subclass Theria into three infraclasses. The Mammalia are thus divided:

Subclass : Prototheria (monotremes).

Subclass : Allotheria (multituberculates).

Subclass : Theria.

Infraclass : Pantotheria (pantotheres and symmetrodonts).

Infraclass : Metatheria (marsupials).

Infraclass : Eutheria (placentals).

The multituberculates have often been subdivided into two groups, the Tritylodontoidea and the Plagiaulacoidea. Simpson considers that there is very little connection between these two groups and that the former may be regarded as sufficiently close to the reptiles to be excluded from the Mammalia. Of the remaining major groups of Mesozoic mammals the Triconodonta, from the Jurassic, are suggested to be a separate offshoot from the mammal-like reptiles, having acquired mammalian characters by parallel evolution ; as they are so poorly known they are at present regarded as *incertæ sedis*. The Jurassic Pantotheria show unmistakable signs of marsupial and placental affinities, probably being an offshoot from the common stock ; they are included, therefore, in the subclass Theria as a separate infraclass. This contains two orders in the first of which the pantotheres are placed and in the second the poorly known Symmetrodonta, whose position is provisional.

The Marsupialia is the only order of the infraclass Metatheria ; its boundaries are well-established. Simpson rejects previous attempts to subdivide the order into two basic groups, either as polyprotodonts and diprotodonts on the characters of the incisor teeth or as didactyls and syndactyls on the absence or presence of union between the second and third toes of the hind foot. Both classifications, turning on single characters, produce contradictions. Instead he divides the order directly into six superfamilies, whose names are known from older classifications : Didelphoidea, Borhyaenoidea, Dasyuroidea, Parameloidea, Cænolestoidea and Phalangeroidea. The didelphids were the primitive stock, and the five other groups appear to have risen independently but at about the same time in the late Cretaceous from the ancestral stock.

The placental mammals, forming the last infraclass, are divided into four cohorts ; some of these are named by resurrecting prior but little-used Linnæan names, the more modern synonyms being confined to lower grades in the hierarchy of the classification. The primary criterion that a collection of orders should be grouped as a cohort is the belief that they arose together and very early from a relatively undifferentiated eutherian stock, which was probably not yet showing any definitely incipient ordinal division.

Cohort 1. Unguiculata

Order Insectivora.—This is the oldest known placental group. The characters that unite the insectivores are largely primitive and

it is probable that they have remained closest to the origin of all other placentals. The subdivision of the order presents many difficulties and is discussed. Simpson considers a recent proposal that the Macroscelididae and Tupaiidae are so closely related that they should either both be included as insectivores or both as primates. Provisionally, however, he retains the older arrangement, placing the first with the insectivores, the second with the lemurs among the primates, for they have many definite and special lemuroid characters.

Order Chiroptera.—The bats, which comprise this order, are a well-defined group. Because of their small size and their habits the fossil record is poor; however, the subdivision into two suborders, containing the fruit-eating and carnivorous forms respectively, appears to be a natural one.

Order Primates.—This group is discussed at some length. Its classification is much disputed, and this Simpson attributes to the fact that much of the relevant work has been done "by students who had no experience in taxonomy, and who were completely incompetent to enter this field" instead of by "sober and conservative mammalogists." The author's classification begins with a dichotomy into two suborders: Prosimii and Anthropoidea. The former is divided into three infraorders, containing the lemur-like, tarsier-like and loris-like prosimians respectively. The tarsiers are thus not regarded as of subordinal rank as they are by some anatomists. The classical division of the Anthropoidea into Platyrrhini and Catarrhini is abandoned. Instead there are three superfamilies: Ceboidea, Cercopithecoidea and Hominoidea. These groups appear to be of about equal antiquity. With closer approach to man the confusion in classification becomes greater, and for further discussion the original paper must be consulted.

Order Edentata.—This group, though always confined geographically, is one of the most abundant and varied of the mammalian orders. Most classifications separate them sharply from other mammals, but Simpson has abandoned this procedure and places them here with the other primitive eutherian orders. The group contains two suborders; the larger, Xenarthra, contains the armadillos, glyptodonts and sloths, now considered as a natural group. The second suborder is small and extinct; it includes *Metacheiromys* and *Palaeonodon* and is known as Palaeonodonta. Two groups, the Pholidota (pangolins) and Tubulidentata (*Orycteropus*, the aard-vark) are now removed from the edentates. The Tubulidentata are removed altogether from the Unguiculate cohort to that of the Ferungulata (see below).

Order Pholidota is, as just stated, separated from the Edentata.

The above unguiculate orders Chiroptera, Primates and Edentata all retain many primitive characters, and represent insectivores that evolved on particular trends of specialisation. The Chiroptera chose flight; the Primates cerebral complication, with its associated characters; the Edentata followed three trends, to a varying degree: digging and climbing, dietary restriction with tooth degeneration, and defence by dermal armour.

The remaining unguiculate orders are:

Order Dermoptera ("flying lemurs").—This is a small group with both insectivore and bat resemblances. They are certainly not lemurs and cannot fly.

Orders Tillodontia and Tæniodontia are extinct and incompletely known.

Cohort 2. Glires

This contains two orders only: the Lagomorpha (hares and rabbits) and Rodentia (all other rodents). This subdivision of the old rodent group is justified, not only on the morphology of the living forms, but also on the discovery that the lagomorphs and rodents *sensu stricto* were already decisively distinct in the late Palæocene and Eocene. Indeed, the union of the two orders into one cohort is "permitted by our ignorance rather than sustained by our knowledge." Almost nothing is known of the derivation of this cohort and its relations with the others. More than half of living mammals are rodents, but less than one-tenth of the known Tertiary fossils. The classical taxonomic subdivisions of the lagomorphs and rodents, based entirely on recent forms, will probably suffer great modification when the fossil record has been further revealed.

Cohort 3. Mutica

This contains only one order: Cetacea. Being perfectly adapted to aquatic life the whales are the most peculiar and aberrant of the mammals, and because of the absence of connecting fossils nothing approaching a unified phylogeny can be suggested. They are extremely ancient, and probably arose early from an undifferentiated eutherian stock. The old division into two suborders, the one for the toothed whales, the other for the whale-bone whales, still stands as a valid phylogenetic classification. The Eocene fossil cetaceans are primitive and in some respects intermediate; they are placed in a third suborder Archæoceti, but they cannot be regarded as

ancestral to the members of the two surviving suborders, *Odontoceti* and *Mystacoceti*.

Cohort 4. *Ferungulata*

When Gregory first split the classical group *Ungulata* by removing the *Artiodactyla* he did so in the belief that the *artiodactyls* were derived from carnivores, while the other ungulates came from unknown Cretaceous fossils similar to the early Tertiary *condylarths*. Now, however, it is known that some of these *condylarths* are so like *artiodactyls* that an exclusive connection between carnivores and *artiodactyls* is no longer to be accepted. The cohort *Ferungulata*, therefore, contains the carnivores as well as all the ungulates, and its name is correspondingly a compound of Simpson's devising. This arrangement is justified by the palaeontological evidence that the earlier carnivores and ungulates are more closely similar to each other than the members of either group are to their contemporaneous insectivores and others of the cohort *Unguiculata*. There is an early and clear separation between *unguiculates* and *ferungulates*, and this is the sort of basic division that properly corresponds to their classificatory grouping into two cohorts.

The main subdivisions of the cohort *Ferungulata* are as follows ; wholly extinct groups are marked *.

Superorder : *Feræ*. *Order* : *Carnivora*.

Suborders : * *Creodonta*, *Fissipeda*, *Pinnipedia*.

Superorder : *Protungulata*. *Orders* : * *Condylarthra*, * *Litopterna*, * *Notoungulata*, * *Astrapotheria*, *Tubulidentata*.

Superorder : *Pænungulata*. *Orders* : * *Pantodonta*, * *Dinocerata*, * *Pyrotheria*, *Proboscidea*, *Hyracoidea*, * *Embrithopoda*, *Sirenia*.

Superorder : *Mesaxonia*. *Order* : *Perissodactyla*.

Suborders : *Hippomorpha*, *Ceratomorpha*.

Superorder : *Paraxonia*. *Order* : *Artiodactyla*.

Suborders : *Suiformes*, *Ruminantia*.

The *Ferungulata* became enormously diversified during the Tertiary, though they are fewer in individuals and in genera than the *Glires*. Since the cohort contains fifteen orders a grouping of these into five superorders is adopted ; the superorders distinguished are considered to be of roughly equivalent phylogenetic status though they are very unequal in size. No transition between the superorders can be traced in detail, so the relationships are somewhat uncertain. Nevertheless, it is probable that the first dichotomy in evolution was between the earliest *Feræ* and the earliest *Protungu-*

lata. The ancestral form of the cohort would probably be most easily definable as belonging to the *Ferae*. Within the *Ferae* the *Crocodonta* includes the most primitive and structurally ancestral members of the order ; they ramified and from within them arose the *Fissipeda* ; these themselves ramified and from them arose the *Pinnipedia*.

Simpson's publication should be consulted for the further justification and elaboration of this classification. He has taken the fullest advantage of the labours of his predecessors in the study of the group. His historical sense is so well-developed, and his knowledge of living and fossil forms so impressive, that the reader feels that here is a modern basis, as solid as it can be, for teaching and research on the *Mammalia*.

NOTES

New Journals

We have received a copy of a new journal called *Microtecnic*, published in Switzerland and devoted to "Fine Mechanics"—particularly metrology and the techniques allied to watchmaking. The journal is to appear six times a year; the articles are in French, but there is a complete translation of each article into English.

The subjects dealt with are peculiarly identified with Swiss industry; the journal bears evidence of this background, and will probably be particularly useful to those concerned with similar industries in this country. In the review number (Vol. I, No. 2) there are interesting articles on the curriculum and the equipment of the Horological School of La Chaux-des-Fonds—it appears that it is considered economically desirable to add to the watchmaking industry of the Jura other instrument work requiring similar skills, and the four-year training at the school covers a general course in the technology of fine instruments.

The number also contains articles on some metrological apparatus made by the celebrated Société Genévoise, on the technique of hair-springs for watches and electrical instruments, on beryllium and its alloys, on the timing of aircraft speed records, on the use of rotatory substandard meters in electrical testing, and on the International Decimal Subject Classification.

The sole British agents for the publication are Bailey Bros. and Swinfen Ltd., 11, Ronalds Road, Highbury, London, N.5, and the price is 52s. 6d. per annum.

H. J. J. B.

It is a pleasure to welcome a new journal of Analytical Chemistry in *Analytica Chimica Acta*, published by the Elsevier Publishing Co., Ltd., Amsterdam (London Agents: Cleaver-Hume Press, Ltd. 55s. yearly). It is intended to be of international character and to embrace all branches of modern analytical chemistry, so research workers in all branches are invited to contribute original papers. These should be sent to The Editor, Prof. Dr. P. E. Wenger, 8 rue St. Victor, Geneva, and may be in English, French or German. The Editorial Board includes well-known names in Britain, France, Switzerland, Holland, Belgium and S. America.

Studying the first number of the journal, it is clear that a high standard is required, and that a wide field is to be covered. For example, there are memoirs on the acidimetric determination of

aluminium, on borax (colorimetrically)—particularly in boron steels, on electrometric titrations, on semi-micro estimation of silica, on spectrographic analysis, on alkaloid-cadmium complexes, and other matters. Each paper has a summary in the three languages. The style and format of the journal are pleasing; printing and paper are better than is, alas, possible in England to-day.

The *Acta*, as represented by its first number, is of good quality and is very welcome; we wish it an expanding future.

H. E. Cox.

"Quarterly Journal of Mechanics and Applied Mathematics"

We have received a letter, signed by Prof. S. Goldstein, Dr. Southwell, Prof. G. I. Taylor and Prof. Temple, announcing the publication of a new quarterly journal dealing with classical mechanics and covering such topics as Sound and Dynamical Meteorology; Elasticity and Plasticity; Non-linear Dynamics and Classical Electromagnetism, including the propagation of electric waves in wave-guides. It is intended that the journal shall contain also occasional papers dealing with experimental work related to appropriate mathematical topics. The signatories, together with other eminent British investigators, will form the Editorial Board and the business of the journal will be conducted by Dr. V. C. A. Ferraro (University College, Exeter) and Dr. G. C. McVittie (King's College, London), the Executive Editors. The journal will be published by the Oxford University Press and it is proposed that the first number shall appear in April. Contributions will be welcomed. They should be sent to one of the Executive Editors at King's College, Strand, W.C.2.

Miscellanea

The Nobel Prize for Physiology and Medicine for 1947 has been awarded in two parts, one to Prof. Bernardo A. Houssay, recently professor of physiology in the University of Buenos Aires, and the other to Prof. Carl F. Cori and his wife, Dr. Gerty T. Cori, of the Washington University School of Medicine. An account of their work is given by Prof. F. G. Young in his article on "The Mechanism of Action of Insulin" in this issue of SCIENCE PROGRESS.

The Nobel Prize for Chemistry for 1947 has been awarded to Sir Robert Robinson, F.R.S., and that for Physics to Sir Edward Appleton, F.R.S.

Dr. E. Ashworth Underwood, director of the Wellcome Historical Medical Museum, has been awarded the Cross of the Chevalier of the Legion of Honour.

The American Medal of Honour with Silver Palm has been awarded to Dr. H. Roxbee Cox, director of the National Gas Turbine Establishment and chairman and managing director of Power Jets (Research and Development), Ltd., and the American Medal of Freedom with Bronze Palm to Dr. E. Talbot Paris, principal director of scientific research (defence) in the Ministry of Supply, for their services to the United States during the war.

Other awards for 1947 include : Kelvin Gold Medal to Air Commodore F. G. Whittle ; and those of the Physical Society—Duddell Medal to Dr. R. J. Van de Graaff, of the Massachusetts Institute of Technology ; Charles Chree Medal and Prize to Sir Edward Appleton ; and Charles Vernon Boys Prize to Dr. C. F. Powell, of the H. H. Wills Physical Laboratory, University of Bristol.

Prof. P. I. Dee, professor of natural philosophy in the University of Glasgow, and Sir William Griffiths, chairman and managing director of the Mond Nickel Co., Ltd., have been appointed members of the Advisory Council for Scientific and Industrial Research. Sir Lawrence Bragg, Sir John Lennard-Jones, Sir Andrew McCance and Sir Raymond Streat have retired from the Advisory Council on completion of their terms of office.

Dr. Norman C. Wright, director of the Hannah Dairy Research Institute, has been appointed chief scientific adviser to the Minister of Food.

Mr. N. B. Kinnear has become director of the British Museum (Natural History) in succession to Sir Clive Forster-Cooper.

Prof. I. de Burgh Daly has been appointed director of the new Institute of Animal Physiology to be established by the Agricultural Research Council.

The Society of Dyers and Colourists ask us to announce that their general secretary, Mr. P. A. Wells, M.A., M.Sc., has been appointed director of education in the International Wool Secretariat, London, from October 1st, 1947.

We have noted with great regret the announcements of the death of the following scientific workers : Surgeon Rear-Admiral C. M. Beadnell, C.B., President of the Rationalist Press Association and author of several scientific books ; Dr. H. T. Calvert, M.B.E., formerly director of the Water Pollution Research Laboratory ; Prof. A. T. Cameron, C.M.G., professor of biochemistry in the University of Manitoba ; Sir Clive Forster-Cooper, F.R.S., director of the British Museum (Natural History) ; Prof. J. M. Gulland, F.R.S., professor of chemistry in University College, Nottingham ;

Sir Albert Howard, C.I.E., formerly director of the Institute of Plant Industry, Indore ; Dr. Ellsworth Huntington, formerly research associate in geography, Yale University ; Prof. Ll. Rodwell Jones, formerly professor of geography in the University of London, London School of Economics ; Prof. T. D. Jones, C.B.E., professor of mining in the University of Birmingham ; Prof. F. E. Lloyd, emeritus professor of botany in McGill University, Montreal ; Dr. S. Miall, for many years editor of *Chemistry and Industry* ; Prof. Max Planck, For.Mem.R.S. ; Mr. R. I. Pocock, F.R.S., superintendent of the Zoological Society's Gardens, London, from 1904 to 1923 ; Prof. T. Tammes, emeritus professor of genetics in the University of Groningen ; Prof. W. W. Watts, F.R.S., emeritus professor of geology in the University of London, Imperial College of Science and Technology.

An article by two United States scientific observers, Haraden Pratt and Arthur Van Dyck, published in the March edition of *Electrical Communication*, gives, besides an account of the results of the Bikini atom bomb tests, a well-considered summing up of the effects of this new weapon on modern civilisation.

Due to over-exaggeration by the Press in their forecasts of the damage likely to be caused during the tests, the public were led to believe that the very forces of nature might be unleashed, such as earthquakes and tidal waves. Consequently there was a certain feeling of anti-climax when the actual results were reported. It is necessary to remember the large distances involved, that ships were wrecked and sunk up to half a mile from the point of explosion of the surface bomb ; and sea-worthy ships are not easily sunk.

A better name for the bomb would probably be the "Poison Bomb," because the chief danger is from the radiations from the radio-active products of the explosion. The poisoning was very apparent in the case of the underwater test. A photograph of this explosion shows an enormous column of water rising to a height of over a mile and falling on to the ships which, by comparison, look like ants clustered around the base of a large toadstool. The millions of tons of sea water, contaminated with the fission products, was estimated to be equivalent to hundreds of tons of radium. This water, falling on to the ships and washing down the ventilators, pipes and funnels, entering into the circulating systems and pumps, and becoming lodged in the incrustations inside sea water piping, contaminated the ships to such an extent that their reconditioning is an almost impossible task. If a similar bomb were to explode

under New York City it would destroy subways and render an area of at least ten square miles uninhabitable for months.

Familiarity breeds contempt. The article ends by remarking that already the subject of the atom bomb and all it means "has become familiar and has lost its true meaning to many people. This complacency is dangerous. These facts mean that a revolution in human affairs has occurred. Hereafter civilisation must struggle not to advance, but actually to survive."

An account of "Pulse Code Modulation," a new system for long-distance multi-channel telephony, was published by the Bell Telephone Laboratories in the July edition of the *Record*. The cumulative building up of noise with each of the many amplifications necessary over long distances has always been a controlling factor, and this new system was designed to produce high-fidelity reproduction undisturbed by all but an abnormally high noise level.

In this system a sequence of on-or-off pulses is sent to the receiving station, where the reconstruction of the signal depends only on the arrival or non-arrival of pulses, nothing more. Thus at every repeater point the presence or absence of pulses is all that is required to permit the message to be regenerated with a noise-free start.

The principle of the method may be seen from the following analogy. An experimenter is watching a curve being automatically recorded on a moving sheet of graph paper. Every fifteen seconds he calls out the reading and a second person records it on a similar piece of graph paper. Beforehand they will decide on a certain degree of accuracy, e.g. using a 40-unit scale, points are to be given to the nearest unit. Thus a value of, say, 35.6 will be given as 36; and only 40 different values (and zero) are possible. Despite this limitation the second graph, when drawn smoothly, will be very similar to the original.

The pulse system works in a similar manner. The voltage of the voice frequency wave is measured every 125 micro-seconds and its value is transmitted to the receiving station where the wave is to be reconstructed. This is done by means of coded signals, each signal consisting of a group of five on-or-off pulses and representing one value of the voltage. The group of pulses can be made to form 32 different "characters," so each character can be made to represent one-thirty-second part of the voltage range. Splitting the voltage range into 32 parts is found to be quite sufficient provided the voice wave is first passed through a logarithmic compressor. For higher fidelity a six-pulse code may be used giving 64 possible characters. This type of modulation has great possibilities for future

improvements in long-distance telephony, and, as it requires a wide band-width, it will be particularly suitable for use over micro-wave radio relay circuits. It can also be used as a multi-channel system, since each group of pulses uses up only 16 micro-seconds of the 125 micro-seconds allowed between groups.

Fading has been an ever-present problem with radio engineers, and with the use of super-high frequencies for communication purposes new techniques are required to eliminate it. An account by G. H. Huber of the methods employed over a 510-mile circuit from San Francisco to San Diego is published in the *Bell Laboratories Record* for September 1947.

The circuit, which was set up to evaluate the performance of the "AN/TRC-6" system operating on a wave-length of approximately 6 cm., was split into five relay sections, varying from 76 to 170 miles in length with a side branch of 24 miles. Parabolic reflectors were used, mounted on high masts, and, by making use of high ground, it was possible to make all the paths optically clear.

Although the parabolic type of reflector concentrates the radio waves into a narrow beam, at certain points between the relay stations the line of sight path comes close to the earth, and here strong reflections are possible. As a result two signals may arrive at the receiver, one directly and the other after reflection. If the two signals are in phase the reception will be good, but will deteriorate as they get out of phase and act in opposition. Changes in atmospheric conditions result in small changes in the relative length of the two paths. These small changes are, however, large compared with a wave-length of 6 cm., and result in deep fades as the signals get out of phase.

Over some links it was possible to reduce fading by cutting down the reflected signal. Thus, over the short 24-mile link, where the angle between the two rays is comparatively large, slight tipping of the reflectors upwards reduced the fraction of the reflected ray in the total signal received. On another link it was possible to move the antennas so that a mountain obstructed the reflected ray.

Over other links a new type of "Diversity Reception" was developed. Considering both the direct and reflected rays, the signal strength along a vertical line will be found to consist of a pattern of alternate maxima and minima, this pattern moving up and down with small changes in the atmospheric conditions. The distance apart of the maxima and minima may be calculated approximately, and, if two receiving paraboloids are then fixed on the mast at this distance apart, when one is at a maxima the other

will be at a minima and vice-versa, i.e. their signals will be complementary. This arrangement was tried, the output from the receivers being connected in parallel after the first amplifier stages. The automatic volume control voltages of the two receivers were connected together and fed back to both initial receivers, their gain thus being controlled by the stronger signal. The shift of control from one receiver to the other was very smooth.

The series of tests made on this circuit are of particular interest on account of the improvement obtained by using this particular type of Diversity Reception. An idea of the improvement achieved can be seen from the results obtained over the 76-mile link. In one hour the upper receiver had recorded seven deep fades and the lower ten. On connecting the two receivers in parallel no deep fades in the final output were obtained during the following month. Similarly over the 170-mile link a large improvement was obtained and it was possible to dispense with an intermediate relay station.

Piezoelectric crystals have the property of developing electric charges on their surfaces when subjected to mechanical strain, those most commonly used being quartz, Rochelle salt and ammonium di-hydrogen phosphate (A.D.P.). While quartz occurs naturally, the other two have to be grown from solutions of their salts. During World War II the demand for piezoelectric crystals for service requirements became so urgent that new plants had to be started for growing A.D.P. crystals. An account of the development work which was necessary before these crystals could be grown on a large scale and of the final methods adopted is given in the *Bell Laboratories Record* for October 1947.

In crystal culture, as in farming, it is necessary to start with a "seed" and to grow the final product under suitable temperature conditions. The seed crystal is placed in a saturated solution which is made to flow past its growing surfaces. As the crystal grows and removes salt from the solution, the temperature is lowered slightly so that the decrease in the solubility of the salt is just sufficient to maintain a saturated solution. Care has to be taken to keep the saturation of the solution below a higher limit at which small crystals are likely to form and, by settling on the prism faces, cause disorderly growth. Also, with high rates of growth or poor circulation of the solution, undesirable cloudy areas may form on the growing surfaces.

Two types of tank have been used. The first is rectangular in shape and can hold up to 40 crystals. Numbers of these tanks are mounted on rocking platforms in a room whose temperature

can be controlled to within 0.1°C . The crystals may grow to a length of 10 to 12 inches without further attention, after which the circulation of the liquid is sometimes altered. A second type of tank which is particularly suitable for laboratory research is cylindrical in shape and has its own temperature control. The crystals are mounted on radial arms about a tube which passes down the centre of the tank and is made to rotate backwards and forwards.

For practical purposes the final crystals must have cross-sections perpendicular to their length of at least 1 square inch in area. This proved a difficulty at first, as the A.D.P. crystals tended to grow only on their end or pyramid faces. One method used commercially to overcome this difficulty was to form all the natural faces on a Z cut plate of suitable size, thus forming a cloudy capped plate on which clear bars could be grown. Another method was also developed, in which a bar was cut into diagonal seed plates so that all the faces of the seed were natural faces or parallel to natural faces. Unfortunately this method, although producing a bar of completely clear and useful material, is not so attractive commercially because of the close requirements in the angle of cut and also in the temperature control during the initial stages of growth.

The widespread growth of mould on the ceilings and walls of modern houses in New Zealand led to an investigation described in the *New Zealand Journal of Science and Technology* for January 1947.

It was soon realised that the important factor to consider was the high humidity which so often accompanied the growth of mould, and that methods such as the use of fungicides would not solve the problem satisfactorily. Observations were made in modern houses and it was found that the relative humidity inside was generally above 80 per cent., the point at which mould can be expected to occur. Assuming the mean humidity outside to be 86 per cent., it only requires a rise of 2°F . to lower the humidity to 80 per cent. If, therefore, warming of the air entering the room by 2°F . could be ensured, the growth of mould would be avoided.

By measuring the temperature of the walls and ceilings of these houses it was found that they were colder than those in the traditional type of house, indicating large losses of heat. Closely associated with temperature considerations is ventilation. A special method developed to measure the ventilation depended upon the release of a small amount of hydrogen in the room and then, by continuously sampling the mixture, estimating the rate at which it was replaced by fresh air from outside.

The article ends by giving recommendations based on the results

of the investigation. For the elimination of dampness and of the consequent mould, the natural ventilation should be capable of giving at least three changes of air per hour with windows and doors shut. The walls and ceilings should be heat insulated and, in exposed localities, the floors as well. Also to help in maintaining a higher temperature inside the house, no room should have more than two exterior walls, nor window space greater than 20 per cent. of its floor area, and the fireplace should be built on an interior wall so that heat from the back would not be lost but kept in the house.

An account of the new "Agrocide" range of insecticides is given in a booklet produced by Plant Protection, Ltd., Nobel House, Buckingham Gate, London, S.W.1. A comprehensive list is included of the insect pests to which the insecticides have proved toxic, as well as a detailed account of the methods of application in specific cases.

Since 1936 several thousand organic and other chemicals have been tested in the ICI laboratories in an attempt to find a cheaper and more persistent insecticide. The method of testing was to find the concentration required to obtain a 50 per cent. kill on a series of standard insects, although, later, attention was focussed instead on particular insect problems. With the fall of Malaya in 1942, when the supply of derris almost entirely ceased, the problem became more acute and, by a process of elimination, it was found that benzene hexachloride was the most promising. Of the four known isomers of this chemical the gamma isomer was found to be the most toxic and, under the proprietary name of "Gammexane," forms the basis of the new range of insecticides. This chemical was first prepared by Michael Faraday in 1825, but more than a century was to elapse before its remarkable insecticidal properties were discovered.

While Gammexane has proved quite as effective as the standard products against many pests, it can also be used against pests which previously it was either not economic to treat or for which no chemical had been sufficiently potent, *e.g.* the apple-blossom weevil. Unlike arsenicals and nicotine, Gammexane is of a very low toxicity to man and warm-blooded animals, and it has been used with marked success in veterinary, domestic and industrial fields.

ESSAY REVIEW

A CENTURY OF BRITISH CHEMISTRY. By T. S. WHEELER, D.Sc., Ph.D., F.R.I.C., Professor of Chemistry in University College, Dublin. Being a Review of **British Chemists**. Edited by A. Findlay and W. H. Mills. [Pp. 431, with 16 plates.] (London: The Chemical Society, 1947. 18s. net.)

THIS well-produced volume, on which the Editors and the Chemical Society are to be congratulated, is one of two published by the Society to mark the passing of its first century. It contains accounts of the life and work of sixteen of its more distinguished Fellows, who contributed to the advancement of chemical science during the last ninety years. All now have done with chemistry, but their combined work bridges the gap between the relatively superficial knowledge of the eighteen-fifties and modern science with its massive theoretical background and its wealth of experimental technique.

In 1841, the Foundation Year of the Society, Dalton had yet three years to live, and atoms were stable, indivisible particles, indistinguishable as regards any one element. Molecular theory was in the state of chaos from which it was rescued by Cannizzaro about 1860, while organic chemistry was, as Wöhler wrote in 1835, "like a primeval forest of the tropics full of the most remarkable things"—modern structural organic chemistry began with Kekulé about 1865. Sir William Crookes (1832–1919), with whose life this volume begins, was born three years after Kekulé, and, in addition to discovering thallium in 1861, lived to separate Uranium-X and to invent the spinthariscopes. His work on electrical discharges in gases under low pressure led to the discovery of X-rays and to Sir J. J. Thomson's classical researches on the electron.

At times Crookes seemed almost to have the gift of prophecy. In 1879, apropos his work on electrical discharges, he indicated the dual nature of the electron when he told the British Association that "in some of its properties Radiant Matter is as material as this table, while in other properties it almost assumes the character of Radiant Energy." Again in 1886 he hinted at isotopes: "I conceive, therefore, that when we say the atomic weight of, for instance, calcium is 40, we really express the fact that, while the majority of calcium atoms have an actual atomic weight of 40, there are not a few which are represented by 39 or 41, a less number by 38 or 42 and so on." A boy of nine when the Chemical Society was founded, Crookes had a surprisingly close family connection with the period

when the much older Royal Society began; about 1897 he wrote to *The Times* to say that his father (1792–1884) heard from a great-grandmother (1710–1814) anecdotes of the Great Plague of 1665, told her by her grandfather (1639–1729) who was an eye-witness of the incidents he related.*

Sir James Dewar (1842–1923) was like Crookes a fine experimentalist. Early in his scientific life he interested himself in organic chemistry and his name is still associated with structural formulæ for benzene and pyridine. Later, at Cambridge, he began an extensive series of spectroscopic observations, but when, in 1887, he was appointed to the Fullerian chair at the Royal Institution, which Faraday had held, he devoted himself to the liquefaction of gases, a subject on which his famous predecessor had worked. It is for these experimental researches that Dewar will be for ever remembered.

Ramsay (1852–1916) and Dixon (1852–1930), who were born in the same year as van't Hoff and Emil Fischer (a vintage year in chemistry), were, like Crookes and Dewar, masters of experimental technique; in addition they were teachers and founded distinguished schools. Dixon made Manchester world-famous as a centre of research on gaseous combustion, while Ramsay's school achieved even greater fame at University College, London, by adding a whole group to the Periodic Table, thus making outstanding contributions both to the theoretical and practical sides of chemistry.

Although Dixon and Ramsay trained many to carry on their work, H. E. Armstrong (1848–1937) who, born before, outlived both, was perhaps the greatest of the chemical pedagogues. As Professor at the Central Technical College, South Kensington, he influenced many, destined later to achieve distinction as chemists. He was one of the last of the typical Victorians, an outspoken man of tremendous energy, wide interests and strong views, not often assailed by fears as to the possibility of his being mistaken. He carried out much important and varied work in chemistry, but it is probable that he will be remembered as a teacher and as a man—and for the coloured waistcoats which he wore to support the British dye industry.

Three of those included in this volume were "Armstrong's men"; the preoccupation of Sir William Pope (1870–1939) with stereochemistry and crystallography, fields in which he accomplished so much, go back to the ten years he worked in Armstrong's laboratory. Their collaboration in research on camphor led to Pope's use

* The reviewer is indebted to Professor J. L. Simonsen, F.R.S., for this information.

of camphorsulphonic acid for the resolution of optically active

Lapworth (1872-1941) also studied under Armstrong, his interest in camphor and in the mechanism of aromatic substitution being derived from his teacher. Lapworth was a pioneer in physical organic chemistry, a subject of increasing importance. His influence on the development of chemical theory was very great, for he was one of the founders of the British school of electronic organic chemistry, the existence of which has given British chemists an insight into the mechanisms of organic reactions far ahead of anything that obtains on the Continent. It is surprising, therefore, to find the subject neglected in some organic textbooks which still feature views current at the time of Claisen. Significant, perhaps, of a fundamental British preference for practice rather than theory is the fact that Lapworth, in spite of his essential greatness as a chemist, received relatively few distinctions during his life.

Lowry (1874-1936) was assistant to Armstrong for seventeen years (1896-1913), and under Armstrong's inspiration started the work on the stereochemistry of derivatives of camphor which led to his distinguished studies on dynamic isomerism. It was Armstrong, too, who directed Lowry's attention to crystallography, which remained with him a lifelong interest.

Four organic chemists of the classical school figure in this volume. Of the four, W. H. Perkin, Jun. (1860-1929), the British Emil Fischer, was definitely the greatest. His researches compass a variety of important topics, relating mainly to the chemistry of natural products and including camphor, the terpenes and alkaloids. He was a master of synthesis, and it is tribute to his ability as a teacher that some of the most distinguished British chemists now living were his pupils. His brother, A. G. Perkin (1861-1937), devoted himself mainly to the isolation and study of the vegetable dyes, in which field he gained international reputation. Sir Jocelyn Thorpe (1872-1940), as assistant to W. H. Perkin, Jun., at Manchester from 1895 to 1909, was naturally influenced by that great man, particularly as regards his interest in alicyclic rings. He founded at the Imperial College a school of chemistry which has produced many chemists of distinction. Unlike Thorpe, Barger (1878-1939), the fourth organic chemist, had biochemical leanings, being chiefly interested in the chemistry of the alkaloids. Sir Arthur Harden (1865-1940) was definitely a biochemist, being distinguished for his work on alcoholic fermentation and on bacterial chemistry.

Meldola (1849-1915) studied natural history as a young man and it was mainly as a biologist that he was elected to the Royal Society

in 1886. His first interests were industrial, but, like Perkin Sen. before him and Green after him, he failed to overcome the apathy of British dyestuff manufacturers towards research. Turning to academic life, he succeeded Armstrong at Finsbury Technical College in 1885, when the latter went to the Central Technical College. Meldola was cast in a smoother mould than his predecessor, but with the assistance of Streatfeild and Castell-Evans he built up a department which gained a high reputation as a training centre for industrial chemists. His researches dealt mainly with the chemistry of synthetic dyes. Green (1864-1941) also, was industrially minded. Although he did much work on dyestuffs in general, he is mainly remembered for his discovery of primuline. For many years he sought to have the dyestuff industry in Britain developed on scientific lines, but it required a war to compel acceptance of his views.

Sir Gilbert Morgan (1870-1940) was also interested in dye chemistry, but he carried out much other work. A pupil of Meldola's and of his colleagues, he first entered the dyestuff industry, but some years later returned to the Royal College of Science to equip himself for an academic career. He added greatly to our knowledge of co-ordination compounds, gaining distinction in both inorganic and organic fields. He had a varied career, for he occupied chairs in the Royal College of Science for Ireland and at Birmingham; finally he became the first Director of the National Chemical Laboratory, which he made an active centre of important research.

Such then in brief outline were the men whom the Chemical Society honours itself in honouring. All were great chemists, all something more; in charm and personal qualities which command respect, not lacking. An examination of the courses of their lives reveals interesting features of similarity. The distinguished chemist of the last generation was long-lived, happily married in the late twenties, fond of music, and a good speaker and lecturer. He was appointed to a chair in the early thirties, was elected to the Royal Society a few years later, and became President of the Chemical Society when about fifty-five. A knighthood, if he achieved it, came when over sixty.

The thoughts and efforts of many distinguished chemists have gone to the making of this book. Each memoir is competently written; in many instances it is a pupil who is commemorating his teacher, so that the work is a labour of respect and admiration, and can be strongly recommended to all who seek to learn of the men who but recently toiled to advance chemistry. Particularly it is for the student girding himself to enter the profession. There is much about chemistry in it clearly and interestingly presented.

But there is, too, something more fundamental than chemistry, something of the ups and downs of human life and of men who bent to their tasks, addressing themselves to great problems, and questing for successful solution. Lives such as theirs are an inspiration to all.

The publication of this volume makes one more service of the Chemical Society to Chemical Science. And in felicitating the Society on its publication one must spare a word of praise to the Society's printers, Messrs. Richard Clay & Company, who have defrayed the cost of this volume as a contribution to the Centenary Fund.

REVIEWS

PHYSICS

Physique appliquée des installations à rayons X. By GEORGES DESTRIAU. [Pp. viii + 209, with 173 figures.] (Paris: Dunod 1945.)

THE aim of this book, which is written primarily for engineers, is to present the physical principles underlying the component parts of X-ray installations, and in his aim the author is eminently successful. His style is clear and precise, and the simple mathematical approach he makes to each of the subjects he discusses not only serves to demonstrate the principles, but also allows useful numerical data to be derived.

The book is divided into five sections. The first part deals in an elementary way with the basic physics of radiations. The subsequent parts deal with (a) the problems of the X-ray tube, (b) the generator, (c) accessories (mainly radiographic) and (d) dosimeters and the problems of filtration.

It is a pity that the author has confined himself in his examples so largely to apparatus of French manufacture. For this reason many recent developments are not described, both in the field of X-ray plants and dosimeters.

A full table of contents is given, but no index. The book is well printed and the diagrams are numerous and well done.

L. F. L.

Introduction to Atomic Physics. By S. TOLANSKY, Ph.D., F.I.C. Second edition. [Pp. xii + 351, with 5 plates and 115 figures.] (London, New York, Toronto: Longmans, Green & Co., 1946. 15s. net.)

IN the compass of some 350 pages the author treats most subjects which might come under the heading of Atomic Physics, including a chapter on Relativity. To accomplish such a programme in so short a space does mean that the treatment in many places can be little more than superficial, and this is the chief criticism of the book. True, it is confessedly an introduction and as such is very readable, but too often brevity has led to an inadequate treatment of some subjects, while the choice of subjects for which a mathematical treatment is given seems to be rather arbitrary. Another general criticism is that the book would have been greatly improved if it had been illustrated by more diagrams, and some of those which are given are rather sketchy. However, this does not apply to the plates, which are well chosen and reproduced, and bear a much closer relation to the text than is sometimes found in such illustrations.

In making criticisms of detail, one is disappointed to note that in this reprint a number of mistakes have not been corrected; for example, the cloud chamber on p. 23 will not work, the valve being in the pre-expansion position, while the piston can expand no further. The statement on the Compton effect on p. 163 conveys the impression that this effect is not observed in the scattering from a bound electron. Again, in mentioning the application of the Schrödinger equation to the hydrogen atom some of the

statements are very misleading and help little in revealing the implication of this treatment.

In this particular reprint of the second edition a very brief appendix is added on "Nuclear fission and atomic energy."

W. E. D.,

Applied Architectural Acoustics. By MICHAEL RETTINGER, B.A., M.A. [Pp. xii + 189, with 69 figures.] (Brooklyn, N.Y.: Chemical Publishing Co., Inc. \$5.50.)

THE statement, by the author in his preface, that architectural acoustics is still not taught as extensively in technical schools as it deserves, though intended as a criticism of the state of affairs in the United States, is probably equally true of the British Empire, though his conclusion that the sadly deficient acoustics of motion picture theatres is due to lack of education in such matters does not apply in Britain, where our architects, who are responsible for such matters here, are mostly well grounded in the subject in the University schools. The author, however, disclaims his book to be a textbook for colleges, rather it is "a handbook for the man in the field" and as such it must be reviewed.

The speciality in its treatment is that the acoustics of auditoria is treated as fundamentally a problem in geometrical acoustics; in ray-tracing, in fact. This is certainly a unique method of approach in a book on this subject, which gives it the appearance rather of a text on illumination, suitably modified by the replacement of light rays by sound rays. The author does not, of course, stick to this method of presentation exclusively, as very soon he must warn his readers that diffraction and interference take place in a room. Nevertheless, the method of approach is novel and perhaps more readily understood by the non-physicist than one based primarily on intensity distribution would be.

For those to whom the fundamentals of the subject are already familiar, the author's own work for the talking-film industry will prove most interesting. They will learn the mysteries of scoring stages, vocal rooms, sound stages and broadcasting studios, finding in this part of the book a mine of practical experience not to be found elsewhere.

The book concludes with some useful charts and a bibliography.

E. G. R.,

The Experimental Study of Structures. By A. J. S. PIPFARD, M.B.E., D.Sc., M.I.C.E. [Pp. viii + 114, with 57 figures.] (London: Edward Arnold & Co., 1947. 9s. net.)

THOUGH it belies its somewhat ambitious title, this pleasantly written little book should be of value to the teachers to whom it is addressed. It is based on a short course of lectures and demonstrations given by the author at the invitation of the Ministry of Education to the Summer School for teachers in Technical Colleges at New College, Oxford, in July 1946, and describes the laboratory experiments which the author has found useful to amplify classroom teaching. The principal elastic theorems and methods of stress analysis are first summarised, and in subsequent chapters detailed descriptions of apparatus and experiments used to illustrate the theorems are set out.

The first type of apparatus described is that intended to show qualitatively the behaviour of loaded structures. The most important section of the book

REVIEWS

deals with that branch of the rapidly expanding subject of experimental stress analysis which depends on the application of Clerk Maxwell's reciprocal theorem. Other chapters cover strain energy and distribution methods, experimental study of arches, to which the author has contributed so much, and a short description of experiments with sand.

There is no doubt that laboratory experiments and demonstrations do help to drive home first principles, and the experiments described in this book are excellent for the purpose. The serious weakness of the book, however, is that it does not point out the limitations of this approach. The danger is that, having carried out an experiment on a model, the student will think that he has done something more than illustrate a simple principle—that he knows how a structure will behave.

In University teaching, at least, every opportunity should be taken to emphasise the difference between the results obtained from a consideration of the idealised structure dealt with mathematically, or, as described in this book, experimentally, and of the behaviour of the real thing.

J. F. BAKER.

Fluid Mechanics. By R. C. BINDER, Ph.D. Seventh printing. [Pp. ix + 307, with 242 figures.] (London: Constable & Co., Ltd., 1947. 30s. net.)

SOME thirty years ago hydraulics, hydro- and aerodynamics were regarded as independent subjects. Gradually it has been realised that they have much in common. The volume under review attempts to blend those parts of particular interest to engineers under one heading, that of "Fluid Mechanics." How successful Prof. Binder has been may be judged from the fact that the book is now in its seventh printing.

Amongst the chapter headings are Kinematics of Fluid Flow, the Energy Equation for Steady Flow of Any Fluid and Dimensionless Analysis. There has been a tendency in some texts to stress the last-mentioned subject as if it led to the solution of all experimental difficulties, which is far from the case. Dr. Binder has given just sufficient on this matter to stimulate interest without overloading.

Other chapters deal with the Flow of Incompressible Fluids in Pipes and in Open Channels, Momentum Relations for Steady Flow, the Resistance of Immersed Bodies, Dynamics of Compressible Flow and the Flow of Compressible Viscous Fluids in Pipes.

In a new edition, it may be hoped that a little greater connection will be shown between Dynamic Lift, the subject of another chapter, and the characteristics of propeller pumps and turbines.

There is a useful chapter on lubrication, a matter usually neglected in such a text. A discussion on the Mathematics of Fluid Motion closes the book.

Many references and problems with answers are included. The work is easy to read and should be in the hands of all students studying for honours in any branch of fluid mechanics.

B. LLOYD-EVANS.

Mechanical Vibrations. By J. P. DEN HARTOG. Third Edition. [Pp. xii + 478, with 282 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 30s. net.)

THIS book aims at presenting to the reader of modest mathematical attainments ("simple differentiation and integration") a fairly comprehensive account of problems of mechanical vibrations arising in Engineering. It is essentially a textbook for the student, being in fact the substance of a course of lectures delivered to students of the Westinghouse Company in Pittsburgh during the years 1926-32. It gives an excellent account of the subject. The author, however, appears so concerned to simplify the mathematics that sometimes the opposite effect is produced, as, for instance, on p. 107, where the method of solution of the equations of motion of a system of two degrees of freedom is somewhat unsatisfactory. Sometimes the author is irritatingly long-winded. But often the attempt to simplify the mathematics is highly successful, as, for example, the derivation of the Fourier sine and cosine coefficients on p. 21.

The vibrations of systems of one degree of freedom, and those of two and higher number, are firstly discussed mathematically; the physical principles involved in the various practical problems discussed are always stressed, however, and Rayleigh's method, introduced early in the book, is used extensively in the discussion of the vibrations of rotating shafts, of beams, membranes, etc. An account of Stodola's method is also included.

In this new third edition of the book all matter has been brought up to date and a new section on helicopter ground vibration has been added. There are few misprints: on p. 138, where $x_2 - x_1$ is written for $\psi - \phi$. Again, the notation used in the vector equations on p. 11 is misleading without a bar over a_x and a_y . There are eight chapters in the book and the topics treated are indicated by their titles: Kinematics of Vibration. The Single Degree of Freedom System. Two Degrees of Freedom. Many Degrees of Freedom. Multicylinder Engines. Rotating Machinery. Self-excited Vibrations. Systems with Variable or Non-linear Characteristics. There is an appendix on the Gyroscope and one in which important formulae are collected.

The book is well illustrated with good diagrams. There are over one hundred problems, with answers given at the end of the book, as well as an extensive bibliography.

V. C. A. F.

Modern Gas Turbines. By A. W. JUDGE, A.R.C.S., D.I.C., Wh.Sc. [Pp. xi + 311, with 214 figures, including 19 plates.] (London: Chapman & Hall, Ltd., 1947. 28s. net.)

TO most readers the gas turbine is a machine which was thrust upon them by the late war, for few had interested themselves in this newest form of prime-mover prior to the advent of jet propulsion due to Whittle. Actually this turbine has as long a history as any form of engine, for the first machine of real importance was built as far back as 1894 by Armengaud and Lemale.

Descriptions of modern developments in gas turbines and their use have appeared in so many different publications that it is difficult for the average engineer to keep in touch with them. Mr. Judge has accordingly rendered a real service in his latest book, by giving in his usual lucid style a comprehensive discussion of the various types. The work is of an introductory character and gives an excellent outline of the subject.

After dealing with some general considerations, followed by a chapter on thermodynamics, the author discourses on efficiencies and how they may be increased. Under this heading he discusses the performance of compressors and the application of preheaters and heat exchangers in the various circuits. The corresponding cycles are given and a comparison is made between gas and steam turbines and certain forms of piston engine.

Mr. Judge is clearly of the opinion that the gas turbine is superior to the reciprocating machine for high-speed aircraft, but whether it will prove as reliable as some of the older forms of power unit over long periods of time has yet to be seen.

Other chapters discuss the use of exhaust gas turbines for supercharging oil engines, the general problem of gas turbines for aircraft and the question of performance.

Due to the rapid development of this subject, there is as yet no text available of the type of Stodola's *Steam Turbines*, but there is available a number of papers by specialists. The present volume, being of an introductory nature, will amply repay for reading prior to the study of such papers, to which an excellent list of references is given.

B. LLOYD-EVANS.

Aircraft Engines. Vol. II. By A. W. JUDGE, A.R.C.S., D.I.C., Wh.Sc.
Second edition. [Pp. ix + 513, with 390 figures, including 106 plates.]
(London: Chapman & Hall, Ltd., 1947. 36s. net.)

THE object of the present volume is to describe current (and to some extent past) practice in engine design in order to supplement the theoretical and experimental aspects discussed in Vol. I.

Any purely descriptive treatment of this subject runs some risk of superficiality, but by assembling a mass of information on the constructional features of engines and accessories, not readily found elsewhere, the book performs a useful function in providing a background of fact for the critical student who has already been well grounded in the fundamental principles.

Space limitations forbid the detailed comment which the book deserves, but in general one feels that the frankly descriptive presentation could have been improved by a rather more scientific approach. In Chap. I the somewhat vague discussion on h.p. per litre would have been given precision by a simple analysis based on permissible piston speed, leading to an explanation of the better criterion of h.p. per sq. in. of piston area. Chap. II contains a self-contradictory statement about a phase difference between harmonics of unbalanced force, and Chap. III devotes what would seem disproportionate space to C.I. engines which have found no significant application to aircraft propulsion. Chap. XII describes early experimental transmission dynamometers, but omits reference to the most important current type. Fig. 357 is over-simplified and Fig. 359 apparently inverted. The overall efficiency of the gas turbine is *not* (repeat, *not*) given by the product of compressor and turbine efficiencies (a fallacy repeated from Vol. I), and the reader will have difficulty in reconciling the value of 72.25 per cent. given for overall efficiency (p. 443) with the statement (p. 440) that the gas turbine is relatively poor in efficiency compared with the aircraft reciprocating engine. The misprints on p. 260 should be corrected. The style is too often wordy and unnecessary repetition is frequent.

E. GLAISTER.

CHEMISTRY

Modern Advances in Inorganic Chemistry. By E. B. MAXTED.
[Pp. viii + 296, with 25 figures.] (Oxford: at the Clarendon Press,
1947. 20s. net.)

It would be very illuminating to ascertain from chemists engaged in the field of inorganic chemistry what they consider should be the content of a volume entitled *Modern Advances in Inorganic Chemistry*. Certainly their views would differ. It is an open question whether the majority verdict would endorse the selection made by Dr. Maxted. This author opens his volume with a chapter of 80 pages which he designates "Physical Introduction." He deals in a very readable way with the newer physical conceptions which underlie modern chemical theory. The wide interest which has been aroused in recent years in the representation of simple molecules by molecular orbitals causes one to turn with eager anticipation to the treatment of this subject in the present volume. Although the author is very sound in his presentation, his account lacks the sparkle associated with the writings of Coulson in recent months on this matter. Then follow chapters dealing with hydrogen and its isotopes; the recent chemistry of the halogens and the systematic chemistry of hafnium and rhenium. Dealing with massium, the author emphasises that compounds of this elusive element have not yet been extracted in a pure state, although Dr. Maxted is of opinion that there seems no reasonable doubt of its presence in very small concentrations in certain minerals. Various radio-active isotopes of molybdenum can be formed from molybdenum and it may well be, as Dr. Maxted points out, that through the study of such products we may get the first experimental data about the element 43. Chap. VI contains some very useful material about reactions in discharge tubes. Recent work on active nitrogen and free hydroxyl is discussed, and the author describes the preparation and properties of the less-familiar oxides of nitrogen, phosphorus and sulphur. In the two final chapters some of the results of nuclear chemistry, with special reference to artificial radioactive elements, are fittingly dealt with. The volume is admirably produced with some excellent line drawings. It can be warmly recommended to those who are looking for a book which will give them sound and interesting information about some recent advances in inorganic chemistry.

W. WARDLAW.

Laboratory Experiments in General Chemistry and Qualitative Analysis. By GEORGE W. WATT. [Pp. viii + 223, with 47 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1946. 10s. net.)

This book is of a type that has not found wide favour in this country. It is a practical manual particularly intended for use in conjunction with *General Chemistry*, by Schoch, Felsing, and Watt. Each of the first forty-nine sections describes concisely an experiment, or series of related experiments, providing sufficient practical work for one period in the laboratory. Annotated spaces are left for the recording of results in the book itself, and for the answers to printed questions testing the student's appreciation of the significance of his practical work. The usual laboratory manipulative methods are covered, together with the chemical laws and simple quantitative chemistry,

the determination of density, viscosity, solubility, and molecular weight, simple acid-base titrations, and the preparation of a few simple gases and inorganic compounds. Some physico-chemical topics, such as reaction rate, chemical equilibrium, the ionic theory, and colloids, are dealt with in an elementary manner.

The second part of the manual is a detailed introduction to conventional methods of qualitative analysis, in which the cation groups and acid radicals are considered in succession, some preliminary sections being given to cover general reactions and techniques. The full explanation of the reactions used in analytical separations is a useful feature of the book.

The whole work provides useful material for elementary chemistry courses, but it does not correspond in scope with any normal examination syllabus in Britain. It is admirably compiled and well produced in a spiral-spring binding from which the sheets may be detached for filing. If the book does not find a ready sale in this country, the sole reason will be that our courses are so different in scope and method from those extant in America.

A. J. E. W.

A Practical Chemistry for Higher School Certificate and Intermediate Students. By G. P. McHUGH, M.Sc., Ph.D., M.Ed. Third edition. [Pp. viii + 119, with 17 figures.] (London, New York, Toronto: Longmans, Green & Co., 1946. 4s.)

THIS book represents an unusually successful attempt to provide, in a single volume of small size and reasonable price, the essential details of all the practical exercises indispensable to a Higher School or Intermediate course in chemistry. The three main sections of the text cover qualitative analysis, elementary volumetric analysis, and inorganic and organic preparations.

Qualitative analysis forms the subject of about half the book. The reactions of the individual metal ions and acid radicals are described concisely but adequately, and full equations are given for all important reactions. The separation tables follow conventional lines and are clearly set out; they are supplemented by excellent, though brief, accounts of the chemistry underlying the separation procedures. The volumetric analysis sections are concise, but form a sufficient basis for a laboratory course in which theoretical detail and manipulative techniques can be filled in by the teacher. The same is true of the final chapter on preparations, which should be used in conjunction with a systematic lecture course.

By reason of its strict limitation to the essentials of an examination syllabus, the book tends to lack coherent presentation of chemistry. Nevertheless, it should provide a most acceptable text in classes where its main function will be to form a simple basis for supervised practical instruction, and not to relieve the teacher of the systematic exposition of his subject. The book should be particularly acceptable to students who do not intend to specialise in chemistry.

A. J. E. W.

An Introduction to Quantitative Inorganic Analysis. By RONALD BELCHIE, F.R.I.C., M.Inst.F., and M. B. THOMPSON, B.Sc., Ph.D., A.R.S.M. [Pp. xii + 160, with 15 figures.] (London and Glasgow: Blackie & Son, Ltd., 1946. 5s. 6d. net.)

THIS book is noteworthy in that it is not a merely conventional addition to an already long list of textbooks of approximately Intermediate standard.

It has features which make it a genuinely useful accession to the literature of elementary analytical chemistry, and it can be recommended heartily to all students of Intermediate, Higher Certificate, and University Scholarship standard.

The authors quite rightly complain that "‘sloppy’ working is all too prevalent among students." Probably nothing but careful laboratory supervision is a fully effective antidote to this defect, but the emphasis on careful manipulation in this book is most refreshing.

The sections on gravimetric analysis precede the treatment of volumetric analysis, and are comprehensive for an elementary text; the determinations described include, for example, lead and phosphate (via ammonium phosphomolybdate) as lead molybdate, and methods for potassium and sodium. The long chapter on titrimetric analysis is thorough, and includes details of all the well-established general procedures; the determination of iron by means of mercurous nitrate is a less usual addition to the list. All the instructions for practical work are clear and concise, the theoretical introductions are adequate, and methods of calculation are sufficiently indicated. It is perhaps regrettable that the use of equivalents is avoided in some of the volumetric calculations, both the Preface and the Foreword (by Edwin Gregory) containing unjustifiably grim forebodings on the complexity of equivalents and normalities.

Separate short chapters are included covering the use of five of the more important organic precipitants in gravimetric analysis, and of four well-established colorimetric methods. The practical part of the book then finishes with a chapter on selected industrial methods of analysis, in which important determinations in the analysis of coal, steel, and water are described. This chapter serves very well to impress on the reader the importance of analytical chemistry in modern industry.

An Appendix is added in which the theory of all the main types of indicator is clearly and thoroughly discussed.

A. J. E. W.

Controlled Atmospheres for the Heat Treatment of Metals. By IVOR JENKINS, M.Sc. [Pp. x + 532, with 268 figures, including 54 plates.] (London: Chapman & Hall, Ltd., 1946. 50s. net.)

In the heat treatment of metals it is inevitable that the surface of the metal, if heated in the normal types of furnace atmosphere, should be affected by oxidation. Not only does this, with certain metals, spoil the surface, but it may be necessary to remove the resultant oxide. These effects lead to the need of additional processes to make good the surface, and the possible loss of valuable amounts of metal; both results entail increased costs of manufacture.

The measures taken to overcome these disabilities in the heating operation by the use of controlled atmospheres has led to an extensive range of processes and techniques. A considerable literature on the scientific aspects of practice has been developed as the number of ingenious methods of treatment have been multiplied. There was accordingly a distinct need for a book which would bridge the gap between the technical and practical features of the subject, and this has been met by the appearance of this useful volume by Mr. Ivor Jenkins, which has emanated from a famous source, the Research Laboratories of the General Electric Co. at Wembley.

The first section of the book describes the generation of controlled atmospheres. The author rapidly warms to his subject, with a lucid exposition of the leading modern methods of producing controlled atmospheres by all the best-known methods, from the most complicated ammonia dissociator or cracker to the most simple methods entailing the use of either town's gas or charcoal. At the same time the descriptions are interwoven with a presentation of the physico-chemical principles underlying the reactions, discussed in a manner which must be attractive alike to the scientist and the practical operator.

The extension of the use of controlled atmospheres has been assisted considerably by the improvement of technique in gas purification. This branch of the subject is competently treated, and to this discussion there is appended an effective summary of the modern methods of gas analysis, even to the point of reference to the use of vacuum technique and the micro-analysis of gases. This is perhaps not surprising when at least the research aspects of the subject require the application of such techniques. The matter of detailed manipulation is of necessity left to the more specialised type of volume.

The greater part of the book is, however, concerned with the physico-chemical and metallurgical background of the industrial applications of controlled atmospheres. The measure of detail is to be realised from the fact that 132 pages are devoted to the treatment of the carbon steels. The alloy steels by contrast receive less detailed treatment, but one must be fair to the author in realising that the fundamental principles involved are common to both. Here only has detail been discarded, for, whilst the stainless steels and other chromium steels offer a fruitful field for scientific treatment, the recommendation for treatment with freedom from oxidation is confined, perhaps advisedly, to cracked ammonia. There are excellent chapters on carbonising and nitriding, and, in regard to the latter, the scientific background is treated with a wealth of detail and modernity which is not to be found in any other volume. Nevertheless, no words have been wasted. The book comprises a work of reference, as well as an exposition of the subject.

In such a work one would expect to find a description of the bright heat-treatment of non-ferrous metals and alloys handled in the same complete and informative manner. The most discerning reader should not be disappointed. If there is any question of such a judgment, then his attention should be directed to the scope of the selected bibliographies given at the end of each chapter. These bibliographies alone, which accompany every chapter, should commend the book to the most fastidious seeker after knowledge.

The publishers, Messrs. Chapman & Hall, have made the book a worthy example of the bookbinder's and printer's art, particularly for these difficult times. The typescript is easy to read, and both diagrams and plates are of a high standard; indeed, a word of commendation is due to the technicians of the author's laboratory from which the many excellent photomicrographs must have emanated.

The book as a whole is a first-class achievement, the appearance of which should be welcomed by the scientist, the engineer and the production manager as well as the metallurgist.

R. J. S.

Organic Chemistry. By P. KARRER. Translated by A. J. MEE, M.A., B.Sc. Third English edition. [Pp. xx + 957.] (Amsterdam : Elsevier Publishing Co., Inc.; London : Cleaver-Hume Press Ltd., 1947. 38s. net.)

PROF. KARRER says in his Preface : " the chemistry of naturally-occurring substances and biochemical topics have been particularly emphasised. These topics have been the centre of interest in the last decade." Perhaps this last statement is true ; at any rate, the book can be recommended at once as an interesting and trustworthy introduction to the branch of organic chemistry mentioned. Many chemists nowadays, without being biochemists, are less interested in the molecules Nature has put in plants and elsewhere than in the way she may have set to work. So, because of the author's predilection for the natural molecules, and for dyestuffs, the book says very little about mechanisms, and, when it does, it leaves a good deal to be desired as a work suitable for students in this country. And similarly some of the intriguing unnatural molecules produced in laboratories are passed over rather quickly. In illustration of these tendencies we may point out that : the benzidine change is left where Jacobsen left it ; the diazoamino-aminoazo transformation is unexplained ; a cyclic structure is retained for the product of diazotising sulphanilic acid ; a three-membered ring structure is regarded as possible for diazomethane ; sulfoxides are given the old semi-polar double bond formulation ; and so on. In dealing with the formula for furan, the author says that " the oscillating linkage, the centric formula and Thiele's neutralisation of valencies in a conjugated system of double bonds have been put forward without being any nearer to the solution of the problem." So far, so good. But what of the sequel ?

Few would agree with the author's definition of diastereoisomerides, or find his introduction of the Fischer projection formula satisfying. Nor would they regard Debye's work on the chlorine-chlorine distance in chloroform and in methylene chloride as accurate, in view of the later conclusions of Sutton and Brockway.

The book touches on some industrial matters. An air of unreality is produced on occasion. Thus, perspex is not to be found among " the best-known artificial materials " : of the 42 trade-names listed under this heading only bakelite and " pollopas " are associated with British industry.

E. E. TURNER.

Unit Processes in Organic Synthesis. By P. H. GROGGINS, Editor-in-Chief. Third edition. Chemical Engineering Series. [Pp. xiv + 931, with 191 figures.] (New York and London : McGraw-Hill Book Co., Inc., 1947. 37s. 6d. net.)

NINE years have elapsed since the publication of the second edition of this valuable textbook and the appearance of the third edition is therefore timely. The term " unit processes " is used to cover all the factors in the technical application of an individual reaction in organic synthesis, and the various processes discussed cover nitration, amination by reduction, diazotisation, halogenation, sulphonation, amination by ammonolysis, oxidation, hydrogenation, alkylation, esterification, hydrolysis, the Friedel-Crafts reaction and polymerisation. Each chapter has been revised and brought up to date, and some 170 extra pages have been added since the previous edition. Of

the various subjects dealt with in the book, none has undergone such a rapid expansion during the war years as polymerisation and accordingly this chapter has been entirely re-written by Profs. Mark and Howenstein, the former of whom is generally accepted as a leading authority on the subject. Other changes from the previous edition include the greatly increased emphasis on the synthesis of aliphatic compounds, the incorporation of many new flow diagrams and the introduction of an appendix containing charts showing the various organic products derived commercially from methane, ethylene, propylene, petroleum and ammonia, and tables giving physical constants of a number of the compounds discussed in the text.

The principal appeal of this book will be to chemical engineers and, for those interested in chemical plant, there is contained a wealth of readily accessible and up-to-date material, supplemented by relevant economic appraisals of the various methods discussed. Each chapter is subdivided to cover the types of the respective reaction, the various reagents which may be employed, a discussion of the theoretical mechanism of the reaction, and finally a description of the technical applications and industrial processes, as well as the design and construction of equipment. The discussion of the reactions is of necessity rather limited and the theoretical treatment is even more so, but the material is supplemented by numerous references to the literature.

The valuable accounts of these common organic processes, together with the critical description of the methods of carrying out the reactions, should have a wide appeal to all organic chemists. At the same time, the ample treatment of recent advances in chemical technology will ensure that the volume finds a place in all industrial chemical libraries.

A. W. JOHNSON.

The Chemical Constitution of Natural Fats. By T. P. HILDITCH, D.Sc., F.R.S., F.R.I.C. Second Edition. [Pp. xiii + 554, with 114 tables.] (London: Chapman & Hall, Ltd., 1947. 45s. net.)

THE value of this book has already been clearly recognised by the rapid exhaustion of the first edition, published in 1940. It resounds to the credit of the author and publishers that a revised second edition, of such excellent quality, has been produced so soon after the end of hostilities. Due in part at least to the stimulation of the first edition, advances made during the war years have necessitated an increase of one hundred pages.

Prof. Hilditch breaks largely new ground by developing the theme of the relationship of the chemical constitution of natural fats to their biological origin. The chemical and physical analysis of fats are, therefore, only considered with this end in view, and hence the book does not infringe upon the many technological treatises on the fats. More than half the book is devoted to the correlation of the distribution and composition of the component acids and glycerides with their origin; and only then is the structural chemistry of individual acids and glycerides considered. This treatment leads to a reversal of the older method of classification starting with the fats of plant seeds. The data assembled here show that natural fats align themselves in groups, by their component acids, according to their biological origin; thereby revealing that the fats of the simplest and most primitive organisms are made up from a complex mixture of fatty acids, whilst, as biological development has proceeded, the component acids of the fats of

the higher organisms have become fewer in number. Hence the sequence aquatic flora, aquatic fauna, land animals, and land plants is adopted.

An unusual but valuable addition to the book is the inclusion of a detailed description of the experimental methods developed in the Liverpool laboratories during the past twenty years. Methods of low-temperature fractional crystallisation and of vacuum fractional distillation are given, which will be most useful to workers in this and other fields. The description of the low-temperature technique would have gained by illustration of the apparatus employed.

Prof. Hilditch's book is to be strongly recommended as a noteworthy contribution to the organic chemistry of natural products and as an outstanding achievement of British authorship and publishing.

S. H. HARPER.

Advances in Protein Chemistry. Vol. III. Edited by M. L. ANSON and JOHN T. EDSALL. [Pp. xii + 524, with 42 figures.] (New York: Academic Press, Inc., 1947. \$7.50.)

It might have been expected that after two volumes of reviews on protein chemistry had already been published there would be few subjects in the field that had not already been reviewed; however, in this third volume there is very little repetition of what has gone before. In fact, it is probably the most interesting and useful of the three. There does not appear to be any special branch of protein chemistry dealt with, as in the previous volumes, and the articles are concerned with all aspects of proteins in nutrition, metabolism and structure, and give an excellent overall picture of the state of protein research to-day.

In the field of protein metabolism the importance of the dicarboxylic amino-acids is becoming more and more clear, and the first review by A. E. Braunstein on "Transamination and the Integrative Functions of the Dicarboxylic acids in Nitrogen Metabolism" deals with this very dynamic aspect of protein chemistry. Although this is a favourite subject for review, this article is of special value as it comes from the discoverer of the transaminating system and is a clear and stimulating exposition of the subject.

The next article by L. Michaelis is a short and concise review of the chemistry and metabolism of one of the lesser-known proteins—"Ferritin." This protein is remarkable for its high iron content, and presents a number of interesting problems to the protein chemist.

The chief "bottleneck" to advances in protein structure is probably the elucidation of new methods of study, especially for the fractionation of protein breakdown products. One method on which work is being started and should prove of great value is adsorption analysis, and this is very clearly reviewed by A. Tiselius, who is putting on a scientific basis a method which has been used somewhat empirically in the past.

The next two articles deal with "Protein Films." The first by H. B. Bull summarises the methods and results obtained with protein films to date. The second by A. Rothen is of special interest for the somewhat revolutionary ideas expressed in it. If these are true, we must regard reactions between proteins as being able to take place over a considerable distance, in contrast to the more usual organic reactions. While it is felt that this is not completely proved, it is certainly worthy of consideration and further study.

One of the first requisites for a chemist who is working on a new com-

pound is to have a method for estimating it. This has always been a difficulty for the protein chemist, as is illustrated in the article on "The Chemical Determination of Proteins" by P. L. Kirk, who reviews the various methods that have been used for this purpose.

"The Reactions of Native Proteins with Chemical Reagents" is ably and comprehensively reviewed by R. M. Herriot. This method of investigating protein structure by the more standard techniques of organic chemistry have been rather neglected in the past, but it is probable that our knowledge of proteins has now advanced far enough to enable us to use such methods with some success.

The next review by A. A. Albanese on the "Amino-acid Requirements of Man" consists, firstly, of a summary of various work on the protein requirements of humans of various ages, from which it is difficult to draw any definite conclusions, and secondly of a review of the authors' own work on feeding humans with synthetic diets, which seems the only satisfactory approach to the subject.

"The Use of Protein and Protein Hydrolysates for Intravenous Alimontation" is ably dealt with by R. Elman, though some of the clinical results may be rather lost on protein chemists. This method of treatment has been introduced recently, and the uses and limitations are well illustrated in this review.

The next article on "Preparation and Criteria of Purity of Amino-acids," by M. S. Dunn and L. B. Rockland, is more a work of reference than a review, and as such should prove very useful. Details are given for the preparation of the 21 naturally occurring amino-acids and there are 886 references.

The final review by J. T. Edsall on "The Plasma Proteins and their Fractionation" centres round the low salt-ethanol method of fractionating plasma that has been developed at Harvard. The article is extremely interesting and clear, but it seems premature to review a subject before the experimental details of the method are published.

F. S.

Concise Chemical and Technical Dictionary. Edited by H. BENNETT. [Pp. xxxix + 1055.] (Brooklyn, N.Y.: Chemical Publishing Company, Inc., 1947. \$10.00.)

THIS new dictionary is attractively produced, well-printed, easy to use, and there are few, if any, printers' errors. In planning it, however, insufficient regard has been paid to the purpose it is to serve. It is not etymological, like Chambers' *Technical Dictionary*, and there are no long articles under, for instance, "Dyes" such as in Haack's *Chemical Dictionary* or Miall's *A New Dictionary of Chemistry*, nor, like these two last, does it contain biographical articles. It is, however, more generally useful than Miall's and, in regard to trade names, it compares well with Gardner's *Chemical Synonyms and Trade Names*. In this respect it is reasonably comprehensive, especially from the American point of view; among the few trade names the reviewer has missed are "terylene" and "abavit." Much of the supplementary information given in the Merck Index, however, is absent, and although, like the latter, it includes by name numerous reactions, such as the Fries reaction, this feature is less complete than the list given in Merck and could well have been extended.

On the other hand, the dictionary attempts to venture rather too much

into the handbook field, as distinct from that of the dictionary proper. It contains, for example, a section on the nomenclature of organic chemistry based on the International Rules, an alphabetical list of names and formulae of radicals, based on that given in the Decennial Index to Chemical Abstracts, as well as a reprint of the report of the Nomenclature, Spelling and Pronunciation Committee of the American Chemical Society on the pronunciation of chemical words, and an alphabetical list of important organic ring systems. There is also an alphabetical list of indicators, a list of the chemical elements and their discoverers, temperature conversion tables, comparisons between U.S. and metric measures, and lists of Greek and of mathematical symbols. How far these are usefully included in a dictionary is a matter of opinion, but there are many archaic chemical names in the text which could well have been omitted. Much of the information included under the names of chemical compounds could also have been excluded. No chemist, for example, would consult a dictionary of this type for the molecular weight or melting point of dianisidine. For such information he would invariably have recourse to *Lange's Handbook of Chemistry* or *Hodgson's Handbook of Chemistry and Physics* or like works.

Some of the descriptions, too, are incomplete, "Castrix" being an instance: it would have been simpler to complete the description of this product as 2-chloro-4-dimethylamino-6-methylpyrimidine than to leave it as "Rodenticide made from acetoacetic ester and urea reaction product by chlorination and amination." "Lucex" is also inaccurately described, and under "Chromotrope 2B" the reader is referred to "*p*-azo-nitro benzene," under which, however, there is no entry, instead of the description being completed as "the coupling product from diazo *p*-nitro aniline and chromotrope acid." Generally, it may be said that the description of trade names of dyestuffs is defective, and it is rarely indicated that such names as "Durafur" and "Duranol" indicate a range or brand of colours and not a single dyestuff. There is a spelling mistake of indigisol instead of indigosol, and the definition of phthalocyanine is inadequate and might well have been elaborated.

On the whole, it may be said that there is a failure to differentiate between trade names, which may also be registered trade marks, such as "Alkathene," and names for general use such as "Polythene," and authority might well have been cited for some definitions such as "Chalkogen" and "Elastomer." In contrast, the structure of quite simple compounds such as "lactam" and "lactone" is lavishly indicated and illustrated. While the definition of "rayon" is good and well indicates the difference between present British and American practice, other definitions are too clipped, that of "autoclave," for instance, confusing the industrial meaning and the pharmaceutical usage.

A feature that mars the dictionary is the inclusion in the text of innumerable abbreviations. These should have been segregated, for no reader, for instance, would expect to find "B.A. British Association" in the text of the dictionary itself.

R. BRIGHTMAN.

An Introduction to Textile Finishing. By J. T. MARSH, M.Sc., F.R.I.C., F.T.I. [Pp. xvi + 552, with 161 figures, including 27 plates.] (London: Chapman & Hall, Ltd., 1947. 35s. net.)

WHEN taken from the loom or knitting frame textile fabrics are usually dirty and greasy, and unpleasant to handle. They are consequently submitted to operations collectively referred to as dyeing and finishing, as a result of which they are given the attractive appearance and handle characteristic of the modern commercial product. A detailed account of processes such as scouring, bleaching and drying is given in the author's earlier work—*An Introduction to Textile Bleaching* (Chapman & Hall, Ltd.)—and this new publication on textile finishing is therefore concerned with creping, mercerising, parohmentising, acid finishes on cotton, setting processes such as crabbing and blowing, milling wool fabrics, methods of producing unshrinkable wool, cotton and rayon fabrics, softening, starching, weighting silk, delustring—a process particularly important in processing certain rayon fabrics—resin finishes, e.g. the anti-crease finish for cotton and rayon, rubber and resin coating, waterproofing, mothproofing wool, fireproofing and mildew-proofing.

Until comparatively recently, finishing was regarded solely as a craft, but during the last thirty years the increasing application of scientific thought to industrial problems and the great advances in our knowledge of the structure of textile fibres and of the nature of well-established textile processes have created a completely new outlook towards finishing. That the importance of the chemist in this field is now well established is evident from the large number of patent specifications referred to in the present volume. As a practising textile chemist over a period of some twenty-five years, Mr. Marsh has made considerable contributions to the subject of finishing, and is admirably qualified to produce an authoritative work on this branch of textile processing. The treatment of the subject matter is essentially scientific and some may regret the absence of practical details, but had these been included the book would have become unwieldy. Moreover, in an introduction to textile finishing emphasis should be laid on principles—the science of each process and the chemistry of the reagents employed—and presumably it is for this reason that considerable space is given to discussing the action of formaldehyde on textile materials and the chemistry of synthetic resins. This book is probably the first to present the new approach to the finishing of textiles and its publication may be regarded as a major event. It will be useful to research workers, especially to those pure chemists who are commencing work in the applied science of textile chemistry, and it contains a wealth of information which will be invaluable to both students and technologists. It is a book which should be read by all who are interested in textiles, and not least by educationalists who plan schemes of work for textile students, for it emphasises indirectly the great importance of ensuring that those intending to play a leading part in this important branch of the textile industry should have a sound knowledge of basic science.

In a book which is so outstanding in scope and in quality of material it is perhaps pedantic to comment on a minor point. Although the publishers have, on the whole, maintained their high standard of production—the type is clear and neat, the plates and line drawings are well reproduced, and the binding is good—it is unfortunate that in a number of cases formulae

are given in type which is out of keeping with the rest of the text. This is particularly noticeable on pages 260-7 where three different styles of type are employed without any obvious reason. This might not be considered important if the volume were for home consumption only, but, since there is little doubt that the book will be in great demand abroad, every effort should be made to maintain the traditionally high standard of British scientific publications.

C. S. WHEWELL.

Matthews' Textile Fibers. Fifth edition, prepared by a Staff of Specialists under the Editorship of HERBERT R. MAUERSBERGER. [Pp. xvi + 1133, with 353 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 75s. net.)

LITTLE more than the name remains of this classic reference book, the last edition of which prepared by the original author was published in 1924. Since then there have been so many developments in the textile world, with small chance of any one author having sufficient knowledge to deal with the whole field, that it was decided this time to compile an effectively new work as a co-operative effort of forty-seven specialists. The reader must look no longer, then, for anything like the old familiar "Matthews," but rather for something more abreast with the rushing moderns.

But not too up to date, though, at least as regards the molecular aspects of fibre structure, in which there is a certain inequality between one contributor and another and perhaps a certain diffidence and sketchiness, particularly in matters of the proteins. The contributors are predominantly technical people, so this is probably excusable in a work of this kind, but we do wish they would disabuse themselves of the idea that nylon is by way of being an artificial protein. On the more technological and industrial side the book is as valuable as ever; in fact, it is not aimed at fundamentalists but is, as it claims, "a textbook for colleges and technical schools, and also a reference book for all those who deal with the sale, production, processing, and conversion into fabric of all the different fibers now used in the textile and clothing industries." This new edition, by the way, has dropped the analysis, testing, and processing of fabrics, and also the waterproofing and fireproofing of textile materials, and rubber and metallic yarns have been omitted.

It is good to see "Matthews" back again in modern dress, and, to perpetuate the old phrase, no library should be without it—and no textile person either, if he or she can afford it!

W. T. ASTBURY.

The Technology of Industrial Fire and Explosion Hazards. Vols. I and II. By R. C. SMART, M.C., M.I.Min.E., F.R.Econ.S. [Vol. I: pp. 202 with 5 figures; Vol. II: pp. x + 184, with 2 figures.] (London: Chapman & Hall, Ltd., 1947. 16s. net each.)

THE industrialist will read with satisfaction the title of these two volumes. "Here," will he say to himself, "the risks that I, my staff and my Insurance Company have to take will be fully explained and my development programme can receive at the outset a guide to safety."

With such thoughts in his mind he will open the book. At once, we feel, his sense of well-being will fail. Instead of the technical aid he seeks,

there will confront him a series of promiscuous jottings which might have as an alternative title, "Fires and their Extinguishment I Have Known." He may, if he is not a warm supporter of the famous A. P. H., even pass the word "extinguishment" with little more than a shudder, but it is very doubtful if he will read with comfort a work in which the minor conventions regarding the use of stops and capital letters have been so consistently ignored. Neither has the author been assisted by his proof reader having left here and there a typographical error in the text.

Misled by the title, even an occasional chemist may get hold of this work which, he could rightly assume, would deal, at least in part, with the chemistry of combustion. He will wonder why it is necessary to use the symbol Cl for chlorine. He will probably agree that air is a supporter of oxidation, but may well wonder whence comes the phosphorus from the average sawdust. Silicate of cotton might puzzle him, as well may the author's observation regarding the oxidation of carbon dioxide to oxygen, but, if he feels that as a result life is no longer worth living, he could always investigate the manufacture of nitro-glycerine by spraying glycerol with mixed acids.

By way of recompense, however, the chemist may be interested to learn the precise composition of the explosive Cheddite, that Menhaden oil is prepared from a fish of the family name Clupeidæ and that the Greek for "amber" is "elektron." Long-suffering, he may at this stage discard the volume in favour of lighter reading, and his reactions will probably be much less violent than those of the farmer who, carefully carrying round with him during hay time a large Centigrade thermometer, discovers to his horror that the temperature gradient in his stack has exceeded 60° C., when, of course, he has to suspend operations and allow the stack to cool.

It is particularly unfortunate that, while there is undoubtedly a need for some authoritative work of this nature, Mr. Smart should have presented his facts and knowledge in such extreme disorder and with such scant regard for the sciences to which he refers. His presentation is not aided by the eleven references in the text to one of his own papers, and, whereas we feel that in this work there is a certain amount of useful information, the labour of sorting the wheat from the chaff, aggravated by the errors both of science and style, makes the two volumes of but little value.

W. P.

GEOLOGY

Outlines of Palaeontology. By H. H. SWINNERTON, D.Sc., F.Z.S., F.G.S. Third edition. [Pp. xii + 393, with 368 figures.] (London: Edward Arnold & Co., 1947. 30s. net.)

In the present scarcity of scientific textbooks, this new edition of the *Outlines* will be welcomed by all teachers of Palaeontology who are familiar with the earlier issues. Several changes have to be noted; some chapters have been recast, others amplified, and a few explanatory figures added, while the book has been brought thoroughly up to date in respect of recent research, nomenclature, and the somewhat voluminous literature which has appeared since the last edition was issued. The result is a textbook eminently suited to the needs of advanced and honours students of Biology, who may be expected to possess the necessary geological and biological background and training for a full appreciation of the book.

The author's aim has been to get away from the old concept of Palae-

ontology as the handmaid of Geology, a compilation of genera and species of special value as stratigraphical indices, and to place it in its true position as the foundation of Biology, a study of the life of the past, of the structural modifications apparent in a race of organisms throughout their geological history, whether due to environment or inherent in the organism itself. In the present state of our knowledge of the causes underlying these changes such a method is bound to lead to controversy, but experience has shown that therein lies the chief merit of the book, a stimulus to the student to think for himself, to question, to formulate and discuss his own ideas on the subject.

The field covered is a wide one ranging from Protozoa to Man. Wisely the author has dealt in greater detail with groups which illustrate the principles he is seeking to establish, such as Echinoderms and Cephalopods, yet on the whole he has succeeded in giving a balanced presentation of the subject.

Print and production are excellent, and the publishers are to be congratulated on a book produced under present-day conditions of stringency.

T. M. F.

The Pulse of the Earth. By J. H. F. UMBROVE, D.Sc. Second edition. [Pp. xxii + 358, with 5 coloured and 3 other plates, 2 folded tables and 204 figures.] (The Hague: Martinus Nijhoff, 1947. Gld. 20.-.)

PROF. UMBROVE has long been known for his inspiring and thought-provoking studies of the evidences of periodicity in the behaviour of the earth. Some of these studies were gathered together into a masterly synthesis which appeared in 1942 as the first edition of *The Pulse of the Earth*. Others, developed during the occupation years, have now been incorporated into a second edition which, with its several new chapters and other additional material, including 123 new illustrations and tables, is double the size of the first. Since the first edition was a limited one and not generally accessible, the second is virtually a new book.

The introductory chapter deals with the earth in space and time, and includes a recently revised geological time-scale which serves as a qualitative background for the various cycles that come under discussion. Then the author surveys in turn the pulsation of folding and mountain building; the characteristics of basins and troughs and their chronological relations to other tectonic phenomena; the nature and behaviour of the crust and substratum and the metamorphic and magmatic cycles associated with their movements; the oscillations of sea-level; the features of the continental margins, including marine terraces and submarine valleys; the problems of island arcs—tectonic, isostatic, volcanic and evolutionary; the nature of ocean floors and the origin of continents; ice ages and the periodicity of climatic changes; the rhythm of life; and finally the significance and modes of origin of the linear patterns to be seen in the face of the earth. The concluding chapter summarises the chronological and genetic relations of the various periodic events and raises the fundamental question of the inner mechanisms responsible for the astonishing recurrence of phenomena in the long procession of terrestrial history. The author is sympathetic to the now familiar hypothesis of subcrustal convection currents, but he wisely adds: "Nevertheless, the mysterious interior of the earth involves more than just

a single periodic process. It is far more complicated than the tentative theories have supposed so far." With this conclusion even the most enthusiastic convectionist will whole-heartedly agree.

Prof. Umbgrove has an enviable flair for recognising problems, for presenting and illustrating the relevant data with unusual clarity, and for discussing interpretations with zeal tempered with scientific discretion. The book is extraordinarily interesting and is a delight to read. To avoid cluttering up the body of the text with too much technical detail, a summary of observational facts has been assembled in an Appendix. This amplifies the description of a fine series of beautifully produced Plates and Tables, which themselves are particularly welcome as a graphic synthesis of our present knowledge of the structure and history of the earth's crust. As to the hypotheses specifically—though cautiously—favoured by the author, it is only in the nature of the subject that readers with a different background of geological experience may find occasional reasons for holding diverse views. What is more important is that no reader can fail to be richly rewarded by the lively blend of correlated facts and stimulating discussion which make up this highly original book. Geologists and geographers throughout the world have reason to be grateful to Prof. Umbgrove for an outstandingly important contribution to the literature of the science of the earth.

A. H.

Limestones of Canada: Their Occurrence and Characteristics.

Part V. Western Canada. By M. F. Goudge. [Pp. x + 233, with 12 tables, 40 plates, 6 figures and 2 maps.] (Canada: Dept. of Mines and Resources, Mines and Geology Branch, Bureau of Mines, No. 811, 1945. 50 cents.)

THE report on the "Limestones of Canada," of which this work is the fifth part, is based on a survey of the limestone resources of the country, with a view to ascertaining the physical and chemical nature of the rocks, methods of quarrying and preparing the stone for the market, technology of lime manufacture, and the uses of lime and limestone in industry. The importance of limestone to Canadian industry may be gauged by the fact that in 1943 nearly 10 million tons of limestone (including marble) were obtained from the quarries, of the selling value as manufactured lime, etc., of 24½ million dollars.

Reports previously published have been: I. Introductory; II. Maritime Provinces; III. Quebec; IV. Ontario. The present report deals with the limestones of western Canada. After an introductory chapter on limestones in general, it describes in successive chapters the deposits in Manitoba, Saskatchewan, Alberta and British Columbia. These limestones include practically all types from pure high-calcium limestones, through magnesian limestones, to pure dolomites. They range in age from Pre-Cambrian to Jurassic; but Ordovician, Silurian, Devonian and Carboniferous limestones in widespread flat-lying beds provide most of the output.

The memoir is well written and arranged, and is illustrated by a number of excellent plates.

G. W. T.

The Canadian Mineral Industry in 1945. Reviews by the Staff of the Bureau of Mines. [Pp. v + 103.] (Canada : Dept. of Mines and Resources, Mines and Geology Branch, Bureau of Mines, No. 820, 1946. 25 cents.)

ALTHOUGH a large part of the enormous area of Canada is still unsurveyed for minerals, it is already safe to say that Canada is one of the richest mineralised countries on the globe. The report here reviewed fully confirms this view. It is divided into three parts, respectively, I. Metals; II. Industrial Minerals; III. Fuels. Twenty-four metals are listed. The production of nickel, of which Canada still supplies 80 per cent. of the world's total output, has, of course, suffered a decline with the end of the war. Although there is no bauxite in Canada, the Canadian aluminium industry is the second largest in the world. Low-cost hydro-electric power, situated at coastal points where the raw material derived from Empire sources can be conveniently assembled, is the secret. Experiments in the utilisation of clay, shale, nepheline-syenite, and anorthosite with from 20 to 30 per cent. of alumina, abundant in Canada, are in progress. Is it significant that uranium is missing from the list of metals?

The industrial minerals (non-metallic) include an amazing variety of substances, of which "roofing granules" was a "new one" on the reviewer. This turned out to be asphalt sheeting peppered with small particles of rock, or slate, artificially coloured. The importance of this industry may be judged from the fact that 77,559 tons of rock were mined for this purpose in 1945. Such items as this provide interest in an otherwise arid collection of facts and figures, which testify, however, to the rapid growth of the mineral industry in Canada.

G. W. T.

BOTANY

Introduction to Plant Ecology. By A. G. TANSLEY, M.A., F.R.S. [Pp. 260, with 15 figures.] (8s. 6d. net.)

Practical Field Ecology. By R. C. McLEAN and W. R. IVIMEY COOK. [Pp. 207, with 50 figures.] (9s. net.)

Plant Ecology and the School. By A. G. TANSLEY, M.A., F.R.S., and E. PRICE EVANS, M.Sc. [Pp. 97, with 10 figures.] (6s. net.) (London : George Allen & Unwin, Ltd., 1946.)

THE three books which are included in this notice are all of value to those interested in acquiring or teaching the elements of plant ecology. The first one by A. G. Tansley is based on an earlier work by the same author, *Practical Plant Ecology*, which is now out of print. Much of the text in the original work has been retained, but additional chapters have been added and, in view of the impending production of the second of these books by Prof. McLean and Dr. Cook, a great deal of the practical details has been omitted. The new part of the book is largely an extension of the ideas relating to plant communities and plant successions, and there is an expansion of the section dealing with the life form of plants. The principal additional feature of importance has been the revision of the treatment of the plant communities on peat which has been brought into line with recent advances in knowledge.

The original work was for long a useful source of information about the aims and methods of ecology and, in its present form, it should continue to be equally acceptable.

Practical Field Ecology is a survey of the methods employed by plant ecologists in the field and, compiled as it is by authors with great experience in teaching this subject, it is an extremely valuable survey of methods which can usefully be employed in the field. The survey methods include those required for mapping, levelling and the construction of transects as well as profile charts. There are separate chapters on soil determinations, and on limnology as applied to ponds and streams, as well as the ecology of the seashore. Another chapter deals with the determination of climatic factors such as light intensity, evaporation rates and rainfall. Three keys are appended for determining species of British grasses by their vegetative characters or determining the chief genera of British marine Algae and the common genera among the higher Fungi. This is an extremely valuable book at a very modest price.

The third of these books deals with such parts of the study of plant ecology as can be usefully made in schools. The emphasis perhaps in this work is laid on the relationship between plant ecology and regional geography, although there are many practical hints as to the type of field work which can usefully be done by students in schools. A list of references is given to other sources from which fuller information can be obtained. This little work represents an interesting experiment and, in conjunction with the other two books already mentioned, gives a survey of practically the whole of the field in which ecological ideas can usefully be taught.

W. H. P.

Peat Moss Deposits in Canada. By HARALD A. LEVERIN. [Pp. vi + 102, with 9 plates and 10 figures.] (Ottawa, Canada: Department of Mines and Resources, Mines and Geology Branch, Bureau of Mines, 1946. 25 cents.)

THIS report contains a geographical assessment of the peat moss resources of Canada, based on a five years' survey from 1938 to 1943. It may be noted that the term *peat moss* is used here, as in the trade in this country, to mean any relatively less-humified peat, whether formed from sphagnum moss or from other fibrous plants such as cottongrass, reeds or sedges—the kind of peat used in horticulture for soil conditioning, or as litter and the like where absorbing power is important, rather than the denser kinds of peat generally used as fuel. Such peats consist of the remains of sphagnum moss (good horticultural peats), of *Sphagnum-Eriophorum* (good litter peats) or of sedges and reeds which form only lower-grade peat moss for various reasons, and wood or heath residues which are unsatisfactory, but may be marketed with other fine material in the form of peat flour or mull.

The locations and general descriptions of the actual survey, which take up four-fifths of the report, are more of practical than scientific interest, although they provide some ecological and geographical data which are not without interest for comparison with our own types. Practically, they indicate Canadian peat moss resources of some 80 million tons. Of the corresponding resources of this country we have only very vague ideas, since adequate peat survey has not yet been attempted.

The development of the industry in Canada during the war years is very

striking. Due to the cutting off of European supplies from the United States, Canadian exports rose from 4000 tons in 1938 to 63,000 tons in 1944. Of this 41 per cent. was used as litter, 39 per cent. in horticulture and 20 per cent. in metallurgy, the remaining 0.5 per cent. being taken up in small amounts for insulation, etc.

The introductory pages deal with the technology of moss peat, stress being laid on low ash content and high absorbing power, and the tests used for these and other properties are described. (Reference should be made to the inclusion in the report of the American standard specifications for these peats—the chaotic condition of the trade in this country is due to the absence of any such specified standards.) Short accounts along the usual lines are given of the methods in use for the selection of bogs and for the winning, harvesting and processing of peat moss (for the cutting of which hand-labour is preferred), and of its uses in agriculture, horticulture, industry and in the home. For assessing the relative importance of these uses, the percentages of American imports quoted above might often be used in this country with advantage.

G. K. F.

Mother Earth. Being Letters on Soil addressed to Sir R. George Stapledon, C.B.E., M.A., F.R.S. By G. W. ROBINSON, M.A., Sc.D. [Pp. viii + 201, with 8 figures, including 2 folding plates.] (London: Thomas Murby & Co., 1947. 9s. 6d. net.)

It is a pleasure to find that Prof. G. W. Robinson's delightful book *Mother Earth* has been reprinted. It is what it aims to be, a series of philosophical essays covering the main aspects of our knowledge of soils, and should appeal to all who have an interest in the countryside. Although Prof. Robinson specifically states in an introductory chapter that it is not intended as a textbook, students will undoubtedly find it useful as an introduction to the subject.

Some of the popular books on soil which have appeared in the past few years are propagandist in character and scientifically quite unsound, and it is to be hoped that *Mother Earth* will help to counteract the influence of such literature. It gives in simple and scholarly language the modern views about soil and presents the facts in such a way as to stimulate further thought. The layman should be able to follow the book with ease and to obtain from it a sound understanding of the principles of soil science and of its applications to agriculture.

Mother Earth is written in the form of a series of letters. Personally I should have preferred it as a collection of essays, but that is a very minor point and does not affect the excellence of the matter contained in this little book.

W. G. O.

BIOLOGY, BIOCHEMISTRY AND PHYSIOLOGY

Essentials of Biology: An Introductory Text-book for Secondary Schools. By W. F. WHEELER, M.A. [Pp. xii + 286, with 8 plates and 216 figures.] (London: William Heinemann, Ltd., 1947. 7s. 6d.)

THIS book contains much valuable material, covering, with the exception of the natural history and ecology sections, the syllabuses for General School

examinations in Biology and the biological sections of the General Science examinations (two credits).

The reading matter is concisely worded and interesting, and any candidate of average ability should be able to use it without much help from the teacher, but the book seems too academic in parts for General School candidates, and, in spite of the author's comment that "necessary technical vocabulary should never exceed minimum requirements," there are too many technical terms in black type in the text, and even more on some of the drawings.

Most of the book is divided into Zoology and Botany, but some integration is attempted in the last chapters. More attention than is usual is given to human anatomy and physiology, with a corresponding cutting of the work on the rabbit. Apart from a few odd points, the plant physiology is dealt with much more efficiently than is usual in a General School textbook, and the chapter makes interesting reading. The section on bacteria, with its suggestions for practical work, is good. Illustrations are also good.

Teachers may well feel that Dogfish is not too suitable as a type for practical work with present-day classes of 35 or more students. The dogmatic statement on p. 137 on the development of root tissues from histogens would be better omitted.

It is a pity that the author gives no help with fieldwork, which must always provide the dynamic interest underlying all good biological work with young students, and one feels that much of the anatomical work on earthworm, frog and cockroach could have been sacrificed to make room for some practical guidance on field studies.

J. PHEASANT.

Radioactive Tracers in Biology. By MARTIN D. KAMEN, Ph.D. [Pp. xiv + 281, with 38 figures.] (New York: Academic Press, Inc., 1947. \$5.80.)

FOLLOWING the recent declaration by President Truman that radioactive isotopes are to be released for distribution to foreign research workers, and the announcement that a uranium pile is now operating in this country, many people will soon have an opportunity to use these materials. They will be well advised to acquire this book. This advice can be given despite the fact that the presentation is far from lucid and there is an unusual number of errors, many additional to those shown in the errata sheet and some which seriously confuse the text. The book owes its indispensability to the fact that in it both biological and physical fields are skilfully interwoven by one who is clearly master of both. It will in many instances be found side by side with another recently published manual on the stable isotopes*.

The introductory chapter on atomic properties is perhaps the most disappointing, especially to the biologist who is plunged into such complicated subjects as positron- and K-emission without even an outline of the properties of electrons or gamma rays. Thereafter the author warms to his subject, and Chapter II, on the production of radioactive isotopes, includes a particularly useful section on internal conversions associated with nuclear isomerism. Range-energy relationships for homogeneous and continuous beta radiation are clearly dealt with, but the scattering and absorption of gamma rays receives only superficial consideration. A conventional but none the less

* *Preparation and Measurement of Isotopic Tracers*, by D. W. Wilson, A. O. C. Nier and S. P. Reimann.

useful treatment of G-M counters follows, and leads up to a masterly survey of "tracer methodology." This chapter contains extracts from numerous outstanding biological papers, and in one or other of these the reader is likely to find an analogue to his own problem and thereby to be forewarned of difficulties and pitfalls. The remaining chapters deal with the more popular tracers in turn under the sub-headings of preparation and properties, assay, and applications. Much of the information on the physics and chemistry of cyclotron targets has not been published hitherto. The section on the assay of tritium and the numerous organic syntheses for incorporating carbon and sulphur into biological compounds are particularly valuable.

A. S. McF.

The Artificial Insemination of Farm Animals. By E. J. PEARRY and others. [Pp. vi + 265, with 47 figures.] (London: Chapman & Hall, Ltd., 1947. 15s. net.)

WITH the increasing use now being made of artificial insemination in the ordinary practice of breeding farm animals—some 60,000 cows will be inseminated in this country this year as compared with about 25,000 last year—the appearance of a textbook on the subject is most opportune. Not only are technical details given for all classes of farm livestock, cattle, horses, sheep, pigs and poultry, each written by an expert in the subject, but each chapter contains good illustrations and references to original publications, so that the student can easily obtain all the details he requires from the very scattered literature. In addition to describing the technical methods adopted for each class of livestock, much information on reproductive physiology is included, for, especially in horses, the timing of the insemination in relation to the time of ovulation is important. Considerable space is also allocated to the selection of sires, systems of breeding and to methods of livestock improvement, for this is the main purpose for which artificial insemination is now being used in farm practice. A chapter on disease is also included, as artificial insemination has proved most useful in controlling the spread of such venereal diseases as dourine in horses and trichomonas infection in cattle, which is the cause of much abortion and sterility in cattle.

The details given in this book serve to illustrate the interrelation between the pure and applied sciences. The original scientific discovery of the possibility of artificial insemination led to very little progress being made in the science of the subject until attempts were made to apply its use to the breeding of farm animals. It was then discovered that there exist very great differences between species in the length of time spermatozoa could be kept alive outside the body, showing that innate differences in metabolism are involved. It has also been found that sperms suffer from temperature shock and that this can be reduced by the addition of egg yolk to the semen. Thus many questions of fundamental scientific importance are raised which form starting-points for new fields of work in pure science.

J. H.

Advances in Genetics. Vol. I. Edited by M. DEMMEBO. [Pp. xvi + 458, with 41 figures.] (New York: Academic Press Inc., 1947. \$7.50.)

ACCORDING to the Editor's Preface, "This series of review articles, *Advances in Genetics*, has been started in order that critical summaries of outstanding

genetic problems, written by competent geneticists, may appear in a single publication. The articles are expected to deal with both theoretical and practical problems, and to cover plant breeding, animal breeding, and human heredity, as well as the related fields of biophysics, biochemistry, physiology, and immunology. The aim is to have the articles written in such form that they will be useful as reference material for geneticists and also as a source of information to nongeneticists." To judge by the material of this first volume, Dr. Demerec's series should be one of the most important genetic publications now available.

There are here ten essay reviews, each summarising a considerable body of research and explaining the progress of the last ten or fifteen years. It is naturally not possible to summarise in a brief review the actual advances in genetic knowledge incorporated in this material. Only a few contributions of exceptional merit can here be noted.

Two of the papers summarise recent advances in knowledge relevant to the origin of cultivated plants, namely maize, by P. C. Mangelsdorf, and the New World tetraploid cottons, dealt with by S. G. Stephens.

It may be recalled that the cultivated cottons of the New World are tetraploid, with twenty-six chromosome pairs, while those of the Old World have only thirteen. Tetraploid species have also been named in various localities in the Pacific, but apart from *G. tomentosum* of Hawaii these are now thought to have been derived secondarily from the New World tetraploids. There are also towards the Pacific coast of tropical America some wild diploid species. The conclusion towards which these cytological preliminaries, together with much experimentation by direct breeding, have been tending is that native diploid cottons were brought into cultivation in America, and that with these, by some means at present unknown, Asiatic diploid cottons of the cultivated species were intermixed, ultimately yielding the allotetraploids, which by their superior qualities rapidly replaced the primary diploids in American cotton production.

On the evidence presented, there seems little doubt that thirteen of the chromosome pairs of the New World cultivated cottons have been derived at no very distant date from the Old World cultivated species *herbaceum* or *arboresum*, and in Stephens' opinion the South American species *raimondii* is the most probable source of the remaining thirteen pairs.

The story developed by Mangelsdorf for maize is somewhat more complex, and indeed somewhat less securely established. His account of the relevant researches can only be described as masterly.

No notice of this very fine book would be complete without reference to T. M. Sonneborn's magnificent article on "Recent Advances in the Genetics of *Paramecium* and *Euplotes*."

R. A. FISHER.

Animal Nutrition. By LEONARD A. MAYNARD, A.B., Ph.D., Sc.D.
[Pp. xviii + 494, with 44 figures.] Second edition. (New York and London: McGraw-Hill Book Co., Inc., 1947. 25s. net.)

THE second edition of this well-known book includes references to the striking new advances made in this field during wartime years. The general outline of the book is as before, but extensive revision of the chapters on vitamins, protein and mineral nutrition, and the metabolism of carbohydrates and fats

has been made. Isotopes as "tracers" in metabolism have revolutionised many of our ideas regarding the metabolism of fats, proteins and minerals, and these are referred to in the appropriate chapters. Protein synthesis from urea in the rumen is discussed and references are given to recent work on this subject; while the protein reserve and the plasma proteins in relation to resistance to infectious diseases are mentioned. Trace elements such as cobalt, copper, manganese and magnesium are referred to, as also is the toxicity of fluorine. In these sections, as in many others, there is much which should be useful to the student of Veterinary Medicine as well as those engaged in Animal Husbandry. The account of the vitamins, and particularly those of the B complex such as thiamine, nicotinic acid and pyridoxine, is extensive, and the list of selected literature, which is given at the end of each chapter, is especially valuable to those who may wish to consult original research or specialised reviews of the subject. As in the first edition, there are also chapters dealing with the application of the chemistry and physiology of nutrition to the feeding of farm animals for special purposes such as growth, reproduction, lactation and work production.

J. H.

Foods, Their Values and Management. By HENRY C. SHERMAN.
[Pp. x + 221.] (New York: Columbia University Press, 1946;
London: Oxford University Press, 1947. 18s. net.)

PROF. SHERMAN'S name is known, far outside the academic circle, to all interested in food, biochemistry and nutrition; he writes with the authority of an original investigator, coupled with the skill of an exponent of science to ordinary non-scientific folk.

He has now produced a volume complementary to his well-known *The Science of Nutrition*, co-ordinating economic considerations with nutritional knowledge. Such a union must be fruitful and useful, specially in the circumstances prevailing over almost all the earth to-day. The term "management" is to embrace this type of knowledge or activity, which has as its object planning of better food and nutrition as an essential of the better life.

Foods are divided for convenience into ten groups. Their essential composition is indicated; stress is laid upon the actual constituent amino-acids of their proteins, so it is shown how one food can most usefully supplement another, and improve the dietetic value of both. Vitamins and provitamins are considered in the most up-to-date manner. We are glad to note that the term Vitamin A "value" is used in relation to plant foods such as the potato, rather than vitamin content. This provides for the effects of carotenes. The value of potatoes and other vegetables as sources of vitamin is apt to be neglected because their content is low, but the author points out how important their small content is in view of the large amounts consumed.

The chapter on food adjustment problems is interesting and stimulating. Milk is given pride of place, as it represents the most economical, as well as most efficient, transformation of animal feeding stuffs into human food; and it is the best source of specific nutrients. Perhaps we shall not all agree with Prof. Sherman as to the extent of prejudice in the popular evaluation of various grades of beef, but we do all agree with Disraeli that the first duty of any nation is to safeguard the health of its people by the supply of adequate and proper food. Any well-informed contribution to knowledge of how health

depends upon nutritive values of food is doubly welcome at the present time when supplies are so limited.

The book is well written, and of course it is accurate in its facts and up to date. It is inevitably American, and this shows itself not only in the American angle of view on diet and the consumption of certain foods, but also in the bibliography which, with a very competent index, occupies nearly a quarter of the volume. We miss a number of prominent British names and references to outstanding papers on this side of the Atlantic. Perhaps this is to some extent inevitable; we all read first our own journals, but a book to be offered to the whole English-speaking world should more evenly survey all the relevant scientific journals.

When one reads books on food and diet one is sometimes tempted to hope that we shall not be too much occupied with vitamins, amino-acids, calories or values, and that greater stress may come to be laid on palatability and preference. Food is more than the mere means of existence, and many must have a secret sympathy with the crowd in Hamburg who had banners "We want bread, not calories."

We are indebted to Prof. Sherman for his work.

H. E. Cox.

Chemistry and Methods of Enzymes. By JAMES B. SUMNER and G. FRED SOMERS. Second edition. [Pp. xii + 415, with 10 figures.] (New York: Academic Press, Inc., 1947. \$6.50.)

THE second edition of this useful book has involved much careful revision of the original edition and the authors have succeeded in producing a valuable general survey of this rapidly expanding field. The many controversial problems which face the enzymologist to-day make it very difficult for any author to write authoritatively on this subject except within the limits of his own specialised studies. Minor inaccuracies or omissions are therefore unavoidable.

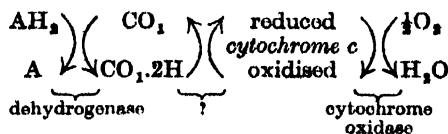
The introductory chapter on general properties of enzymes is clear and concise, but deals almost entirely with hydrolytic enzymes. Omission of a survey of oxidising enzymes in this introduction is unfortunate, especially as Chap. VIII, which introduces the oxidizing enzymes, is very brief. Some account of the controversy over the hydrogen activation theory of Wieland and the oxygen activating theory of Warburg, which was finally resolved by Keilin's discovery of cytochrome, could with advantage have been included, as could also the fundamental work on reversibility of dehydrogenases initiated by Quastel. The second part of the book dealing with hydrolytic enzymes is comprehensive and, although many catalysts in this class are of necessity treated very briefly, the phosphorylating and proteolytic enzymes are discussed authoritatively and in some detail. Part 3 covers the oxidising enzymes, which are classified according to the present-day convention into iron-enzymes, copper enzymes, dehydrogenases reacting with coenzymes and with cytochrome, and the flavin enzymes. Various decarboxylases and miscellaneous enzymes are also considered. The section on copper enzymes is open to criticism, partly on account of its extreme brevity and partly owing to the confusion over nomenclature (p. 239). Thus tyrosinase and catecholase are distinct enzymes, while the "laccase" of potatoes and many other plants is now known to be a catecholase. Part 4 of the book gives an account of various hydrolases and mutases and concludes with an able and

clear summary of the rôle of enzymes in intermediary metabolism. Comprehensive lists of references and careful indexing contribute greatly to the usefulness of this book.

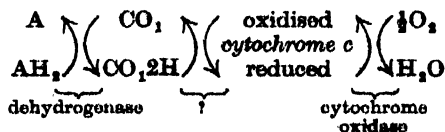
E. F. H.

Dynamic Aspects of Biochemistry. By ERNEST BALDWIN, B.A., Ph.D. [Pp. xviii + 457, with 34 figures.] (Cambridge: at the University Press, 1947. 21s. net.)

DR. BALDWIN'S book breaks with the usual method of treatment of the subject in being written with properties of enzyme systems as its main foundation and not, as is more usual, on a basis of the chemical properties of the constituents of living matter. In the author's own words "the subject-matter of biological chemistry, like that of biology itself, can be roughly divided into two parts; the static or morphological, and the dynamic or physiological." Since the present book, as its title indicates, concentrates on the dynamic aspects of the subject, it presupposes a reasonable knowledge of the static aspects of biochemistry, including those parts of organic chemistry immediately relevant to biological systems. For the suitably equipped reader the book is an inspired survey of the very large mass of information relevant to the biochemistry of function which has accumulated within recent years. The author writes in an extremely readable style and the text is well illustrated by ingenious diagrams of cyclical processes. For setting out in a pictorial form chains of successive chemical events the author makes extensive use of what he describes as "whirligigs"; in this connection a word of criticism may be voiced. Where Dr. Baldwin writes (p. 140):



the reviewer, in his teaching, has always employed the form:



The latter system has the great advantage that the oxidised and reduced forms of the reactants are segregated on the top and bottom lines respectively. There are a few errors in formulae in the book and the author is not justified in stating (p. 352) that the pituitary "diabetogenic hormone [sic] has a powerful inhibitory action upon hexokinase." Whether or not the pituitary hexokinase-inhibitor is identical with the diabetogenic factor still remains to be determined. The author has sometimes given admittedly provisional explanations which would not be accepted by all competent authorities; but he is to be congratulated on his courage in plainly stating in such instances that our present knowledge is insufficient for a final judgment to be affected.

This book may be said to anticipate to some extent the formulation of the newer aspects of biochemistry which are at present in a relatively undeveloped

state. For the important assistance he has given to that formulation the author is to be both thanked and congratulated.

F. G. Y.

Vitamins and Hormones. Advances in Research and Applications.

Vol. IV. Edited by R. S. HARRIS and K. V. THIMANN. [Pp. xvii + 406, with 44 figures.] (New York: Academic Press, Inc., 1946. \$6.80.)

As we pass, almost imperceptibly, from war-time to peace-time conditions, so also does the speed of publication slowly increase—at any rate in the United States. There is by now even a discernible tendency for annual publications to appear annually, and it is just a year since there appeared in SCIENCE PROGRESS a notice of *Vitamins and Hormones*, Vol. III (*loc. cit.*, 1947, 35, 175).

Comparisons of a purely quantitative kind show a small fall in the total number of pages (from 420 to 406), possibly itself an expression of the reduced time between issues, though the number of separate articles remains nine, of which seven are of United States origin, one from Australia and one from Buenos Aires. As heretofore, the general aim of the articles appears to be of the "Recent Advances" type, rather than the "Annual Reviews"; thus in Prof. Houssay's 16-page survey on "The Thyroid and Diabetes" references are made to work as far back as 1917—though naturally this critical and authoritative contribution is concerned mainly with the work of the last decade—with some 90 references, including 7 to his own work. Closely connected with this article is the following one on "Thyroactive Iodinated Proteins" (43 pp.) by E. P. Reineke of Michigan, covering 42 pages with 173 references. Owing to the recent development of this work, from the researches described by Mutzenbecher in Hoppe-Seyler just before the war, the survey is comprehensive. If it seems to suggest that prognosis for therapeutic use of "iodo-protein" is rather more favourable, particularly in the veterinary field, than it has so far generally been judged in this country, this must be credited at least in part to the enthusiasm of the expert, for Reineke has collaborated with C. W. Turner in making many important advances in this field. It may, however, be noted that the *Xenopus* test for thyroid activity elaborated by Parkes and Bruce is mentioned neither here nor in the article by S. A. Thayer in "Bioassay of Animal Hormones" (47 pp.): the former includes a fairly detailed account, with photographs, of the test with *Rana pipiens*, and the latter a cursory reference to "inducement of precocious metamorphosis in the larvæ of amphibia" and both bibliographies contain some 1946 references. Parkes and Bruce published their findings in 1945.

Dr. Thayer's article, in general, though comprehensive in scope, seems to fall between two stools: the elementary exposition of methods for calculating such simple statistics as standard deviation and significant difference is not likely to be read by the tyro and can hardly help the experienced bio-assayist, and there are evident some inaccuracies in the text. Amongst these are the use of the symbol y^- and x^- , where $\bar{y}$ and $\bar{x}$ are meant, and the failure to note in the text that the crop-gland weights finally used by Riddle and Bates in prolactin assay were in fact the weights per 300 gram pigeon. It is surely misleading to say that the "response is directly proportional to body-weight";

the response, according to the original authors, when corrected for body-weight, bears a linear relation to the logarithm of the dose of prolactin.

There is no space to do more than mention in passing the exhaustive survey by F. W. Clements of "Manifestations of Nutritional Deficiency in Infants" (50 pp.), the necessarily tentative review by J. J. Piffner and A. J. Hogan of "The Newer Hematopoietic Factors of the Vitamin B-Complex" (32 pp.)—surely a rather curious title, for it is our knowledge of the factors that is new (and incomplete), not the factors themselves!—and two other interesting articles, Roy Hertz's "Effect of B Vitamins on the Endocrinological Aspects of Reproduction" (11 pp.) and Morton S. Biskind's "Nutritional Therapy of Endocrine Disturbances" (34 pp.).

Two sections of this volume, however, stand out notably. The first is Charles D. Kochakian's account of the work, much of it his own, on "The Protein Anabolic Effects of Steroid Hormones" (55 pp.), which may make many aware for the first time of the complex but fascinating relationship between gonadic endocrine activity and the intermediary metabolism of protein and the probability, to put it no higher, that the effects are distinct from those produced by the adrenal cortical steroids. That these are not under discussion in this important review might, perhaps, with advantage have been indicated in its title.

Finally, it is difficult to praise too highly Howard A. Schneider's magnificent essay on "Nutrition and Resistance to Infection: The Strategic Situation" (32 pp.). Written with masterly clarity, it clears away one misconception after another and makes a profound and searching analysis of a problem of fundamental biological, medical and sociological importance, and one that has been scratched all too superficially by innumerable writers. It repays reading again and again, clear though it is at the first impression, and can possibly be given its due meed of praise by the statement that it is a feather in the cap even of the New York Rockefeller Institute, where its author works.

If a concluding word of constructive criticism may be offered to the editors of this excellent publication, it is that they standardise the methods of giving the bibliographical references at the end of each article: at present there are at least three different ones in use.

A. L. BACHARACH.

Recent Progress in Hormone Research. Vol. I. Edited by GREGORY PINCUS. [Pp. vi + 399, with 162 figures.] (New York: Academic Press, Inc., 1947. \$7.50.)

DURING the past few years there has been a surprising increase in the number of periodical publications devoted to reviews within the biological field. To the specialised research worker many of these publications are of importance, not only because they provide review articles by other workers, but also because the investigator himself, called upon to write such a review, finds salutary discipline in surveying the relationship of his own specialised corner of the field to wider topics. The genesis of the volume under review is of some relevant interest. In 1944 the Hormone Conference of the American Association for the Advancement of Science met at Mont Tremblant in the Laurentian Mountains, near Montreal, Canada. This series of sessions was so successful that it was decided to make this "Laurentian Conference" an

annual affair and to publish each year an account of the papers and of the discussion.

The first volume, dated 1946, represents the Proceedings of the 1945 meeting, and is divided into sections, of which the following are the subjects and authors: *Section I. Neurohumoral Relationships.*—On the Rôle of Acetylcholine in the Mechanism of Nerve Activity. By David Nachmansohn. *Hormones and Mating Behaviour in Vertebrates.* By Frank A. Beach; *Section II. Chemistry and Physiology of Adrenal Hormones.*—Steroids Derived from the Bile Acids: 3, 9-Epoxy-11-Cholenic Acid, an Intermediate in the Partial Synthesis of Dehydrocorticosterone. By Edward C. Kendall. Some Advances in the Partial Synthesis of Adrenal Cortical Steroids. By T. F. Gallagher. The Relation of Cholesterol and Ascorbic Acid to the Secretion of the Adrenal Cortex. By C. N. H. Long. Studies of the Rôle of the Adrenal Cortex in the Stress of Human Subjects. By Gregory Pincus; *Section III. The Rôle of Hormones in Metabolic Processes.*—The Relation of the Anterior Pituitary Hormones to Nutrition. By Leo T. Samuels. The Rôle of Hydrolytic Enzymes in Some of the Metabolic Activities of Steroid Hormones. By Charles D. Kochakian. Studies on Steroid Hormones in Experimental Carcinogenesis. By W. U. Gardner; *Section IV. Aspects of Clinical Endocrinology.*—Endocrine Aspects of Human Cancer. By Ira T. Nathanson. The Effect of Hormones on Osteogenesis in Man. By Fuller Albright. Certain Factors which Influence the Rate of Growth and the Duration of Growth of Children. By Nathan B. Talbot and Edna H. Sobel. Experimental Renal Hypertension with Special Reference to Its Endocrine Aspects. By Arthur Grollman.

The treatment is naturally somewhat uneven from paper to paper and in particular the contributions in Section III hardly justify the general title of this section. One point of outstanding value, however, is the inclusion of an account of the extempore discussion which followed each contribution. This alone makes the volume of great interest and importance to the research worker in endocrinological topics. It is doubtful, however, if the general reader could gain a balanced viewpoint on many of the specialised topics under discussion by the specialised expert investigators.

The bringing together in one volume of Clinical Aspects of the subject (Section IV), with the results of purely experimental work, is particularly interesting. Nevertheless, one may begin to wonder whether we are not fast approaching the saturation point for review articles directed to the active investigator. For the advanced student who is not engaged in research specialised reviews can sometimes be confusing and it is to be hoped that this type of reader will not always be neglected in comparison with the more advanced worker.

F. G. Y.

Annual Review of Physiology. Vol. IX. VICTOR E. HALL, Editor, JEFFERSON M. CRIMSON and ARTHUR C. GIESE, Associate Editors. [Pp. viii + 736, with 1 plate.] (Stanford University P.O., California: Annual Reviews Inc. and American Physiological Society, 1947. \$6.00.)

For the first time for many years the *Annual Review* contains as many as five contributions from Europe, a valuable safeguard against the neglect of certain points of view on given topics throughout the nine volumes. Unfor-

unfortunately a few of these contributors have themselves had difficulty in obtaining access to all the literature, and the lack of exchange of publications during the war will yet take some time to overcome. On the other hand, much research done during the war has been only recently published and some reviewers have been so embarrassed by too much material that the editors have had to do a certain amount of cutting. This may account for the lameness of some of the articles in places, but the general level of the contributions is well up to previous standards. The *Review* continues to serve as an excellent guide to the literature, though authors are increasingly referring the reader to other reviews for the treatment of parts of their subject. Most of the articles are straightforward accounts of the latest work, bringing previous articles by other authors up to date, and perhaps filling in a few gaps. It is good, however, to read the views of Buchthal (Copenhagen) on muscle, and, as always, Adolph is stimulating on water metabolism, in which he deals with problems of desert life and shipwreck. The impetus lent by the war to some lines of research is manifest also in the article by Dill and Forbes on blood gas transport, a subject not previously reviewed for some years; references are given to micro-analytical methods of surprising accuracy. One of the most interesting topics is the fractionation of the plasma proteins which was developed by Cohn and others during the war, using ethanol at low temperatures in addition to other devices. Osmotically more than four times as concentrated as plasma, a 25 per cent. solution of serum albumin is no more viscous than whole blood and one gramme will hold about 20 c.c. of water in the circulation. Fibrin can be converted into many forms, and may be used surgically as a plastic in films, etc., or as a spongy material saturated with thrombin. The iso-agglutinins determining the blood groups have also been isolated and various anti-bodies are under investigation. One of the best-written reviews is that of Grundfest on bioelectric potentials, in which he deals with the nature of the potential which exists between the inside and outside of nerve, and with the initiation, propagation and transmission of excitation. Perhaps the greatest value of each volume lies in its contribution to the accumulated information standing to the credit of its predecessors.

W. H. N.

MISCELLANEOUS

The Design of Experiments. By RONALD A. FISHER, Sc.D., F.R.S.
Fourth edition. [Pp. xii + 240, with 5 figures.] (Edinburgh:
Oliver & Boyd, Ltd., 1947. 12s. 6d. net.)

THIS interesting book was first published in 1935, after ten years in which the author's treatment of statistical methods, and in particular his application of those methods to biological research, had become common knowledge through the medium of his other work, *Statistical Methods for Research Workers*. The book under review is self-contained so far as its subject matter is concerned, although, since quite a part of the earlier work also dealt with experimental design in elaborating the appropriate statistical methods, it naturally follows that full details of the elementary analyses are not always given, and also that much of the space is given up to the more complex designs, and includes analyses which have developed since the publication of the earlier work. Since, also, the book does not claim to expound the statistical method

from the beginning, it is clear that the two works are complementary, and the greatest benefit is to be had from studying the book with *Statistical Methods* ready to hand as a work for constant reference, and as a source-book for the necessary tables.

The call for a fourth edition is evidence that the book continues to have widespread attention. The changes that have been made are not considerable, consisting chiefly of certain additions. Thus two sections were added in the third edition which make very interesting reading; the first pretty well exhausts the possibilities of general confounding where any number of factors are introduced into an experiment at two levels each, while the second deals briefly with double confounding, illustrating from a possible experiment which has important practical applications, in which multiple observations may be required on each of a number of experimental animals.

In the fourth edition a section is added on the Fiducial Limits of a Ratio, which gives a new and simple statistical technique, involving the solution of a quadratic equation, to assist in the interpretation of a ratio calculated from two normally distributed estimates. It would have helped here had the author advanced his first introduction of the term "fiducial probability" from p. 192 to p. 188. It may be considered that 5 decimals is one too many for the solutions of α on p. 190, especially as the solution, although correct, does not follow exactly from the equation as given, with coefficients cut down to whole numbers. Slips have been noted on p. 198 (2.086) and on p. 201 (last term of formula). The book has become no easier to understand, but will well repay the concentrated attention necessary.

J. WISHART.

Statistical Methods in Research and Production: With Special Reference to the Chemical Industry. Edited by OWEN L. DAVIES, M.Sc., Ph.D. [Pp. xii + 292, with 21 figures.] (Edinburgh and London; Oliver & Boyd, for Imperial Chemical Industries, Ltd., 1947. 28s. net.)

THIS book epitomises the wide experience of a group of workers in the Imperial Chemical Industries of the application of statistical methods to chemical, physical, engineering and biological data arising in research and works process control in the chemical industry. This is the first book to cover this field of application; it will be of direct value to scientific workers in a wide range of industries, it will further the cause of statistical education, and it will make an indirect contribution to industrial efficiency.

Broadly, the method of exposition is to present some practical data, discuss briefly their technical implications, and then introduce and explain the appropriate statistical method. The most important mathematical assumptions are clearly stated, some of the formulæ are proved, and methods of computation are given in considerable detail and copiously illustrated by examples.

Frequency distributions, tests of significance, the analysis of variance, regression and correlation, contingency tables, sampling and control charts are dealt with. The treatment is thorough, and the field covered within each chapter is not large.

Anyone who reads through the book will attain a good appreciation of what the statistical methods dealt with can do in the fields covered by the examples, especially as points of practical interpretation are well discussed;

and if he works through the examples he will be well drilled in the technique of application. The logical basis of the methods is not probed very deeply, but the age of scepticism has passed; the application of statistical methodology to experimental investigation is justified by its works, and most experimenters who find by experience that it helps to solve otherwise intractable problems will be content.

Good points of detail are the quality of paper and printing, the systematisation of the symbols, the lists of signs and symbols, the full tables of statistical functions, and the glossary of statistical terms. Not so good is the effect of "scrappiness" given by the many short sections and appendices, and by the fairly frequent references forward, resulting from the method of presentation. These last comments are offered in full realisation of the fact that it is easier to criticise than to improve.

L. H. C. T.

Van Nostrand's Scientific Encyclopedia. Second edition. [Pp. vi + 1600, with many illustrations.] (New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1947. 65s. net.)

THIS is a valuable book, produced with great care and excellent judgment.

It is a sign of the times that, since the first edition in 1939, the number of pages has risen from 1234 to 1600, and the entries by 1000; and the price from 50s. to 65s. Greatly increased, too, is the space given to Aeronautics and Electronics; and the original 10 Contributing Editors and 11 Consulting Editors have been raised to 20 and 16 respectively, most of the earlier workers being retained.

The book is addressed "to the enquiring layman and to the trained technician"; and in the main this is justified, though the understanding of most of the mathematical entries by a layman, however "enquiring," would excite the envy of most technicians. The short biographies of the first edition have been dropped; but the summary histories of Chemistry, Geology, and Flight give hope that other sciences may be included in a subsequent edition.

The very simple method of indicating cross-references could well be widely copied.

"The authors and the publishers will appreciate the indulgence of the reader for omissions. The exercise of judgment in selection of material is unavoidable . . .": not merely "unavoidable" but fundamentally necessary. But will we forgive the absence of *Triangular Coordinates*, although they are used in "Colour Triangle"? Or of *Eruption* as referring to volcanoes: of *Crookes Glass*, of *Parachor*, of *Bromine* from the Dead Sea, of *Buffer Solutions*, of an explanation of *Isobares* (in "Chemical Constitution"), of *Meniscus* in connection with Surface Tension? Or the surprising inadequacy of the entry on "Inks"?

This "encyclopedia" is an American production, so that the British reader must be resigned to "fiber," "sulfur," "hematite," "callus" and the like; and he must accept, as natural, references to American Acts and Regulations, and to American examples of geological periods and eras. But he may be puzzled by "tubes" for wireless valves, and still more by "muffler" for a motor-car silencer. And he will certainly feel outraged by the slovenly American "Aluminum" for the customary "Aluminium."

It is curious to find that "Virus" deals with animal diseases and "Virus Diseases" with those of the vegetable kingdom.

So far as the reviewer has seen, there is only one misprint, and this, as so often occurs, is an obvious one—"Cobera," under "Hamadryad." And only one error has been discovered: under "Platinotype Process" it is stated that "under exposure to light the ferrous salt is *reduced* to ferric."

Further criticism dealing with minor points would lead to a charge of perversity cantankerousness on the part of the reviewer; so he will take leave of the subject with congratulations to all concerned with its production—and its purchaser.

PERCY E. SPIELMANN.

X-Ray the City! The Density Diagram: Basis for Urban Planning.

By ERNEST FOOKS, D.Eng., A.R.A.I.A. [Pp. 107, with 30 figures.]
(Melbourne: Ruskin Press, 1947. 12s., Australian currency.)

THIS book includes a study of the various methods of measuring population densities in urban areas and a new method is suggested, for international adoption, which would enable a comparable analysis of different cities to be made, free from the confusion of arbitrary administrative boundaries. This method involves the use of a circular distance grid, which is superimposed on a city plan, the densities being calculated for individual segments of each concentric ring. The various functions and facilities of a town can also be analysed on this grid basis.

With the growing exchange of town planning information between countries, this more rational approach is timely, but it is doubtful whether the simple grid formula can be applied universally; for instance, towns with a non-circular formation cannot be analysed in this way. However, the objective of an international language for town planners is worth pursuing and, with some refinement, the idea might well be taken up by an international organisation such as the International Federation of Housing and Town Planning.

The book also discusses general town-planning principles and includes a useful bibliography.

A. L.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

- Advanced Calculus.** By David V. Widder, Professor of Mathematics, Harvard University. New York: Prentice-Hall, Inc.; London: Constable & Co., Ltd., 1947. (Pp. xvi + 432, with 39 figures.) 36s. net.
- Mechanics.** By John C. Slater, Ph.D., and Nathaniel H. Frank, Sc.D., Professors of Physics, Massachusetts Institute of Technology. International Series in Pure and Applied Physics. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. xiv + 297, with 44 figures.) 20s. net.
- Manual for Process Engineering Calculations.** By Loyal Clarke, Chemical Engineer, Celanese Corporation of America. Chemical Engineering Series. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. x + 438, with 145 figures and 159 tables.) 30s. net.
- The Growth of Physical Science.** By Sir James Jeans, O.M. Cambridge: at the University Press, 1947. (Pp. x + 364, with 14 plates and 39 figures.) 12s. 6d. net.
- Intermediate Physics.** By C. J. Smith, Ph.D., M.Sc., A.R.C.S., Lecturer in Physics, Royal Holloway College, University of London. Third edition. London: Edward Arnold & Co., 1947. (Pp. xii + 1033, with 702 figures.) 18s. net.
- Methods of Measuring Temperature.** By Ezer Griffiths, D.Sc., F.R.S., Senior Principal Scientific Officer in the Physics Division, The National Physical Laboratory. Third edition. London: Charles Griffin & Co., Ltd., 1947. (Pp. x + 223, with 91 figures and 56 tables.) 20s. net.
- Applied Thermodynamics.** By A. C. Walahaw, Ph.D., M.Sc., D.I.C., A.C.G.I., A.M.I.Mech.E. London and Glasgow: Blackie & Son, Ltd., 1947. (Pp. xi + 401, with 180 figures and 1 folding plate.) 30s. net.
- Light, Vision and Seeing.** By Matthew Luckiesh, D.Sc., D.E., Director, Lighting Research Laboratory, General Electric Company, Nela Park, Cleveland. New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1944 (fourth printing, 1946). (Pp. xvi + 323, with 16 plates and 83 figures.) 25s. net.
- Electricity.** By M. M. Das, B.Sc., A.Inst.P. London: Thomas Murby & Co., Ltd., 1947. (Pp. xii + 483, with 303 figures.) 25s. net.
- Theory and Applications of Electricity and Magnetism.** By Charles A. Culver, formerly Head of the Department of Physics, Carleton College. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. viii + 594, with 407 figures.) 25s. net.
- Electromagnetism.** By John C. Slater, Ph.D., and Nathaniel H. Frank, Sc.D., Professors of Physics, Massachusetts Institute of Technology. International Series in Pure and Applied Physics. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. xiv + 240, with 40 figures.) 17s. 6d. net.
- The Atom and its Energy.** By E. N. da C. Andrade, D.Sc., Ph.D., F.R.S., Quain Professor of Physics in the University of London. London: G. Bell & Sons, Ltd., 1947. (Pp. xii + 196, with 12 plates and 17 figures.) 10s. net.

- Atomic Challenge. A Symposium.** By Professor J. D. Cockcroft, C.B.E., F.R.S., and others. London: Winchester Publications, Ltd., 1947. (Pp. x + 180, with 40 plates.) 8s. 6d. net.
- Meet the Atoms. A Popular Guide to Modern Physics.** By O. R. Frisch. London: Sigma Books, Ltd., 1947. (Pp. xiv + 226, with 7 plates and 26 figures.) 12s. 6d. net.
- Electrons (+ and -), Protons, Photons, Neutrons, Mesotrons, and Cosmic Rays.** By Robert Andrews Millikan, Director, Norman Bridge Laboratory of Physics, California Institute of Technology. Second edition. Chicago: The University of Chicago Press; London: Cambridge University Press, 1947. (Pp. x + 642, with 124 figures and 23 tables.) 30s. net.
- Electronics and their Application in Industry and Research.** Edited by Bernard Lovell, O.B.E., B.Sc., Ph.D., F.Inst.P., Physical Laboratories, University of Manchester. London: The Pilot Press, Ltd., 1947. (Pp. xvi + 660, with 404 figures.) 42s. net.
- Nuclear Physics in Photographs. Tracks of Charged Particles in Photographic Emulsions.** By C. F. Powell and G. P. S. Occhialini, the H. H. Wills Physical Laboratory, University of Bristol. Oxford: at the Clarendon Press, 1947. (Pp. xii + 124, with 51 plates and 16 figures.) 18s. net.
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- Radio Aids to Navigation.** By R. A. Smith, M.A., Ph.D., A.M.I.E.E. Modern Radio Technique Series. Cambridge: at the University Press, 1947. (Pp. xii + 114, with 37 figures, including 7 plates.) 9s. net.
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- Applied Architectural Acoustics.** By Michael Rettinger, B.A., M.A., Engineering Department RCA Victor Division, Radio Corporation of America, Hollywood, California. Brooklyn, N.Y.: Chemical Publishing Co., Inc., 1947. (Pp. xii + 189, with 69 figures and 4 tables.) \$5.50.
- Micro-Waves and Wave Guides.** By H. M. Barlow, B.Sc.(Eng.), Ph.D., Professor of Electrical Engineering, University College, London. London: Constable & Co., Ltd., 1947. (Pp. x + 122, with 70 figures.) 15s. net.
- Modern Gas Turbines.** By Arthur W. Judge, A.R.C.S., D.I.C., A.M.Inst.Aut.E., A.F.R.Ae.S., M.S.A.E. London: Chapman & Hall, Ltd., 1947. (Pp. xii + 311, with 214 figures, including 19 plates, and 31 tables.) 28s. net.
- Rational Approach to Chemical Principles.** By John A. Cranston, D.Sc., F.R.I.C., Lecturer on Physical Chemistry, Royal Technical College, Glasgow. London and Glasgow: Blackie & Son, Ltd., 1947. (Pp. xii + 211, with 37 figures and 33 tables.) 8s. 6d. net.
- The Rare-Earth Elements and their Compounds.** By Don M. Yost, Professor of Inorganic Chemistry, Horace Russell, Jr., former Instructor in Chemistry, California Institute of Technology, and Clifford S. Garner,

- Assistant Professor of Chemistry, University of California at Los Angeles.**
New York: John Wiley & Sons, Inc.; **London:** Chapman & Hall, Ltd., 1947. (Pp. x + 92, with 9 figures and 30 tables.) 15s. net.
- Volumetric Analysis. Vol. II: Titration Methods: Acid-Base, Precipitation, and Complex-Formation Reactions.** Second edition. **New York and London:** Interscience Publishers, Inc., 1947. (Pp. xiv + 374, with 14 figures.) 36s. net.
- Practical Emulsions.** By H. Bennett, Technical Director, Glyco Products Co., Inc. Second edition. **Brooklyn, N.Y.:** Chemical Publishing Co., Inc.; **London:** Chapman & Hall, Ltd., 1947. (Pp. xvi + 568.) 48s. net.
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- Matthews' Textile Fibers.** Fifth edition. Prepared by a Staff of Specialists under the Editorship of Herbert R. Mauersberger, Technical Editor, Rayon Textile Monthly. **New York:** John Wiley & Sons, Inc.; **London:** Chapman & Hall, Ltd., 1947. (Pp. xvi + 1133, with 311 figures and 266 tables.) 76s. net.
- The Technology of Adhesives.** By John Delmonte, Technical Director, Plastics Industries Technical Institute, Los Angeles, California. **New York:** Reinhold Publishing Corporation; **London:** Chapman & Hall, Ltd., 1947. (Pp. viii + 516, with 79 figures.) 48s. net.
- The Chemistry and Technology of Plastics.** By Raymond Nauth, Consultant in Plastics. **New York:** Reinhold Publishing Corporation; **London:** Chapman & Hall, Ltd., 1947. (Pp. viii + 522, with frontispiece, 278 figures and 72 tables.) 57s. net.
- The Chemistry of Commercial Plastics.** By Reginald L. Wakeman, Director of Research, Onyx Oil & Chemical Co., Jersey City, N.J. **New York:** Reinhold Publishing Corporation; **London:** Chapman & Hall, Ltd., 1947. (Pp. xii + 836, with 324 figures and 189 tables.) 60s. net.
- Adventures in Man's First Plastic. The Romance of Natural Waxes.** By Nelson S. Knaggs, Hilton-Davis Chemical Co. Division, Sterling Drug, Inc. **New York:** Reinhold Publishing Corporation; **London:** Chapman & Hall, Ltd., 1947. (Pp. xiv + 329, with 92 plates.) 40s. 6d. net.
- Petroleum Production. Vol. III: Oil Production by Water.** By Park J. Jones, Consultant, Houston, Texas. **New York:** Reinhold Publishing Corporation; **London:** Chapman & Hall, Ltd., 1947. (Pp. x + 271, with 175 figures and 40 tables.) 30s. net.
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- Petrographic Micro-Technique.** By A. V. Weatherhead, F.R.M.S., A.R.P.S. **London:** Arthur Barron, Ltd., 1947. (Pp. x + 102, with 94 figures.) 12s. 6d. net.
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- The Canadian Mineral Industry in 1945.** Reviews by the Staff of the Bureau of Mines. Canada : Department of Mines and Resources, Mines and Geology Branch, Bureau of Mines, 1947. (Pp. vi + 103.) 25 cents.
- Mother Earth.** By T. A. Ryder, B.Sc., Ph.D., F.G.S. London : Hutchinson's Scientific and Technical Publications, 1947. (Pp. 184, with 25 figures.) 15s. net.
- An Introduction to Palæontology.** By A. Morley Davies, D.Sc., A.R.C.S., F.G.S., sometime Reader in Palæontology, University of London. Second edition. London : Thomas Murby & Co., 1947. (Pp. xii + 372, with frontispiece and 118 figures.) 18s. net.
- Glacial Geology and the Pleistocene Epoch.** By Richard Foster Flint, Professor of Geology in Yale University. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1947. (Pp. xviii + 589, with 88 figures and 30 tables.) 36s. net.
- An Introduction to Paleobotany.** By Chester A. Arnold, Professor of Botany and Curator of Fossil Plants, University of Michigan. McGraw-Hill Publications in the Botanical Sciences. New York and London : McGraw-Hill Book Co., Inc., 1947. (Pp. xii + 433, with frontispiece and 187 figures.) 27s. 6d. net.
- Ancient Plants and the World they Lived in.** By Henry N. Andrews, Jr., Paleobotanist to the Missouri Botanical Garden, Associate Professor, Washington University. New York : Comstock Publishing Co., Inc. ; London : Constable & Co., Ltd., 1947. (Pp. xii + 279, with 166 figures.) 25s. net.
- The Evolution of Gossypium and the Differentiation of the Cultivated Cottons.** By J. B. Hutchinson, R. A. Silow and S. G. Stephens. Empire Cotton Growing Association. Being the Final Report of the Genetics Department, Cotton Research Station, Trinidad, B.W.I. London : Oxford University Press, 1947. (Pp. xii + 160, with 10 plates and 10 figures.) 15s. net.
- La Vie des Requins.** Par P. Budker. Collection "Histoires Naturelles"—7. Paris : Librairie Gallimard, 1947. (Pp. 277, with 22 plates and 40 figures.) Frs. 325.-.
- The Artificial Insemination of Farm Animals.** Enos J. Perry, Editor. London : Chapman & Hall, Ltd., 1947. (Pp. vi + 265, with 47 figures and 23 tables.) 15s. net.
- Man and Animals. What They Eat and Why.** By Ben Dawes, D.Sc., A.R.C.Sc., D.I.C., F.L.S., Reader in Zoology at King's College, University of London. London, New York, Toronto : Longmans, Green & Co., 1947. (Pp. viii + 100.) 7s. 6d. net.
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- Curare. Its History, Nature and Clinical Use.** By A. R. McIntyre, Ph.D., M.D., Professor of Physiology and Pharmacology, College of Medicine, the University of Nebraska. U.S.A. : University of Chicago Press ; Great Britain and Ireland : Cambridge University Press, 1947. (Pp. viii + 240, with frontispiece, 24 figures and 11 tables.) 27s. 6d. net.
- Malaria. With Special Reference to the African Forms.** By W. K. Blackie, M.D., Ph.D., F.R.C.P.(Ed.), D.T.M. & H., Honorary Consulting Physician, Salisbury Hospital, Southern Rhodesia. Cape Town : The African Bookman, for the Post-Graduate Press, 1947. (Pp. viii + 104, with 1 coloured plate.) 10s. 6d. net.

- Drugs from Plants.** By Trevor Iltyd Williams, B.A., B.Sc., D.Phil., Deputy Editor of *Endeavour*. London: Sigma Books, Ltd., 1947. (Pp. 119, with 12 plates.) 6s. net.
- Physiology of Man in the Desert.** By E. F. Adolph and Associates, Department of Physiology, University of Rochester. Monographs in the Physiological Sciences. New York and London: Interscience Publishers, Inc., 1947. (Pp. xiv + 357, with 136 figures and 65 tables.) 39s. net.
- Fundamentals of Immunology.** By William C. Boyd, Ph.D., Associate Professor of Biochemistry, Boston University, School of Medicine. Second edition. New York and London: Interscience Publishers, Inc., 1947. (Pp. xviii + 503, with 50 figures and 66 tables.) 36s. net.
- Advances in Enzymology and Related Subjects of Biochemistry.** Vol. VII. Edited by F. F. Nord, Fordham University, New York. New York and London: Interscience Publishers, Inc., 1947. (Pp. xii + 665, with 81 figures and 83 tables.) 52s. 6d. net.
- Le Rameau vivant du Monde. Le Déchiffrement des Faits.** Par Georges Matisse. Bibliothèque de Philosophie contemporaine—Logique et Philosophie des Sciences. Paris: Presses Universitaires de France, 1947. (Pp. xii + 298, with 34 figures.) Frs. 320.-.
- The Missing Factor in Science.** Inaugural Lecture by Herbert Dingle, Professor of History and Philosophy of Science, University College, London. London: H. K. Lewis & Co., Ltd., 1947. (Pp. ii + 18.) 2s. net.
- Science in Transition.** By A. W. Haslett, sometime Foundation Scholar of King's College, Cambridge; Editor of *Science To-day*. London: Christopher Johnson, Publishers, Ltd., 1947. (Pp. iv + 244, with 2 plates and 5 figures.) 10s. 6d. net.
- The Royal Society Newton Tercentenary Celebrations, 15-19 July 1946.** Cambridge: at the University Press, 1947. (Pp. xvi + 92, with 6 plates.) 10s. 6d. net.
- German Research in World War II.** By Leslie E. Simon, Colonel, Ordnance Department, U.S. Army; Director, the Ballistic Research Laboratories. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. (Pp. xii + 218, with 78 figures.) 24s. net.

SCIENCE PROGRESS

THE RELATION OF RHYTHMIC SEDIMENTATION TO CRUSTAL MOVEMENTS

By A. E. TRUEMAN, D.Sc., F.R.S.

COAL-BEARING sediments appear to provide some of the most detailed records of oscillations of the relative levels of sea and land. These oscillations may be, in part at least, the result of rhythmic changes in the earth's crust and their further study may be expected to throw light on some problems of orogenesis.

The importance of coal-bearing strata in this connection depends on (1) the fact that coal seams generally represent deposits formed at or near sea level, over an area of unusually stable character, and (2) the occurrence of coal seams as an item in a rhythmic or cyclic development of other sediments. It will be profitable to elaborate these two points before proceeding further.

THE FORMATION OF COAL

For many years two rival theories concerning the origin of coal were widely discussed. These were known as the Drift Theory and the Growth-in-Situ Theory. Briefly, the former was based on the view that vegetation which gave rise to coal had been swept by currents from neighbouring land into a lake or other shallow waters, there to be deposited between other sediments (mud and sand) which had been similarly transported. The In-Situ Theory, on the other hand, was based on the view that the vegetation which formed the coal grew in the area where it is now found, the area of sedimentation having been temporarily converted into a low-lying swamp; in this, peat-like material accumulated until the area became again submerged and mud and sand were once more carried in to bury the peaty layer.

Until twenty or thirty years ago many authorities were prepared to compromise and to adopt the view that both these methods of

formation had operated. In more recent years, however, the great majority of those concerned with coal-bearing rocks have reached the view that, apart from certain coals of unusual character (sapropelic coals) such as the cannels, all other coals (humic coals) have been formed from plant material which grew in or near the place where it is found. This does not preclude a certain limited "drift" of vegetable material within the area of deposition, but it lays emphasis on the view that at the time the coal was being deposited most of the vegetation grew in the area of deposition and not on a land area lying outside it. Various lines of evidence are relied on to support this view, and most of them were pointed out many years ago. But much recent work has reinforced their significance. It has been amply demonstrated, for example, that seat-earths or rootlet beds (sometimes in the form of fire-clays) occur in almost every case immediately beneath a coal seam. Moreover, the extent of individual seams is now known to have been much greater than was formerly accepted, and the relative absence of inorganic matter in seams of coal which extended over areas of several thousand square miles precludes the explanation that the plant debris was transported without contamination from adjoining areas.

In north-west Europe the area of coal deposition in Upper Carboniferous (Pennsylvanian) times extended from Ireland to Westphalia (probably to Silesia and possibly to the Donetz), and from South Wales at least as far as the Highland Border. In the United States areas of comparable size were simultaneously involved. Perhaps the most striking aspect of the deposition of the Coal Measures was the repeated conversion of these great expanses of comparatively shallow water into swamps, followed by their re-submergence.

THE RHYTHMIC OR CYCLICAL NATURE OF THE DEPOSITS

It has long been apparent that coal seams form one element in a rhythmic or cyclical sequence which comprises sandstones (and sometimes conglomerates), mudstones, shales, fireclays and (occasionally in north-west Europe, more frequently in some other areas) limestones. The cycle * has been expressed in various ways, but

* The terms rhythm and cycle are here used without distinction. It has lately been suggested that the sequence in the Coal Measures ought not to be called cyclic. The term cycle is being widely used in this sense in America, however, while in this country the familiar "Cycle of Erosion" relates to a series of changes not very different in essential character from "cycles" described in the Coal Measures.

in Britain the essential succession in each cyclical unit is as follows :

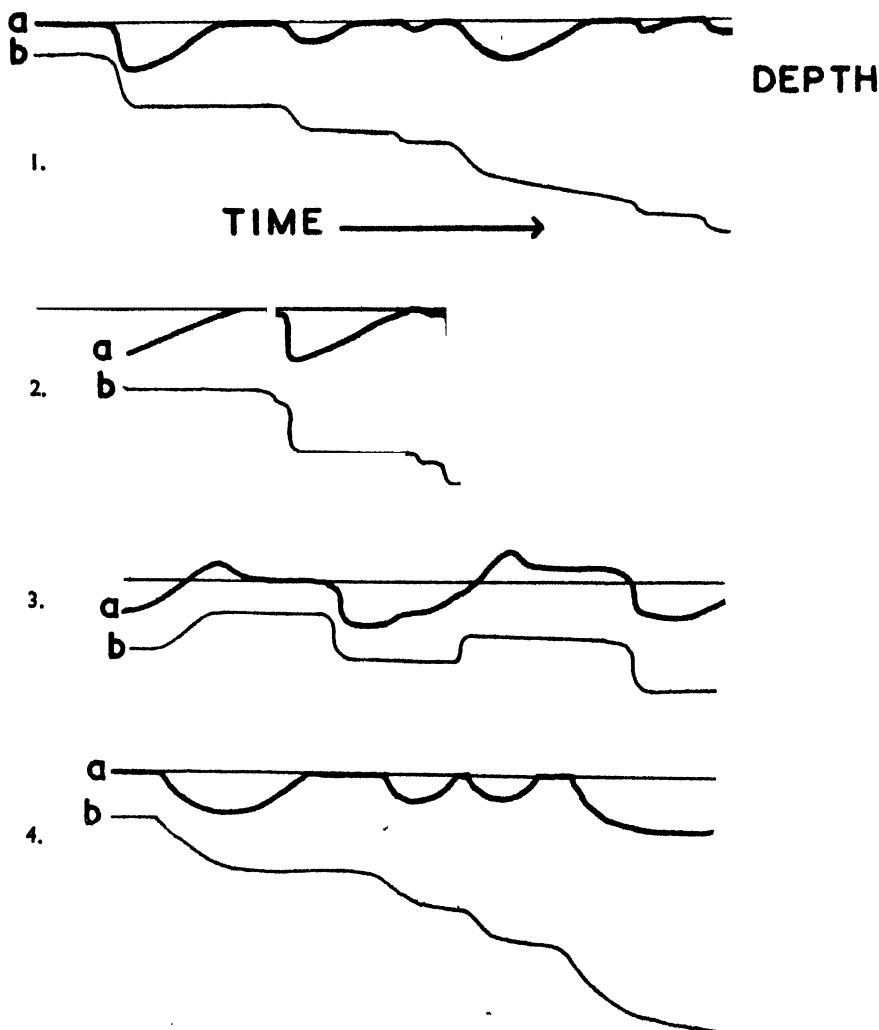
coal
 rootlet bed
 sandstone
 non-marine shale or mudstone
 marine band.

Typically the marine band rests on the coal of the cycle below. Details of this cycle (or cyclothem of recent American writers) have in some instances been elaborated to show the succession of varied types, particularly of marine and non-marine shales (*e.g.* Robertson, 1932, p. 88). But, while much can no doubt be learned from closer study of such details, which show interesting differences in different regions, the outstanding feature in the sequence in the productive parts of the Coal Measures throughout north-western Europe is the almost universal occurrence of the simple five-fold rhythm set out above. Not all the cyclical units are complete : in this country a marine band is frequently omitted, and locally at least the coal seam may be missing. A similar cyclical sequence is found in coal-bearing deposits of other ages and it appears to reflect the depositional conditions which are essential for the accumulation of a mass of sediments containing a number of widespread coal seams.

This fundamental cyclical pattern presumably reflects an intermittent subsidence of the area of sedimentation ; possibly in some cases a eustatic rise of sea level would lead to similar results, but, in general, where the accumulation of a total of some thousands of feet of sediment is involved, subsidence rather than a rise of sea level must have occurred. So far as the British Coal Measures are concerned, it seems that on some occasions a new cycle was initiated by a comparatively rapid submergence, introducing marine or " non-marine " (but not necessarily fresh-water) conditions. If there were no further subsidence during the depositional cycle, the sediments may be regarded as having accumulated until they filled up the area approximately to water level. Sedimentation overtook subsidence. This permitted the growth of the swamp vegetation, the area being progressively invaded. Stigmarian rootlets are predominant in most rootlet beds, and the existence of a variety of Lycopods adapted to the invasion of shallow water may well have been a factor in the development of coal seams : in some respects comparison may perhaps be made with the *Rhizophora* type of mangrove.

The time during which a coal seam was being formed may

have been considerable ; the original peaty deposits may have been at least ten times the thickness of the present seam. The formation of a thick seam may thus reflect a long period of "still-stand" or very gentle subsidence. Some of these interpretations are incorporated in Fig. 1, where the upper heavy line *a* represents the



FIGS. 1-4.—Diagrams to show (a) changes in depth with progressive time during successive cycles of deposition, and (b) the corresponding (epeirogenic) curve showing stages in subsidence of the area.

Fig. 1 shows a generalised interpretation of conditions in part of the British Coal Measures. Fig. 2 is based on Pravost, 1930, Fig. 3. Fig. 3 is based on Waller, 1930, Fig. 5. Fig. 4 is based on Tchernyshev, 1931, Figs. 1 and 2.

(Reproduced by permission of the Council of the Geological Society from the "Quarterly Journal" of the Society, GLI, 1946, ivil.)

possible changes in depth of water during the formation of a series of six cycles : where the line runs horizontally at water level, the area was a swamp and a rootlet bed was formed, and, if other conditions were suitable, coal also accumulated. The beginning of the first cycle is shown as due to a very sharp subsidence, after which deposition steadily reduced the depth of water ; other cycles are indicated as resulting from less-sudden submergence. The lower line *b* shows the actual movement of a point in the base of the deposits, emphasising the view that the intermittent subsidence was not necessarily interrupted by any elevation.

CYCLIC DEPOSITS OF SOME OTHER TYPES

The cyclic changes in some other areas are summarised for comparison with the interpretation of the normal British cycle in the remaining Figs. 2-4. They have been fully discussed elsewhere (Trueman, 1946, pp. lvi-lxii). The sequence of events suggested by Pruvost (1930) in France (Fig. 2) is in many ways similar to that already described. In parts of the Donetz basin (Fig. 4) it has been suggested that the cycle of changes follows a rather different pattern ; the coal is followed by non-marine beds, and these by marine beds, the presumption being that the subsidence which initiated the cycle was more gentle and continued for some time, culminating in the marine phase in the middle rather than at the beginning of the cycle.

Weller (1930) has drawn attention to the cyclic pattern in many parts of the Upper Carboniferous succession of the United States. He emphasised a rhythmic sequence in which coal is succeeded by thick marine strata, in turn to be followed by non-marine or continental strata (including a sandstone which frequently rests on an eroded surface of the marine beds and passes up into an underclay succeeded by the next coal). The most noteworthy feature of Weller's interpretation is that he considers that the area was raised before the erosion and the deposition of the sandstone ; this would carry with it the implication that the area was oscillating, the upward movement being always smaller in amount than the subsidence which brought in marine waters (Fig. 3). Weller (1930) and Wanless (1947) place the boundaries of cyclothems, not above the coal seams, but at the plane of unconformity, thus emphasising the uplift rather than the subsidence. In Britain the sandstone member of a cyclothem not infrequently has an irregular and eroded base, but this is usually ascribed to channelling of the unconsolidated muds by the swifter currents which carried in the sandy material. These swifter currents and the coarser sediments which they brought may,

however, have resulted from the relative uplifting of areas adjoining the region of deposition.

In different parts of the United States the Upper Carboniferous presents cyclic deposits of varied character. Thus the sedimentary cycles of the Mid-Continent region differ from those of Illinois and other eastern states in the greater prominence of marine deposits and the absence of a plane of erosion at the base of many sandstones (Moore, 1936).

The great lateral extent of many cycles has been stressed by several American workers, and it has been proposed to use them as a basis of correlation. Wanless (1947, p. 242) has noted that even thin cyclothems can be traced continuously over great areas, a fact which has also been remarked in Britain by Edwards and Stubblefield (1947), who have lately provided a detailed account of the sedimentary rhythm in that part of the Coal Measures of Nottinghamshire which lies just above the main coal-bearing strata; there, marine bands occur at the base of almost every cycle (from the higher part of the Lower Similis-Pulchra Zone to the top of the Upper Similis-Pulchra Zone) and each cycle can be traced, in spite of variations in thickness and in details of lithology, over the greater part of that coalfield. In fact, the recognition of cyclothems may be of fundamental importance in economic work.

A cyclic arrangement similar to that of the Coal Measures (Upper Carboniferous) of Britain is exhibited by the Limestone Coal Group (Carboniferous Limestone Series) of Scotland, although the details of the sequence await fuller examination. The associated Limestone Groups have a rhythm of different type, with a greater development of marine strata and more rarely a notable coal. These rocks (including the Limestone Coal Group) are broadly equivalent to the Yoredale Series of Yorkshire and Northumberland, and the lower part of the Millstone Grit*; in these latter rocks also rhythmic sedimentation of various types has been noted by a number of authors (*e.g.* Hudson, 1924; Wright, 1927). Brough (1929) has emphasised the widespread character of rhythmic sedimentation in Scotland and northern England at this period. It is apparent that the rhythmic changes influenced a much wider area than that in which there was conspicuous coal formation.

EFFECTS OF COMPACTION OF SEDIMENTS

Diagrams such as those in Figs. 1-4 have been drawn to illustrate the presumed depositional history of various Carboniferous

* I am indebted to Dr. E. D. Currie for unpublished information concerning this correlation.

and other sediments. In no case, however, is it possible to draw them with complete accuracy in respect to the scales of time or depth. It is generally supposed that the length of time required for the accumulation of one foot of coal may have been greater than that occupied by the deposition of a similar thickness of most of the associated sediments, while it is certain that a foot of mudstone generally required a much greater time than a foot of sandstone. But an absolute time-scale cannot be constructed for the stages represented. Nevertheless, if adequate correlations are available, graphical comparisons of the sedimentary cycles in different areas are not dependent on the provision of a complete time-scale, provided that equivalent cycles in different localities are shown as having similar duration.

On the other hand, it may be suggested that future work should make it possible to estimate with reasonable accuracy the actual amounts of the subsidence in those areas where sufficient data are available regarding the sedimentary sequence. This is not a matter for direct measurement, however, for the thickness of the sediments which accumulated in a given cycle, while immediately related to the amount of the subsidence resulting from crustal movements, was also affected by the space afforded by the compaction of the underlying sediments. Moreover, the sediments now forming a given cycle are thinner than was the case before the higher cycles were laid upon them, for they themselves have undergone compaction; indeed, the lower clays of one cycle must have been reduced in thickness by the weight of the upper members of the same cycle.

An analysis of these problems should make it possible to determine for each cycle the approximate extent of crustal subsidence, as distinct from space yielded by the compaction of the underlying sediments. Comparison of the results for equivalent cycles in different areas would be more reliable and probably more illuminating than estimations of crustal subsidence based merely on the present thickness of the sediments accumulated in a given cycle.

There are many problems to be solved before these results can be made available. In a mass of sediments comprising unconsolidated muds, sands and the peaty material from which coal is formed the various constituents will have undergone varying degrees of compaction. The reduction of peaty material to coal may represent a reduction to one-tenth or less of the original material. There is some evidence which suggests that the conversion to coal occurred fairly soon after deposition. The compaction of an underlying coal seam thus proceeded as the sediments were laid above it.

Similar factors also affected the consolidation of the muds. When first deposited, muds contain a high percentage of water, determined in some samples by Sorby as from 75 to 86 per cent. of the total volume. Sorby calculated that some shales and slates may occupy only a ninth of the volume they originally possessed (1908, p. 214). Since this reduction in volume can rarely have been attained by lateral shrinking, it follows that generally there must have been a diminution in thickness corresponding to the decrease in volume. The effects of wide differences in compressibility between sandy and muddy deposits are seen particularly in parts of the Coal Measures where sandstones are of restricted extent or where a sandstone bed varies greatly in thickness. For example, a cyclical unit, consisting of 60 feet of clay, may locally expand to a total of about 100 feet with the incoming of a sandstone bed some 50 feet in thickness ; that is to say the total thickness of the cycle may be increased by an amount almost equal to the thickness of the sandstone (*see*, for example, Dix and Trueman, 1928, p. 92). This may have resulted from the deposition of sediments approximately equal in original thickness at the two places, the sandstone having undergone little reduction, while the clays have been greatly compressed. Further, it should be emphasised that such irregular distribution of rocks of variable compressibility in one cycle must normally have exercised a marked influence on deposition of the succeeding cycle, for there would be less rapid compaction in the area underlain by the sandstone as compared with the area where the whole thickness was made up of clay. Thus, on the whole, the deposits of the succeeding cycle may be expected to be thinner in the sandstone area than elsewhere. In other words, the total thickness of two or more adjoining cycles may show less local variation than the thickness of a single cycle. A consideration of such variations in thickness provides a basis for estimating the probable ratio of compaction of the clays to increasing load.

Recent work on clays provides additional data. It is clear that as overlying sediments accumulate the void-ratio of a buried mud decreases, thus causing a reduction in the thickness of the original mud layer. There are records of numerous observations bearing on this problem, partly in relation to soil mechanics and the stability of more superficial clays, and partly in relation to deeper sediments met with in oil wells (for example, Hedberg, 1936) ; in connection with the latter problem Athy (1930) has examined many determinations of density and porosity of sediments with reference to presumed ultimate depth to which the material had been buried,

and discussed the connection between them. In this country Skempton (1944) has made investigations of some British clays and has shown that the compression curves depend to an important extent on the type of clay, while O. T. Jones (1944) has indicated the bearing of these studies on several geological problems.

The detailed application of the available results to a study of Coal Measures sediments, preferably linked with the examination of the cores of a deep boring, would yield information of great interest. It would be possible to form a reasonable estimate of the original thickness of the sediments in each cycle and also to determine very approximately for a particular cycle what subsidence had occurred during its deposition as a consequence of the compression of the underlying sediments. Although several factors (including the wide variety of muddy-silty sediments) would preclude absolute accuracy in these determinations, it would be possible to draw general conclusions concerning the approximate amount of the subsidence due to crustal movement at the beginning of (or during) a cycle.

MINOR AND MAJOR RHYTHMS

The occurrence of rhythmic changes as a characteristic feature in the history of the earth has long been remarked. In the widest sense, the cycles marked by deposition, mountain-building and erosion provide the basis for some of the main sub-divisions of geological time. The almost world-wide marine transgressions and regressions, the orogenic episodes and related igneous activity present a pattern, repeated with variations, which many have ascribed to "rhythmically changing processes in the substratum" (*e.g.* Umbgrove, 1939, p. 126; 1947; Holmes, 1944, etc.). Moreover, in working out the history of the mountain-building movements, it has become clear that each major orogeny was marked by several phases, and the possible connection of these with changes of sea level in more distant areas has been noted and possible explanations have been suggested (*e.g.* R. C. Moore, 1937, p. 673).

The relation of the minor rhythms characterising the deposition of coal-bearing sediments to these wider oscillations has received little attention. It is certain that most minor rhythms reflect an intermittent subsidence of a part of the crust; it would appear that these pulsations may have influenced areas where no conspicuous coal seams occur (though on some occasions these latter areas may have been contiguous with typical areas of coal formation), but it is uncertain whether failure to recognise their effects elsewhere is wholly to be interpreted as evidence that they did not

occur. For example, a minor rhythm of the kind which gave rise to the cyclic sediments of the Coal Measures may have resulted from relatively sudden subsidences of no more than 50 to 100 feet, sufficient to produce extensive changes in large areas of coastal swamps, but likely to produce little noticeable change in a sea bottom a hundred fathoms in depth. Provided that sedimentation does not almost overtake subsidence, the minor rhythm may thus escape detection. The broad rhythm shown by the Chalk is comparable in some respects with that seen in the Carboniferous Limestone in some parts of South Wales, but in neither is there good evidence of minor oscillation.

That this minor rhythm may at times be superimposed on oscillations of larger amplitude is suggested by the evidence brought forward by R. C. Moore (1936) of the existence of "cycles of cycles" in the Carboniferous rocks of Kansas. Such a cycle of cycles he has called a megacyclothem. The possibility that in some areas a grouping of cycles may be due merely to modifications in the environment, perhaps the result of other factors, is suggested by the sequence of part of the Coal Measures of Nottinghamshire described by Edwards and Stubblefield (1947); here a series of cycles (from the middle of the *Modiolaris* Zone to about the middle of the Lower *Similis-Pulchra* Zone) all commence with a non-marine phase, to be followed by another series practically all beginning with a marine phase. These two series may possibly for descriptive purposes be regarded as megacyclothems (though different in character from those described by Moore), but the rhythmic subsidences were not necessarily of different character in the two stages. If during the first the sea were excluded from the area, but there was no effective barrier to its entry in the later stage, it is unnecessary to suppose that the subsidences marked by marine bands were greater in amount than those of the preceding stage.

Other rhythms of varying amplitude call for consideration in relation to this problem. The periodic transgressions shown by the Lower Tertiary rocks of the London-Hampshire-Paris basins, and the changing sea levels of the Pliocene (and to some extent of the Pleistocene), present different aspects of the problem; other features of the Pleistocene illustrate in part how factors other than periodic crustal changes may produce a rhythmic effect (Umbgrove, 1947, p. 91). Moreover, rhythmic sediments may result from climatic oscillations.

The sedimentary cycle developed in the Coal Measures, when compared with other rhythmic sediments, has some unusual features. It represents a type of sedimentation characteristic of shallow-water

areas in which deposition is so far in excess of subsidence that the areas of deposition remained approximately at water level for comparatively long periods, during which vegetable matter accumulated ; the cycle for its full development requires an area where the times of no subsidence are not interrupted by any marked uplift ; if coal seams of regular thickness and wide extent are to be formed, it requires that the area is not affected by important differential movement over considerable periods, in other words that the area is one of some uniformity of behaviour. Not infrequently, of course, some departure from such uniformity is indicated by the splitting of seams or by the diminution in thickness of both coals and associated strata in particular regions.

Such lateral changes have frequently been determined by differential subsidence controlled by deep-seated structures. Thus over buried anticlinal structures the total subsidence during a particular period may be much smaller than that over the intervening basins, as shown by the differing thicknesses of sediments deposited. Very often the coal seams are thin in the more slowly subsiding areas, but there are instances of the reverse relationship, and very thick coal seams may represent the coalescing of a number of thinner seams which elsewhere are distributed through a considerable thickness of other rocks (as in the well-known case of the Thick Coal of south Staffordshire).

Evidence of such control by more deep-seated structures is abundant even in those parts of the Coal Measures in Britain where the strata show their greatest uniformity and where cyclic sedimentation is most regularly developed. This may be interpreted as giving support to the view that the rhythmic changes were the result of intermittent subsidences rather than recurrent rises of sea level, though it must be agreed that a combination of both factors is not excluded.

These conclusions raise the question of the possible relation of the minor rhythms to the more considerable orogenic movements of the periods. The study of this problem must, of course, include an examination of the time-distribution of all the chief coal-bearing sediments of the world and their relation to the major phases of mountain building ; this would require more exact knowledge of relative geological ages than is yet available in many areas. The occurrence of some coal-bearing sediments in all Systems from the Devonian to the Late Tertiary might be held to suggest that the pulsations causing the cyclic deposition were continuous or nearly so (and not themselves periodic), giving rise to rhythmic deposits wherever other conditions were favourable. But the

time-distribution of important carbonaceous deposits appears to be too irregular to support this suggestion.

The Upper Carboniferous rocks throughout the great belt from Texas to China are notable for their coals, and it is reasonable to accept the view put forward by Du Toit (1937, p. 156) that the development of conditions favourable to the laying down of coal seams was patently determined and controlled by the rising of anticlines and the deposition of the waste from these emergent ridges in the adjacent deepening synclines. Rhythmic sedimentation was almost continuous during the deposition of the Upper Carboniferous rocks of Britain. Rhythmic movements immediately succeeded the important Sudetic orogeny and were maintained until the whole of the Ammanian (approximately the Lower and Middle Coal Measures) had been deposited; during this time the differential subsidences of the basement exercised a generally decreasing influence and the rhythmic changes affected an increasing area of remarkable uniformity (Trueman, 1947, p. xevi). The Morganian movements caused local interruption to deposits of this type, but rhythmic subsidence was maintained in one locality or another in Britain until late in Carboniferous time.

It appears reasonable to suppose that rhythmic phenomena, which provided the characteristic setting for the cyclical coal-bearing sediments, were minor expressions of the orogenic movements of the period, and perhaps an indication of the pulsatory nature of those movements.

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THE SPRAYING OF LIQUIDS FROM AIRCRAFT

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THE spraying of liquids from aircraft has two main intentions : to produce smoke by the use of liquids such as titanium tetrachloride and chlorsulphonic acid which form condensation nuclei, or to spray the atmosphere and crops with a suspension of an insecticide. Both applications require that the liquid shall break up into drops of a determinate size. The physicist is called into these problems to calculate the optimum drop size at the ground for each purpose, and to investigate the conditions of break-up of the jet at the aircraft and so dispose them that the correct drop size shall result at the ground. It is not possible in fact to get all drops of the same size at the ground, but he may hope to get them distributed about the mean optimum size with as little spread as possible.

The liquid is usually forced out of a container, carried on the aircraft, under pressure. The jet breaks up into drops which evaporate as they fall. Given the optimum drop size at the ground, two problems in hydrodynamics at once impose themselves : (1) What should be the mean drop size on formation at a definite height above the ground ? ; (2) How may this initial drop size be assured ?

Evaporation of course diminishes the size. If the drop initially exceeds a certain size it may disintegrate suddenly. There is also the possibility of coalescence by collision, particularly near the source where the concentration is greatest. For the moment, let us ignore the second and third factors and consider an isolated drop under the combined effects of gravity, air resistance and evaporation. Apart from evaporation, every drop has a definite terminal velocity under the influence of the other two factors. For small drops obeying Stokes' law this velocity is proportional to the square of the diameter or to the surface, but, owing to the characteristic variation of resistance with Reynolds' number, there is a falling away from this proportionality even for drops of 2 mm. diameter.

Regarding evaporation as a diffusion problem—as a balance between the molecules which diffuse into the vaporous region and those which return—Langmuir [1] obtained the formula for the rate of decrease of surface :

$$-\frac{ds}{dt} = \frac{4\pi MD}{\rho} \cdot \frac{p}{RT}$$

where M = molecular weight, R = gas constant, D = diffusion coefficient, T = absolute temperature, ρ and p are the density and vapour pressure of the substance.

This equation does not contain any term for convective evaporation, such as occurs when the drop is moving relative to the atmosphere. Frössling [2] investigated this effect by suspending water drops on glass threads in a small wind tunnel. He found that the rate of evaporation was a function of Reynolds' number (Re), being greater than that in still air by the factor

$$F = \left(1 + \frac{.276\sqrt{Re}}{\sqrt{D/\nu}}\right)$$

where ν is the kinematic viscosity of the liquid.

Given a value of ds/dt we can now retrace the path of the drop as a free-fall-velocity : surface-area curve by assuming that it starts from nothing at the ground level and increases in surface by a constant amount for every ten seconds of its life. This gives the right-hand broken curve of Fig. 1—for water drops—to which a time scale has also been attached at the top. But owing to convectional evaporation the lifetime of the drop is less than this curve would indicate, so we correct it by calculating its Re for various stages (shown alongside the curve) and shifting the corresponding points to the left, so that an increment of surface that formerly took place in 10 secs. now occupies $10/F$. This gives the left-hand broken curve. Now we calculate from this curve the *height* at which the drop will have a given surface ; these are the figures alongside the latter curve. The full lines are then height : surface curves for three values of rate of evaporation. If, instead of vanishing as it hits the ground, a drop possesses a finite size and surface, it is only necessary to "raise the ground," i.e. the horizontal axis of the graph, to meet it at the appropriate value of the remanent surface.

Observations on the evaporation of freely falling drops to check the formula have been made by the writer in the interior of the pagoda at Kew Gardens. This tower had till recently holes out in each floor, so that a fall of 40 metres was available, during which

the progress of a drop could be watched through a telescope mounted on the top storey. The size at the end of fall was determined by catching the drop—dyed if necessary—on filter paper at the foot of the tower, after the relevant calibration of drop size and stain diameter. The initial size was determined by the volume released from a microburette required to produce a given number of drops, and by photography. A photograph of a drop of methyl salicilate,

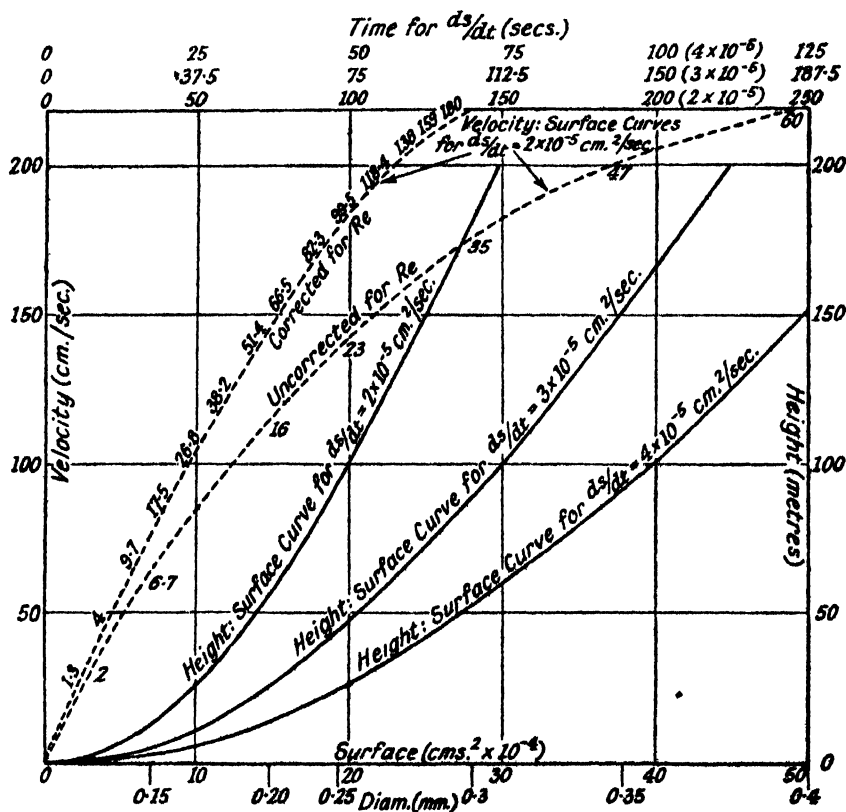


FIG. 1.

about 1 mm. diam.—reproduced from a cinematograph film—is shown in Fig. 2a.

The diminution of size with height deduced from these experiments pointed to a rather greater rate of evaporation than in the Langmuir-Frössling formula, particularly in the final stages of the drop's existence. In fact, small drops which, in the light of Fig. 1, should have reached the floor without evanescence were observed to peter out *en route*, often pursuing a sinuous path at the last. It has been stated that very fine raindrops have an abnormal rate of



(a) (b)
Falling drops: (a) methyl salicylate, (b) titanium tetrachloride.

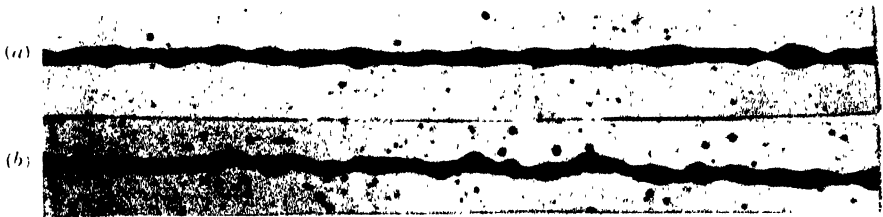


FIG. 3 -- Jets breaking up under (a) capillarity, (b) viscosity.



FIG. 4. Jet ejected into slipstream of aircraft propeller.

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evaporation for their size, but, in so far as such deductions are made from measured time of fall, it may be that the vagaries of their tracks in the final stages have not been reckoned.

Very large drops were seen to break up as they fall into parts. Bond [3] investigated this effect in connection with the motion of liquid drops in another liquid and found the significant parameter (which the writer proposes to call the Bond number) to be $(\rho - \rho')d^2g/\sigma$, where d is the diameter, ρ and ρ' the respective densities and σ the interfacial tension. The critical value for break-up in the Kew experiments was about 4, confirming Bond's value, and giving 1 cm. as the critical diameter for raindrops falling at their terminal velocity in air. This sets a superior limit to the "initial size of drop" in the light of this discussion, which can be regarded as operative.

Small drops retain their spherical shape, but the large ones flatten out, acquiring a mushroom shape before disintegrating. Sometimes a miniature explosion seems to disrupt the drop, which then reaches the mat in the form of a spherical core surrounded by a chaplet of droplets deriving from the fringes of the umbell.

The smoke-forming drops are normally spherical, but the products of evaporation—the smoke nuclei—are particularly obvious in the lee of the projectile, giving the appearance of a comet tail (*cf.* Fig. 2*b*). It is in virtue of the hygroscopic action that these liquids are efficacious, titanium tetrachloride being able to take up half of its mass again to form smoke, providing the water is there to be absorbed. Smoke formation is, therefore, particularly dependent for its effectiveness on the humidity of the atmosphere.

Turning now to the aircraft end of the drop's history, one needs to examine the conditions under which the jet breaks up. The jet is usually directed backwards along the line of flight under pressure, so that the relative speed of liquid and air may be any value desired, as determined by the speed of the aircraft and the speed of ejection.

At low speeds a jet of liquid resolves into drops under the interaction of inertia and capillarity, an effect which may be observed in the slow issue of water from a domestic tap. The disturbance which results in the formation of drops begins as an oscillation of figure symmetrical about the axis. As these grow the jet becomes varicose, until the amplitude of swelling equals the radius of the jet and a drop of approximately twice the diameter of the jet is nicked off. At higher speeds it is friction with the air which breaks up the jet into less regular fragments. Photographs of a jet near the breakdown point in the two cases are shown in Fig. 3. It is the latter type which prevails generally in aircraft jets of water and

aqueous solutions, and it can be shown that two parameters are significant for the dispersal in drops, i.e. the Reynolds number of the jet—mean velocity times diameter divided by kinematic viscosity—and the relative viscosities of the liquid and the surrounding air. Thus for a given liquid squirted into air, of constant viscosity, the mean drop size is found to depend on the relative speed inversely and the one-fifth power of the kinematic viscosity of the liquid directly.

If drop size is to be entirely governed by relative velocity the jet must be ejected back from the aircraft and well below the fuselage, but, when very fine drops are desired, the excessive turbulence produced by the airscrew may be made to help by ejecting the liquid into the slipstream (Fig. 4).

By regulating the pressure of ejection it is possible to control the relative velocity (for jets clear of the slipstream) and so the mean drop size on break-up. Further, if the height of the aircraft is known, it is possible to use data like those of Fig. 1 to predict the mean drop size at the ground. Such theoretical predictions have been confirmed by flights over the runways of the Royal Aircraft Establishment, during which the drops of liquid (suitably dyed) were caught on large sheets of blotting paper. After conversion of stain size to drop size—in the light of a previous calibration—summation curves indicating the percentage by volume of the liquid which resolves into drops less than a specified size were constructed. Two such curves are shown in Figs. 5 and 6 (cf. Merrington and Richardson [4]) for various speeds, some from the aircraft and others from a stationary ejection chamber mounted on a tower over the papers.

If the relative velocity chosen was a low one, so that break-up was influenced rather by capillarity than by viscosity, the drops were more uniform in size and limited to a maximum diameter about double that of the nozzle, whereas at high speeds nozzle bore had practically no effect.

It remains to say a little on the biological aspects of spraying. A certain amount of work was done during the war at the London School of Hygiene [5] on the reaction of mosquitoes to spray mists in enclosed chambers, the factors measured being mean particle size, concentration and toxicity. From the point of view of killing in flight, when the atmosphere is sprayed, the effective droplets are less than 100 microns in diameter. For spraying the crops themselves, in order to kill sitting insects, larger drops are needed. The quantity accumulated by insects in flight depends on their activity, since this increases the chances of collision with drops and with the

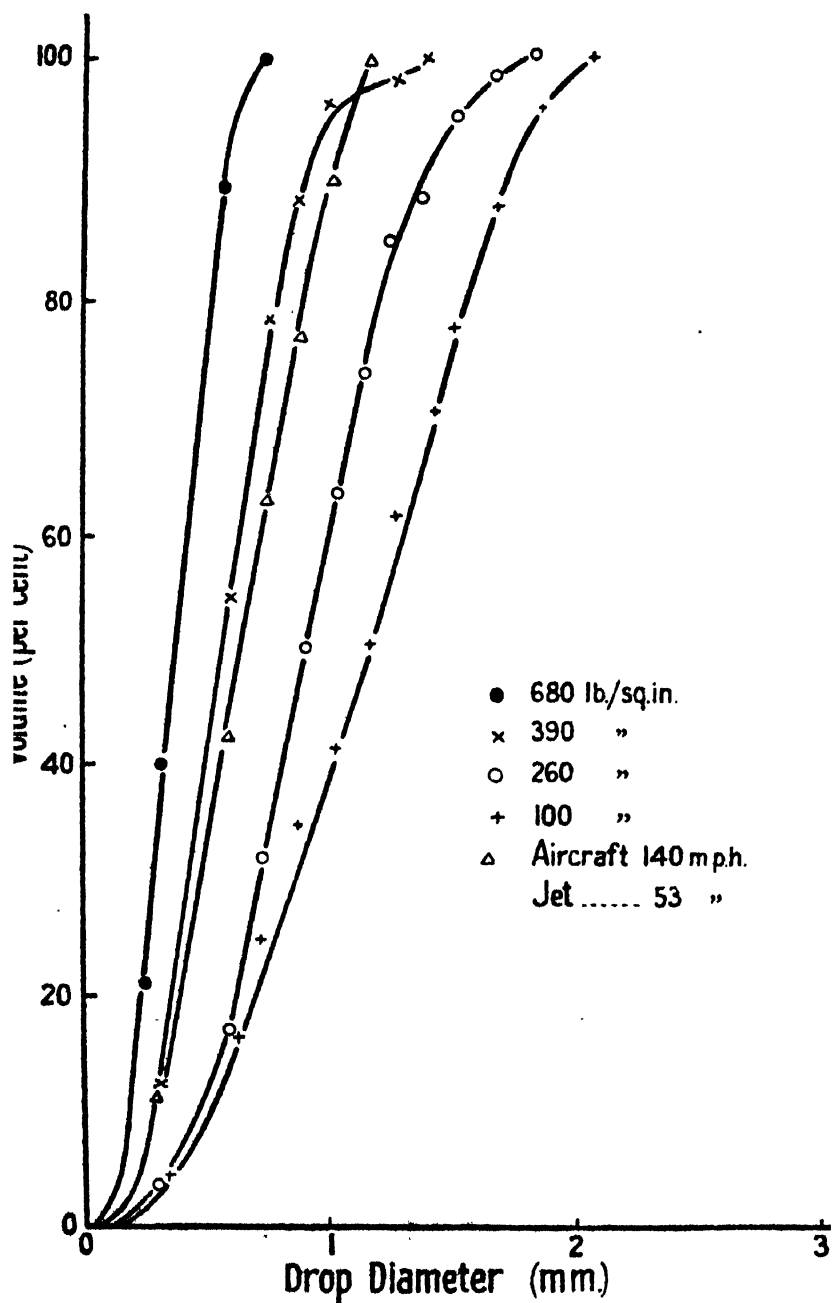


FIG. 5.—Zinc chloride jets.

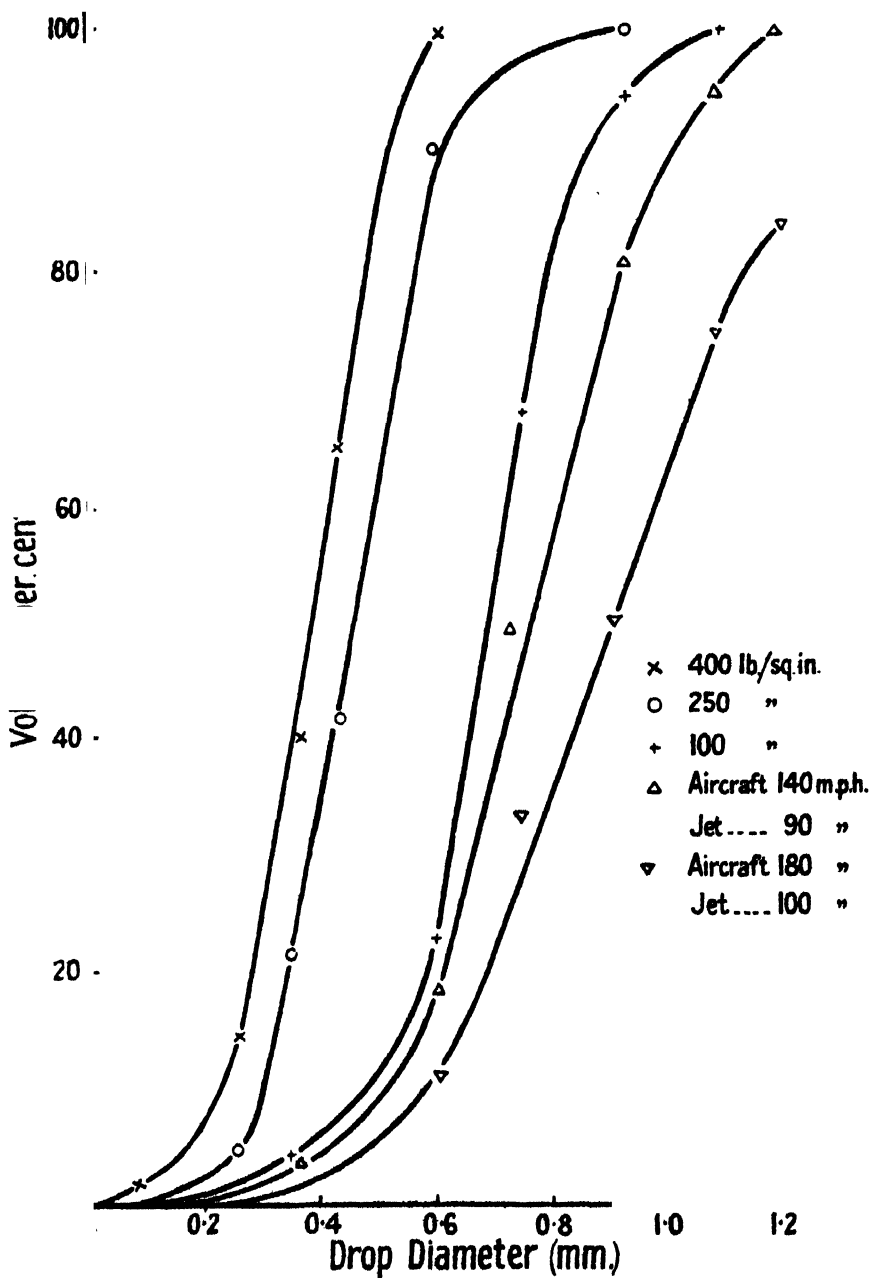


FIG. 6.—Titanium tetrachloride jets.

concentration of the spray in space. Thus the toxic efficiency of a mist decreases with its age as the small drops coalesce and settle. The insecticide itself is often non-volatile, so that, as the medium which supports it evaporates, the remaining size reaches a limit.

Little work seems to have been published yet on the optimum drop size for sprays on to foliage. Once this has been determined by biologists, the requisite aircraft flight conditions can be deduced from the physical research work on the problem, which it has been the main object of this article to set forth.

In concluding, one further point may be mentioned which is receiving attention; the question of getting the insecticide on to the underside of leaves of trees where insects may otherwise escape a lethal dose. For this purpose helicopters are being tried, the drops being carried by the downwash of the rotating wings first towards the ground and then upwards in a return flow which the ground as obstacle imposes on the air current.

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ELECTRICAL PROPERTIES OF NERVOUS TISSUE *

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THE cell is the unit of which living tissues are built. For our present discussion we can regard the cell as an organised aggregate of chemical material marked off from its environment by a specialised boundary, the cell membrane. This membrane is not necessarily the structure which can be seen under the microscope in stained sections of dead tissue, or even in living tissue as viewed by polarised light or with the phase contrast microscope.

The majority of animal and plant cells are normally immersed in a fluid environment, and the membrane acts so as to prevent both dispersal of the cell contents into the surrounding fluid and the invasion of the cell by material from the environment. The cell membrane is, therefore, a semi-permeable structure with respect to certain components on each side of it. The potassium ion, for example, is present in high concentration within nerve fibres and in low concentration outside, *i.e.* in the tissue fluid or cell environment [24]. The ratio is approximately 30:1. Such a difference would account for a potential difference of 85 mV between the inside and the outside of the fibre. Recent work suggests a probable value of 60 mV across the membrane. The inside of the nerve cell is negatively charged and the outside positively charged.

That the cut surface of a muscle was electro-negative with respect to the intact surface was discovered over 100 years ago by Matteucci in 1838 and Du Bois-Reymond in 1843. We now recognise that the site of this injury or demarkation potential is the intact membrane underneath the reference electrode. The cut surface merely serves to provide a more or less direct connection with the inside of the muscle fibre.

The cell membrane is probably a protein-lipoid complex, and

* Based on a lecture to the Institute of Physics (London and Home Counties Branch) on October 16, 1947.

may have orientated molecular dipoles within it [10]. It is sometimes convenient to regard it as a network, through the holes of which small enough molecules or ions might be able to pass under suitable conditions of charge and concentration difference.

The capacitance of cell membranes is found to be of the same order for a wide range of animals and tissues [1]. For example :

Squid and crab axon	1.1	$\mu\text{F.cm}^{-2}$	[6, 15]
Lobster axon	1.33	" "	[17]
Arbacia egg	0.73	" "	[4]
Red blood cell	0.81	" "	[11]
Trout egg	0.57	" "	[22]

This similarity suggests very strongly a structural resemblance between the different membranes. On the assumption that the capacitance arises from a parallel plate condenser of small dielectric thickness and dielectric constant K , the membrane thickness is found to be of the order of $10K \text{ \AA.U.}$ For a thickness of 1μ , K would have to be approximately 1000. No satisfactory physico-chemical explanation to account for this capacitance has yet been advanced [5].

In general, the capacitance of cell membranes remains remarkably constant under many different conditions, but the trout egg shows cyclical variations of 5 to 10 per cent. with a period of one and a half minutes [22, 23]. The fact that the capacitance is practically unaltered with injury, death and ionic permeability changes suggests that the bulk of the membrane volume is occupied by what Cole has described as "a massive, inert and durable framework"—the porous structure to which reference has already been made.

Changes in membrane reactance, such as occur in nerve when a propagated action potential is evoked, are more usually associated with resistance than with capacitance; but in the plant cell *Nitella*, a 15 per cent. change of capacitance is recorded when an action potential is evoked [7]. An inductive element in nerve membrane reactance has been introduced by Cole and Baker (1941) and Cole (1941) in order to explain membrane phenomena observed by them [2, 3].

With the present limited knowledge of membrane structure it is impossible to say whether capacitance and membrane potential are related. The small change of 2 per cent. in capacitance of squid nerve axon, observed when propagated action potentials arise [8], may be adequate to explain the large potential reversal to which reference will be made later.

In nerve fibres, the polarisation of the membrane can be modified and destroyed by application of suitable stimuli. If the

depolarisation is of sufficient intensity, and spreads over a large enough area of nerve surface, it propagates itself over the remainder of the nerve. This propagated disturbance is called the "action potential" (Fig. 1). The membrane returns to its original polarised condition a few milliseconds later.

The property of responding to adequate stimulation by the production of a propagated action potential is referred to as "excitability."

Nerve and muscle fibres are excitable tissues. The existence of excitation in nerve is only demonstrable indirectly, *e.g.* by ultimate effects or by electrical recording. In muscle the wave of electrical excitation is accompanied by visible contraction.

Thermal, mechanical, chemical and electrical stimuli are all capable of exciting nerve cells, but electrical methods are the methods of choice, since they give the maximum precision and reproducibility

and can be graded accurately in intensity. When an electric current is passed through a nerve, if an action potential is evoked at all it originates only at the cathode. Current flow in the cathodal region tends to depolarise the nerve fibre membrane, *i.e.* to lower the potential, outside the fibre, relative to an inactive portion of the nerve. In the vicinity of the anode, the potential rises and the membrane polarisation is enhanced. Cathodal polarisation is termed catelectrotonus, anodal anelectrotonus. For a given current flow, the cathodal current density is always greater than the anodal: there is a greater spread at the anode. On cessation of current flow, the collapse of anodal polarisation is equivalent to the development of a cathodal depolarisation, and excitation may then occur at the anode.

The capacitative property of the nerve cell membrane is responsible for the spread of electrotonus in the neighbourhood of the electrodes. The electrotonic potential spreads spatially with exponential decrement

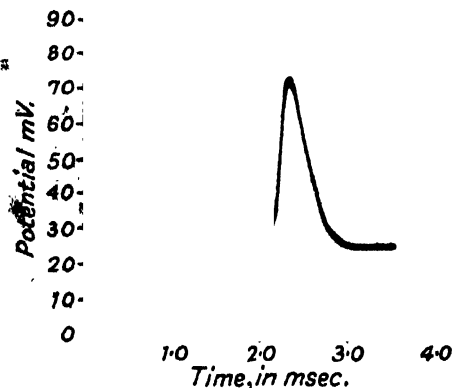


FIG. 1.—Typical monophasic action potential in a 490 μ fibre of the squid. duration is approximately 0.7 millisecc. The small initial downward deflection is the stimulus artifact occurring 1 millisecc. before the action potential.

(After SCHMUTT & SCHMUTT (1940), *J. Physiol.*, 98, 26.)

$$v = v_{\infty} e^{-x/\lambda}$$

That is to say the potential falls to $1/e$ of its value in a distance " λ ." Values of 0.5 to 3.0 mm. are recorded for λ the exponential space-constant.

If a brief current pulse of subthreshold intensity is applied to a nerve fibre, the potential changes evoked in the fibre outlast the duration of the pulse, i.e. the charge deposited on the membrane takes considerable time to dissipate itself. At any given distance from the stimulating electrode, the time course of the potential change is approximately exponential with time constant $\tau \simeq 1 - 3$ millisecc., viz. $v = v_{t=0} e^{-t/\tau}$ (subject to certain limitations to be discussed later).

Combination of the space and time decremental equations gives

$$V = V_0 e^{-(\frac{x}{\lambda} + \frac{t}{\tau})}$$

These transient potential changes can be readily demonstrated by application of a second stimulus a millisecond or so after the first. The effect of the second stimulus is superposed on the electrotonic trace of the first. The excitability of nerve is measured as a percentage by the reciprocal of the strength (in threshold units) of a second stimulus which will just excite. Thus, if the second of two stimuli needs to be $0.5 \times$ threshold intensity in order just to excite the fibre, the excitability at that instant is 200 per cent. Similarly, if a cathodal stimulus, following an anodal stimulus, needs to be $.4 \times$ threshold just to excite, the excitability at the moment of application of the second stimulus is 25 per cent. In this way it is possible to plot the time course of electrotonus and other local potential changes, and results obtained by this method agree with those obtained by the method of direct measurement.

Immediately following excitation of a nerve fibre, the fibre is absolutely inexcitable at the same locus. This absolute refractoriness lasts for one or two milliseconds. In the absolute refractory period no stimulus, however great, will excite the fibre. Recovery of excitability then occurs, rapidly at first and more slowly as 100 per cent. excitability is approached (Fig. 2). Complete recovery may take up to 5 or 10 millisecc. During the relative refractory period, a stimulus above the initial threshold is required to excite. Further, the action potential evoked during this period is of smaller amplitude than the first one. With repetitive stimulation at a rate such that each successive stimulus falls within the relative refractory period of its predecessor, there is a progressive decline in action potential size.

Absolute and relative refractoriness of nerve also occurs immediately behind the excitation wave as it travels along the axon.

The action potential leaves behind it a stretch of nerve in the recovery phase. This can be demonstrated readily by cooling a

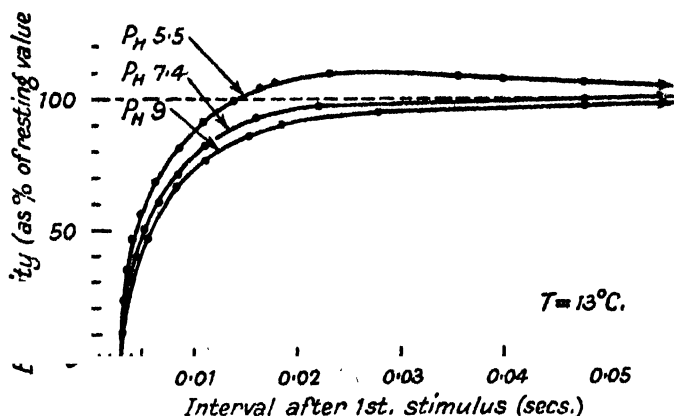


FIG. 2.—Recovery of excitability in frog sciatic nerve perfused with fluids of different pH.

(After ADRIAN (1920), *J. Physiol.*, 54, 1.)

stretch of nerve into which are fired two impulses close together. The temperature of the nerve can be adjusted so that the recovery

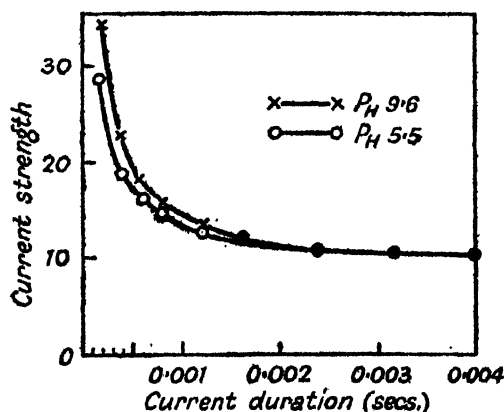


FIG. 3.—Strength-duration curves for frog sciatic nerve in fluids of different pH. The rheobasic current strength is approximately 10, and the minimal duration of the order of 100 μ sec.

(After ADRIAN (1920), *J. Physiol.*, 54, 1.)

process behind the first impulse is slowed down, and the second impulse dies out in the absolutely refractory stretch behind the first impulse. Hodgkin reports that cold blocks with an absolute refractory period up to 50 millisecc. have been observed [13].

Within certain limits, the longer the duration of the current pulse applied to a nerve fibre, the lower the intensity required just to excite the fibre. Increase of the intensity above that

required just to excite has no further effect on the fibre. The minimum intensity of current of (theoretically) infinite duration which will just excite is called the rheobase. In practice, a pulse

duration of 5–10 millisecon. is adequate to determine the rheobase. There is also a minimal duration of pulse which will excite whatever the intensity. Thus the *strength-duration* curve—i.e. the plot of the stimulus intensity and pulse duration values which just excite—is asymptotic to the rheobase and to the minimal duration (Fig. 3). It is sometimes used to characterise the excitability of a tissue. Interpolation on this curve will give the duration for which a twice rheobasic current must flow just to excite. Lapique termed this the “chronaxie.” The *strength-duration* curve is nearly linear for short durations. That is to say, the criterion for excitation by short duration stimuli is that a fixed quantity of electricity must flow through the nerve. This also follows from Hill’s two-factor theory of nerve excitation which treats the nerve fibre as a leaky condenser [12].

Impulse transmission can be blocked in a nerve fibre by cooling, by pressure, by drugs or by electric current flow (i.e. by production of anelectrotonus). Hodgkin, in 1937, using cold blocks and pressure blocks, showed that, when an impulse dies out in the block, a potential change can be recorded in the stretch of normal axon immediately beyond the block [13]. This potential, called by him the

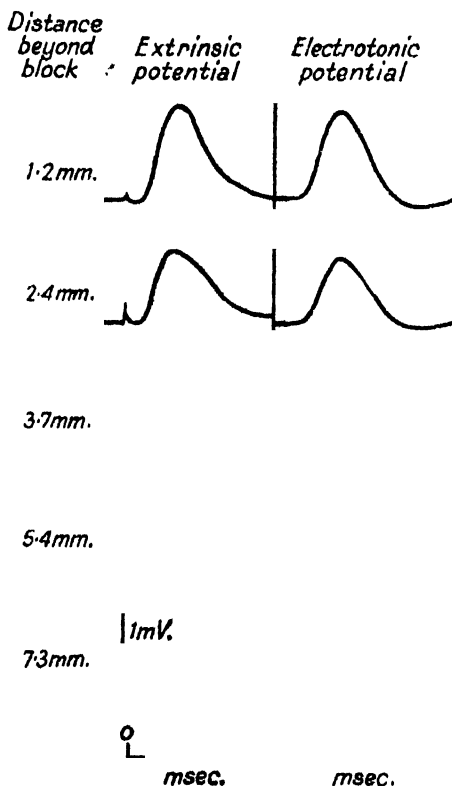


FIG. 4.—Showing extrinsic potential and applied electrotonic potential at corresponding distances beyond a 3.5-mm. cold block. Amplification and time scale are the same throughout.

(After HODGKIN (1937), *J. Physiol.*, 90, 183.)

extrinsic potential, he showed to be similar in space and time distribution to catelectrotonus (Fig. 4). An impulse which dies out in the block can decrease the electrical threshold beyond the block by as much as 90 per cent. (i.e. excitability increases to 1000 per cent.). If the block occupies a short enough stretch of the nerve, impulses will sometimes jump the block.

On the basis of these observations, Hodgkin suggested that the mechanism of extrinsic potential production, cathodal polarisation and of normal impulse propagation were the same, viz. local circuits of electrotonic currents in the vicinity of the already depolarised regions of the fibre (Fig. 5). The high resistance of the axon membrane with the low resistance of interstitial fluid and axoplasm

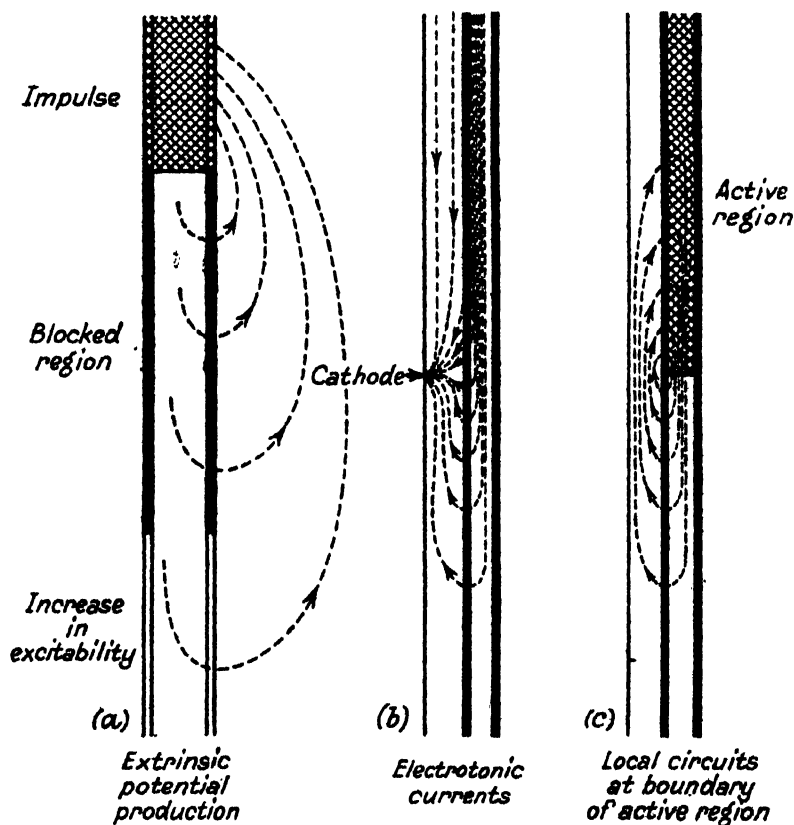


FIG. 5.—Showing the similarity between the mechanisms of (a) extrinsic potential production, (b) cathodal polarisation, (c) normal impulse propagation.

The dotted lines represent current flow in the conventional direction towards the cathode.

(After HODGKIN (1937), *J. Physiol.*, 90, 183.)

(i.e. the outer and inner media) implies that there will be eddy currents around the fibre forming a toroidal shell, with current flowing one way outside and the opposite way inside the fibre. This is the cable-theory of nerve impulse transmission, dating from Hermann in 1899, and modified and generalised by many subsequent workers.

If this explanation is correct, activity in one fibre should modify

the excitability of adjacent fibres. Although Blair and Erlanger in 1932 had been unable to demonstrate this, Katz and Schmitt in 1939 [20] showed mutual interaction between two adjacent nerve

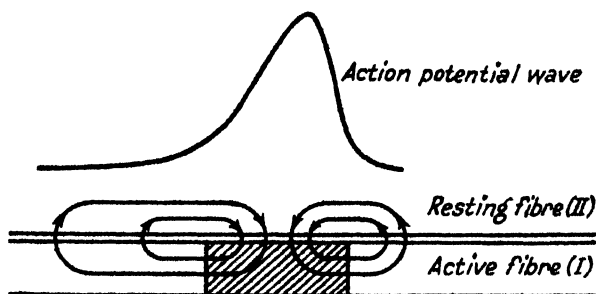


FIG. 6.—'Local circuit' diagram, illustrating the penetration of the resting fibre (II) by the action currents of the active fibre (I).

The shaded area indicates the active region of fibre I. The direction of the current penetrating fibre II reverses twice.

(After KATZ & SCHMITT (1939-40), *J. Physiol.*, 97, 471.)

fibres (Figs. 6 and 7). Furthermore, the extent of interaction demonstrated by them fits in with the order of magnitude of the action current wave revealed by Hodgkin, *i.e.* a safety factor of the

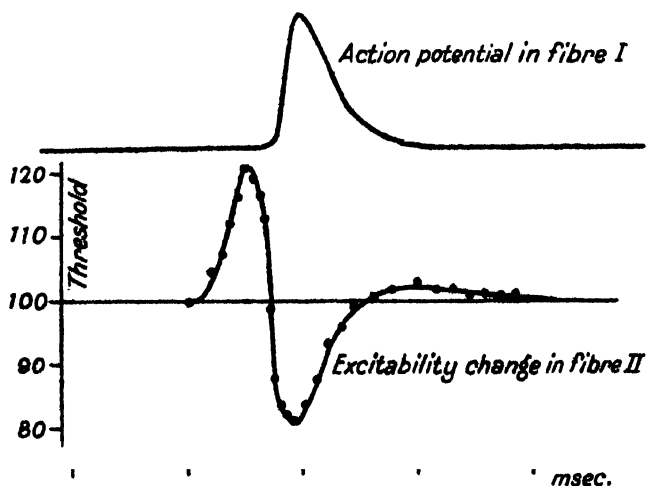


FIG. 7.—Showing excitability changes in fibre II during the passage of an impulse in fibre I, the time course of which is shown in the upper part of the figure.

(After KATZ & SCHMITT (1939-40), *J. Physiol.*, 97, 471.)

order of 10:1 — the action potential is 10 times the magnitude required just to propagate itself.

By direct measurement of the potential at and in the vicinity

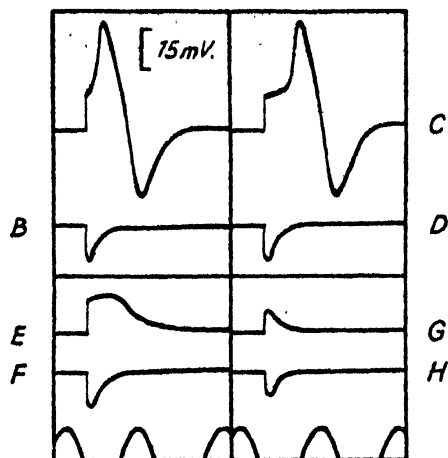


FIG. 8.—Showing potential changes at the stimulating electrode produced by stimuli with relative strengths (i.e. in terms of threshold strength) as follows: A, 1.05; B, — 1.05; C, 1.00; D, — 1.00; E, 1.00; F, — 1.00; G, 0.61; H, — 0.61. Time 1 millisecc. The difference between C and E was due to a slight fluctuation in excitability, since the shock strength was not changed between making these two records. Scale, 15 mV.

(After HODGKIN (1938), *Proc. Roy. Soc. B.*, 126, 87.)

obtained with anodal pulses, apart from the reversed sign. As the cathodal pulse is increased in intensity above $0.5 \times$ threshold, the local potential response deviates more and more from the exponential pattern until, at threshold, the response bursts over into the propagated action potential or spike potential. By subtraction of the anodal local response, with sign changed, from the corresponding cathodal local response is obtained the temporal distribution of the excess of cathodal response over anodal (exponential) response (Fig. 9).

of the stimulating electrodes, Hodgkin, in 1938, using unmyelinated single nerve fibres of the crab [14], and Pumphrey, Schmitt and Young in 1940, using unmyelinated single nerve fibres of the squid [21], studied the initiation of the propagated action potential (Fig. 8).

As stated earlier, the response to a subthreshold stimulus spreads in exponential fashion both spatially and temporally. But this statement needs qualification. It is not valid for cathodal pulses of intensity greater than about $0.5 \times$ threshold. For cathodal pulses of intensity up to $0.5 \times$ threshold, the local potential response is identical with that obtained

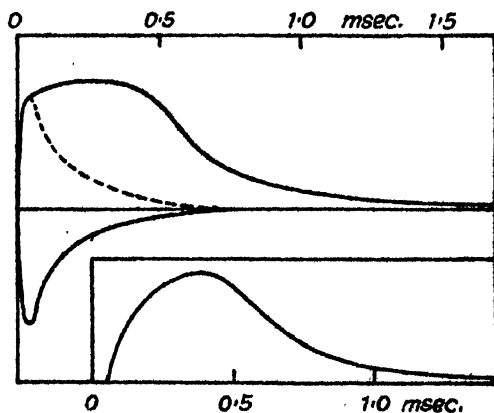


FIG. 9.—Cathodic and anodic potentials traced from Fig. 8, E and F. Cathodic potential analysed into polarisation potential and local response. Inset: local response obtained by subtraction of polarisation potential from total cathodic effect.

(After HODGKIN (1938), *Proc. Roy. Soc. B.*, 126, 87.)

This can be done for each intensity of stimulation and a family of curves obtained showing the development of the "local action potential" at the cathode (Fig. 10). In Hodgkin's experiments the time constant of the exponentially decrementing electrotonic potential was 0.29 millsec. The plot of the local response, at 0.29 millsec. after application of the stimulus pulse, against the

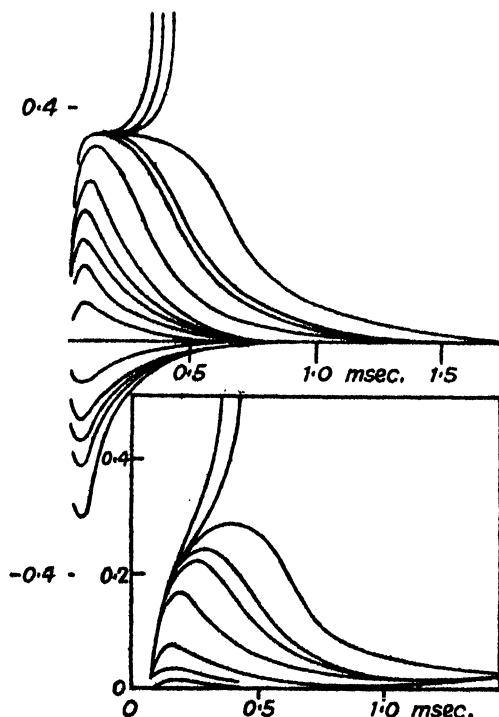


FIG. 10.—Family of curves showing cathodic and anodic potentials at stimulating electrode produced by stimuli with relative strengths from above 1.00 (upper 6 curves), 0.96, 0.85, 0.71, 0.57, 0.43, 0.21, — 0.21, — 0.43, — 0.57, — 0.71, — 1.00. The ordinate scale gives the potential as a fraction of the propagated spike, which was about 40 mV. in amplitude. *Inset*: local action potential produced by shocks with strengths successively from above 1.00 (upper 5 curves), 0.96, 0.85, 0.71, 0.57; obtained from above curves by subtraction of anodic changes from corresponding cathodic curves.

(After HODGKIN (1938), *Proc. Roy. Soc. B.*, 126, 87.)

strength of the pulse in threshold units, displays the rapid development of the local action potential at cathodal stimulus intensities over 0.5 $\times$ threshold (Fig. 11).

Variation in the latency and time course of the local action potential is found in different nerves. In partially refractory nerve the local action potential is diminished or entirely suppressed. That is to say, there exists a refractory period of local action potential.

Thus, it is practically certain that the local action potential is a physical process separate from the exponentially decrementing electrotonic potential. When sufficient axon surface is involved in the process the propagated action potential is reached, and maintains itself in the manner described earlier.

In a multi-fibre preparation of myelinated nerve (frog sciatic) Katz, in 1937 by indirect methods, and later by direct measurement [18, 19], reached conclusions similar to those of Hodgkin for unmyelinated nerve.

It follows from what was said earlier that the action potential is a wave of negativity travelling along the fibre. The classical

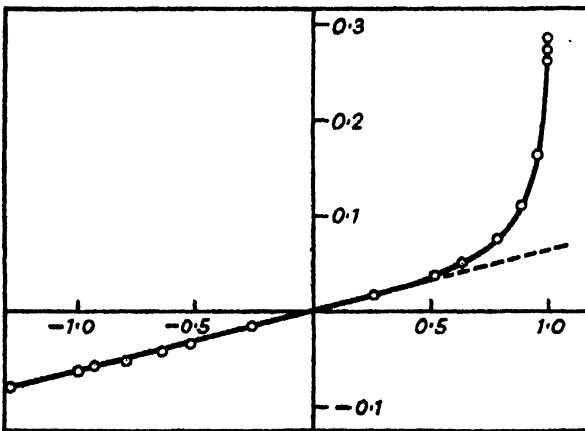


FIG. 11.—Showing the relation between potential at 0.29 millisecc. after application of shock. Ordinate, potential as fraction of spike potential. Abscissa, strength of shock as fraction of threshold.

(After HODGKIN (1938), *Proc. Roy. Soc. B.*, 126, 87.)

view has been that the action potential is a wave of depolarisation on the axon membrane, the electro-negativity, with respect to a remote reference point, being due to the temporary destruction of the membrane polarisation giving more or less direct access across the depolarised membrane to the axoplasm. The potential as measured under these conditions is the potential across the membrane underneath the distant reference electrode. This is similar to the explanation of the injury potential discussed earlier.

Recent work with giant nerve fibres, into the axons of which it has been possible to introduce micro-electrodes, has modified this considerably. Curtis and Cole in America and Hodgkin and Huxley in this country have shown [9, 16] that the membrane polarisation is reversed in sign and magnitude when a propagated action potential occurs (Fig. 12).

The explanation of these results in terms of nerve membrane structure calls for a greater knowledge than exists at present of the fine structure of cell membranes.

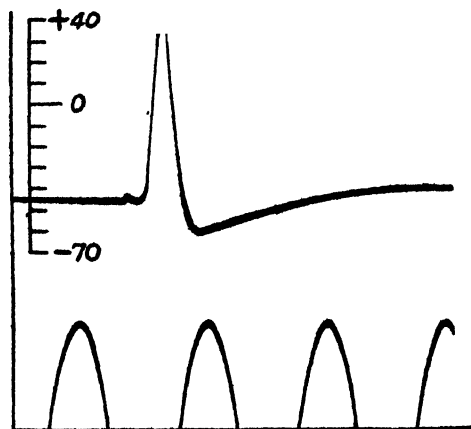


FIG. 12.—Action potential recorded between inside and outside of axon. Time marker 500 c.p.s.

The vertical scale indicates the potential of the internal electrode in millivolts, the sea water outside being taken as at zero potential. The small initial wave is the stimulus artifact.

(After HODGKIN & HUXLEY (1945), *J. Physiol.*, 104, 176.)

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SOME BASIC FEATURES OF CLOUD CHAMBER OPERATION

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1. INTRODUCTION

THE use of the cloud chamber is probably now so widespread that a short discussion of some of the basic features of cloud chamber operation may be of value. In selecting topics under this heading, three of those chosen—The Condensation Process, Supersaturation by Adiabatic Expansion, and Measurement and Identification of Particles—need little comment, but the inclusion of a further subject—The Controlled Chamber—requires some explanation. The justification of this choice is to be sought in achievements of the cloud chamber in the intensive concentration of effort during the last decade on nuclear problems of all kinds. Cloud chamber methods have been applied widely, but in the field which now appears likely to dominate fundamental physics for the next ten years—the study of the complex varieties of particle, short-lived and of mass intermediate between protons and electrons, the so-called “meson group”—they have been shown to have exceptional power of a value only approached by the new, but probably rather less flexible, technique of direct track images in a photographic emulsion. The cloud chamber must, for the majority of studies concerning the meson group, be counter controlled, and, with this in mind, we indicate briefly some of the points of control as they affect the direct operation of the cloud chamber.

2. CONDENSATION ON NUCLEI

2.1.—The main features of condensation in a supersaturated, dust-free gas are well known, and may be described in terms of three categories of condensation nuclei—uncharged nuclei always present, charged nuclei derived from ions formed in the gas, and re-evaporation nuclei, which are not permanently maintained, but which exist

in the gas for a considerable time after droplets, however formed, have evaporated.

Chamber practice requires the control in some degree of all three categories. The aim is to obtain condensation on charged nuclei so soon after their formation that they may be referred to identifiable particle trajectories, which in this way can be studied; old charged nuclei which have drifted far from their points of formation are of no interest, and, since they obscure the required distribution of droplets, they are excluded, perhaps by passing the ionising agent only for a short time when supersaturation is at the critical value, and always by maintaining an electrostatic "clearing field" to sweep old charged nuclei from the chamber. Uncharged nuclei cannot in general be removed, and condensation upon them is controlled by limiting the maximum supersaturation to a value at which only a negligible number of uncharged nuclei become effective. Contamination of the chamber gas, however, increases the number of uncharged nuclei which become effective at a particular supersaturation, and, since this increase is usually unacceptable, careful attention to purity of gas and vapour against certain contaminating materials, which are at present only empirically recognised, is necessary. It is never convenient to wait for re-evaporation nuclei to disappear naturally. They are removed in subsidiary "cleaning expansions" to a low supersaturation at which no new nuclei become effective and at each of which a fraction of the existing re-evaporation nuclei is removed by attachment to the walls of the chamber. The fraction removed increases as the number remaining becomes smaller and as each drop formed therefore tends to become larger; as a result, the cleaning process, which appears to have indifferent success at the outset, becomes increasingly effective, and a definite stage may easily be identified at which cleaning is complete.

2.2.—Condensation on permanent uncharged nuclei and on charged nuclei may be discussed in terms of the Thomson relation giving the vapour pressure which is in equilibrium with a droplet as a function of drop size. Writing R , θ , and M for the gas constant, the absolute temperature and the molecular weight of the vapour in which the drop is in equilibrium, σ , T , and a for the density, surface tension and radius of the drop, then p/p_0 , the ratio of the vapour pressure in equilibrium with the drop to that in equilibrium over a plane surface, is given by

$$\frac{R\theta\sigma}{M} \ln p/p_0 = 2T/a + dT/da - \left(\frac{1}{s_1^3} - \frac{1}{s_2^3} \right) \frac{e^2}{8\pi a^4}$$

where the last terms appear only if the drop is charged, when for our purpose e is the electronic charge, and where, following Tohmfer and Volmer [1], ϵ_1 , ϵ_2 are modified values of the dielectric constants of gas and condensed liquid. This equilibrium for charged and uncharged drops is shown schematically in Fig. 1. The variation of surface tension, dT/da , is as a rule ignored in the approximate considerations which are possible.

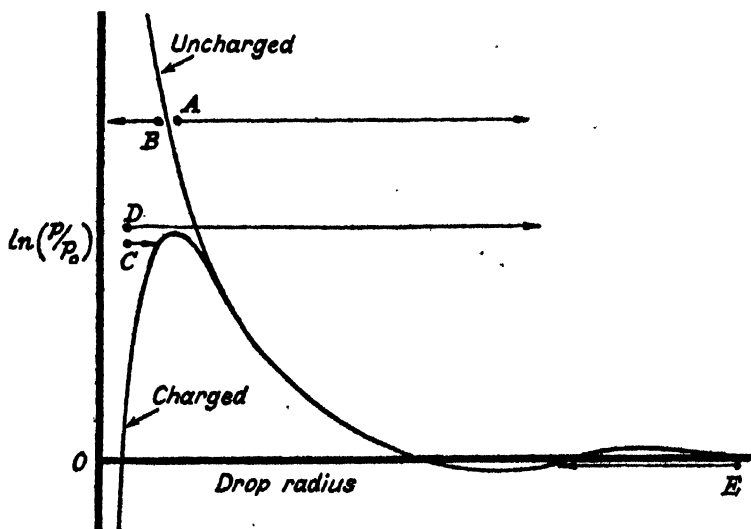


Fig. 1.—The vapour pressure equilibrium of small drops.

The variations indicated in the right-hand half of the graph represent the probable behaviour, but have not been more certainly established.

2.3. UNCHARGED NUCLEI.—We consider first the possibility of the development and growth of uncharged nuclei. It is clear that if a droplet exists at any instant in condition A, Fig. 1, it is in a supersaturated atmosphere and will tend to grow. This growth will only be stopped by external factors and in practical conditions will lead to a visible drop in the cloud chamber. Similarly, a drop in condition B is likely to evaporate. There is no mechanism leading to a general continuous growth of nuclei from zero size to a condition such as A from which growth to visible size will follow. The condition represented at A and B in the diagram, however, is that of a quite small aggregate of a few molecules (~ 10), and the theory of the occurrence of condensation on uncharged nuclei has been developed by Volmer and Weber [2] and by Farkas [3] on the assumption that condensation takes place on the very few drop nuclei, arising from statistical aggregates of vapour molecules, which

reach a size greater than that of the Thomson equilibrium. These workers give for the condition that one aggregate per cm.^3 per sec. will exceed the Thomson equilibrium at a supersaturation $(p/p_0)_{\text{crit.}}$ the relation

$$\ln (p/p_0)_{\text{crit.}} = K(T/\theta)^{3/2}M/\sigma$$

where K is a constant which is the same for condensation from differing vapours. Experiments by Volmer and Flood [4] show variations of $(p/p_0)_{\text{crit.}}$ from vapour to vapour in good agreement with this relation, and an extremely important extension by Flood [5] has shown that this agreement extends also to mixed vapours, and, in particular, to mixtures of the lower alcohols with water, for which the factor $T^{3/2}M/\sigma$ exhibits a minimum considerably lower than the value for either pure liquid. Since a small supersaturation is mechanically relatively easily made, such mixtures are of great practical value.

2.4. CHARGED NUCLEI.—We have seen that the development of an uncharged vapour aggregate to a visible drop is an event of great statistical rarity: the development of charged nuclei is essentially different. The equilibrium curve for these nuclei reaches a definite maximum value of p/p_0 , and if a condition of greater supersaturation exists (**D**, Fig. 1), all charged nuclei will grow indefinitely, while at lower supersaturations (**C**), growth will take place to a finite equilibrium size. When p/p_0 is brought to a value slightly below the maximum of the curve, however, statistical fluctuations lead to partial penetration to the domain of growth, and the onset of condensation on charged nuclei is therefore not abrupt. Moreover, many of the important vapours are polarised and in the liquid phase normally adopt definite configurations at the free surface. The simple equilibrium relation which has been quoted clearly takes no account of this phenomenon, and with such liquids a difference of supersaturation of onset for condensation on ions of differing sign is found. Under clean conditions, and with the normal vapours, complete condensation on ions of both signs precedes condensation on more than 1–10 nuclei/ cm.^3 of uncharged nuclei.

2.5.—Important recent studies of the condensation limits have been made by Scharrer [6] and Flood [5]. It is necessary to notice, however, that these workers adopt a standard of condensation on uncharged nuclei of the order 1 drop/ cm.^3 , and that the numerical values obtained are therefore not comparable with the “cloud limit” of C. T. R. Wilson and other early workers.

2.6. RE-EVAPORATION NUCLEI.—The low supersaturation at which re-evaporation nuclei become effective suggests that these are quite large, and this view is supported by the low rate at which

these nuclei even if charged are swept away in an electrostatic field. It is likely that even at large drop radii variations of surface tension with radius occur which give rise to equilibrium conditions for evaporating drops (E, Fig. 1), in the way shown schematically at the right-hand side of the diagram.

3. SUPERSATURATION IN ADIABATIC EXPANSION

3.1. TYPICAL DATA.—The supersaturation leading to condensation in the cloud chamber is invariably produced by adiabatic expansion of saturated gas. A typical example may be given for air saturated with water vapour ($\gamma = 1.40$), which requires an expansion in the volume ratio $v_2/v_1 = 1.30$ for condensation on ion nuclei. The temperature falls from (say) 10°C. to *about* -18°C. and the vapour pressure from 9.2 to 6.4 mm. of mercury. The saturation vapour pressure after expansion is about 1.0 mm. of mercury, and thus about sixfold supersaturation is obtained. For a given initial condition the supersaturation is determined by the fall of temperature, and hence a lower volume expansion ratio is required in a gas of high γ , since

$$(v_2/v_1)^{\gamma-1} = (\theta_1/\theta_2)$$

For example, an expansion ratio of 1.17 in argon is equivalent to 1.30 in air.

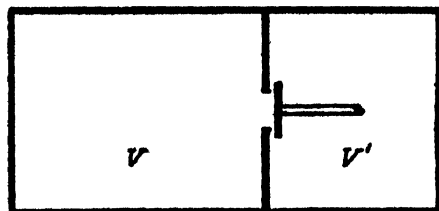
3.2.—While in normal practice the gas temperature falls considerably below zero, the temperature of a condensing drop which is the seat of liberation of heat is considerably higher. The conditions at which direct sublimation of ice takes place in a cloud chamber have recently been demonstrated by Cwilong [7].

3.3. VOLUME AND PRESSURE RELATIONS.—Although the feature governing the development of supersaturation is the change of temperature, this is not measured directly. We may distinguish a *volume-defined* apparatus in which the enclosed volume of the cloud chamber is increased by a mechanically defined amount and a *pressure-defined* apparatus in which the change of pressure, rather than of volume, is determined. The respective expansion ratios, α and β , are given by the relations

$$\left. \begin{aligned} \alpha &= v_2/v_1 = (\theta_1/\theta_2)^{1/\gamma-1} \\ \beta &= p_1/p_2 = (\theta_1/\theta_2)^{\gamma/\gamma-1} \\ \alpha^\gamma &= \beta \end{aligned} \right\}$$

The mechanism of a volume-defined expansion requires no comment ; that of a pressure-defined expansion is best considered in terms of

the connection of two vessels initially at different pressures (Fig. 2). The vessels V , V' initially at temperature θ_1 and pressures p_1 , p'_1 ,



Before expansion p_1, θ_1 p'_1, θ'_1
 After expansion p_2, θ_2 p'_2, θ'_2

FIG. 2.—General form of pressure-defined chamber.

are connected and come respectively to temperatures θ_2 , θ'_2 at the common pressure p_2 . Then,

$$\beta = p_1/p_2 = \frac{V/V' + 1}{V/V' + p'_1/p_1}$$

Two conditions are of importance

$$V/V' = 0, \quad \beta = p_1/p'_1 \quad (\text{expansion into free atmosphere})$$

$$\text{and } p'_1/p_1 \rightarrow 0, \quad \beta \rightarrow 1 + V'/V \quad (\text{expansion into a vacuum}).$$

The first is of particular simplicity and is the condition used in the prototype of *pressure-defined* chambers described in 1933 by C. T. R. Wilson [8]. The second has the advantage of being insensitive to variations of external pressure and of allowing a most delicate control of expansion ratio to be made by varying the residual gas pressure in the vacuum vessel into which expansion takes place.

Volume and pressure defined operation lead to distinctive differences of form of expansion arising from the difference of inertia of the moving systems and from the necessity of damping the gas motion in the pressure-defined version. The most rapid operation is probably reached in volume-defined chambers.

3.4. PERSISTENCE OF SUPERSATURATION.—Supersaturation produced in an adiabatic expansion is an unstable state. We shall consider the reduction of supersaturation which takes place in a chamber suddenly brought to a definite supersaturation, but it may be noted that the time scale arrived at also defines the order of speed of expansion which must be reached if the nominal supersaturation corresponding to the defined pressure or volume changes is substantially to be attained.

Three processes are important :

- (a) evaporation from free liquid surfaces in the chamber (which are not cooled at the expansion) ;
 - (b) heat conduction from the (also uncooled) walls of the chamber (we ignore here the late stage at which convection transfers become effective) ;
- and (c) heat conduction from, and vapour diffusion to, drops already condensed.

The very large effects of (a) in chambers where the partial pressure of the permanent gas is low have been pointed out by Powell [9]. At atmospheric pressure and greater, and at normal room temperature with the vapours in practical use, the effect is not of importance, but it does restrict the versatility of the cloud chamber for particular applications.

In normal chambers, (b) and (c) are the controlling factors which determine the persistence of supersaturation ; they have been studied in detail by Williams [10] and Hazen [11]. In particular, in a volume-defined chamber, the mechanism is very characteristic : gas in the immediate neighbourhood of the chamber wall or of a condensing drop is heated and expands ; in doing so the main bulk of gas in the chamber is compressed and heated. Except in the wall layer, the supersaturation falls equally throughout the chamber and it is possible to define a *sensitive time*—the time over which the chamber is receptive to new tracks. In the absence of condensation, Williams shows that the sensitive time, τ , is given by an expression of the form

$$\tau \sim \frac{\rho s}{k(\gamma - 1)^2} (V/S)^2 (\delta r/r)^2$$

where ρ , s , k , γ are respectively the density, specific heat at constant pressure, the thermal conductivity and the ratio of specific heats of the gas, V and S are the volume and surface area of the chamber, and $(1 + r + \delta r)$, $(1 + r)$ are respectively the volume expansion ratio suddenly established and that for which the minimum supersaturation for track condensation would be reached. The sensitive time increases with the size of the chamber and with the gas pressure ; it also increases as the excess of expansion ratio over the minimum necessary is increased. However, any large increase of δr brings into play another factor : condensation occurs and the third process (c) operates to reduce supersaturation, and we should expect some optimum expansion ratio at which the greatest sensitive time is attained (Fig. 3). Experimental work by Hazen has con-

firmed these conclusions; for a clean chamber the optimum value of δr is of the order 0.01.

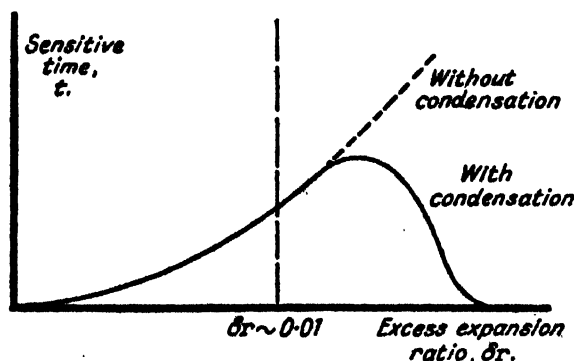


FIG. 3.—Variation of sensitive time with excess expansion ratio.

3.5. EXTENSION OF SENSITIVE TIME.—The conditions in a pressure-defined chamber are not so clear-cut as in the volume-defined chamber; in particular, there is not the same compression of the bulk of gas as the heated gas at the chamber walls expands. Moreover, the form of expansion in which the maximum of supersaturation is reached asymptotically favours a prolonged period of almost stationary supersaturation. Such chambers, broadly speaking, have greater sensitive time. Mechanism for continuing a slow expansion at a rate approximately compensating the normal fall of supersaturation has been described by Frisch, Beardon, Meyer-Leibnitz [12] and others, and leads to an artificially long sensitive time.

3.6.—The order of magnitude of sensitive time is indicated by the examples given by Williams. A chamber at atmospheric pressure 30 cm. dia. $\times$ 30 cm. deep had a sensitive time 0.4 sec., while a smaller chamber 20 cm. dia. $\times$ 20 cm. deep at 80 atmospheres pressure had a sensitive time of about 2 secs. It is necessary to notice that these sensitive times, as they stand, give no indication of the useful reception of the chamber except for the crudest purposes. For critical applications, as is discussed later (4, below), the effective time during which good material can be collected is very much shorter.

4. MEASUREMENTS AND IDENTIFICATION

4.1.—We summarise the kind of information which may be obtained from cloud chamber photographs.

The measurements in practice reduce to three kinds:

- (a) measurements of direction, of angles between tracks, and of curvature of tracks in a magnetic field ; from the latter, of the momentum of the track producing particles.
- (b) Measurements of ionisation in tracks, by drop counts of the track image ; in association with momentum measurements these lead to mass determinations and to the identification of categories of fast particles.
- (c) Establishment of association in time among particles producing tracks, described by the *resolving time* of the chamber.

4.2. GEOMETRICAL MEASUREMENTS.—The geometrical measurements are limited in precision by distorting factors of which some, for example the distortions of the photographic system, are systematic and can be removed, while others, associated with random gas motions in the chamber, are variable and constitute the practical limit of measurement. It is now recognised that the limiting distortions usually arise from steady convection drifts present in the chamber before expansion, and a policy of very careful temperature control over the whole chamber is accordingly followed, while care is also taken to ensure that the convection arising from the final compression of the chamber into the operating condition is allowed to die down before the chamber is made sensitive. It has come about that the highest degree of precision in chamber work has been demanded in the counter-controlled work on cosmic ray problems, and hence the necessity for a minimum of distortion also exists for the actual motion of the gas at expansion. It is found, however, that non-reproducible variations of motion at expansion, and of the gas convection which grows in the cooled gas after expansion, can readily be reduced below the limit set by steady convections. The velocity of the convection drift corresponding to reasonably good chamber conditions is of the order 1 cm./sec.

4.3. ION COUNTS—The classical studies of track ionisation refer to two distinct quantities : the *primary ionisation*, that is, the ion pairs resulting directly from interaction between the fast track-tracing particle and the molecules traversed, and a quantity which is more nearly the *total ionisation*, the total number of ions finally produced, whether directly by the primary particle or by secondary electrons or radiations arising from it. For application as a criterion of identification, the second is almost always adopted, for under convenient conditions of measurement it can usually lead to a statistically more accurate count.

The technique of counting the ions formed along a cloud track has proved to be of considerable difficulty and has been studied

critically by Hazen [13]. The most important technical feature is, according to him, the necessity of ensuring that 100 per cent. condensation has taken place. He draws the ions before expansion into separate columns of positive and negative sign, and gives criteria, for normal condensing vapours, of complete condensation on the more effective sign of ion in terms of the ratio of numbers of drops in the two columns. Except, perhaps, in the use of light gases, the exact composition of the chamber gas is relatively uncritical. It is further necessary to define precisely what is counted: rare fast secondary electrons which pass right out of the chamber can clearly not be followed, while the ionisation on much slower secondary electrons may be bunched until the resulting drop cluster is uncountable. A well-defined maximum size of cluster which is counted, which is preferably not rare in terms of the length of each track available for counting, leads to an unambiguous count. The technique of counting is eased if tracks are allowed to diffuse to some extent before condensation takes place and important studies have been made under these conditions. In much of the exploratory work which is familiar in cosmic ray research, simultaneous values of curvature and momentum are required on a single photograph, and while precision of curvature depends upon a minimum of delay before photography, the drop count is eased by the existence of a delay. The development of the optimum conditions for such work is a problem of the greatest importance.

4.4. RESOLVING TIME.—The resolving time of a cloud chamber compares unfavourably with that, for example, of a system of Geiger counters, but nevertheless is of importance when a rare true association of particles takes place with a frequency comparable with the random association in time of single particles of the same type.

For tracks entering the chamber after expansion, the resolving time is of the order of the sensitive time; for earlier tracks it is based on the detectable differences of diffusion breadth. We shall see (5, below) that the diffusion breadth is proportional to $t^{1/2}$, where t is the time between the formation of ions and the beginning of condensation upon them. If we assume that a fractional difference, f , of track breadth is just detectable, the resolving time, T , is given by

$$t \left(\frac{1}{f^2} - 1 \right)$$

Under good conditions, f may perhaps be as high as 0.7 and then T is about equal to t , between 0.01 and 0.02 sec. for a fast counter-controlled chamber. When photographic quality is low, a much

larger resolving time may easily be obtained. The resolving time is reduced linearly with the speed of expansion.

5. THE CONTROLLED CHAMBER

We shall discuss the central problem of chamber control—the way in which the precision of geometrical measurements depends on the speed of operation, the extra delay, that is to say, between the instant of passage of the ionising particles and the beginning of growth of drops on the ions defining particle trajectories. This feature is of the greatest importance, since it is largely in counter-controlled operation that the energetic particles for which chamber distortions set the limit of measurement are encountered. Apart from the time-sensitive factor, the possibility of distortion also clearly exists in the act of expansion. No systematic work has yet been carried out on the subject, but general experience indicates that in a shallow, unobstructed chamber distortions during expansion are negligible but that they may become serious in a chamber divided by plates into a series of relatively deep cells.

5.1. SPEED OF EXPANSION.—The classical study of the speed of expansion necessary to give reasonably fine tracks was made by Blackett [14]. He showed that, in air, tracks in which 90 per cent. of the drops lie in a projected breadth of about 1 mm. would be obtained if expansion were completed within about $\frac{1}{10}$ sec. of the passage of the ionising particle.

This is a much faster expansion than is necessary to show in a crude way what tracks are present: it represents the order of speed which may reasonably be sought with the normal chamber mechanism, and is thus based on such quantities as the velocity of efflux of compressed gas from an aperture, and the acceleration of a piston under pressure differences of the order of an atmosphere. Any large improvement in the speed of expansion demands a very great complication in the mechanism of expansion, and it is thus important to examine the way in which precision of measurement varies with speed of expansion in the neighbourhood of, and in particular below, the Blackett time of about $\frac{1}{10}$ sec.

Two effects are found to be of comparable importance: the distortion arising from slow convection in the chamber described above, and the intrinsic uncertainty with which the centre of diffusion of a diffuse track is defined by the relatively few drops available for the purpose at any point upon it. Provided that the most effective use of the material is made, and the track is defined by reference to at least a large fraction of the drops in it, it is

unlikely that the second factor has ever been the controlling one, but in measurements based on a minimum number of points, locally defined only, it may be allowed to become predominant. If we write t , the time between the passage of the triggering particles and the beginning of drop growth, and t_0 , the time of growth of the drop before it is photographed, convection distortion will develop as $t + t_0$, while uncertainty of location due to diffusion will increase as $t^{1/2}$. Now although t_0 is in principle at our disposal, and may be reduced by making available more light for photographing tracks, in practice there is little or no excess of illumination for this purpose and t_0 is of the order 0.02 sec. or perhaps even more. It follows that if convection distortion predominates at $t \sim 0.01$ sec. it will continue to do so at shorter times, and that in fact little advantage accrues when the expansion time t is made much less than the growth time t_0 .

5.2. DEAD TIME OF CHAMBER.—We have seen that the precision of measurement in a controlled chamber depends, under good working conditions, upon convection drift acting for a time, $t + t_0$, which cannot greatly be reduced. The convection is essentially that set up by uncontrolled variations of temperature in the chamber, and the assumption is that it varies little with time. Superposed upon this steady drift is comparatively violent convection which arises from the temperature changes set up during the cleaning cycle of the chamber and the final compression to operating conditions; throughout the period for which this induced convection is large compared with the steady drift, the chamber must be inert if large uncertainties of distortion are not to be introduced. This inert period, together with that actually used in cleaning and resetting the chamber, is the "dead time" of the apparatus and it sets the essential time scale of operation of the chamber. In a small chamber at atmospheric pressure (say 30 cm. dia. $\times$ 5 cm. deep) the dead time may be 2 to 4 minutes; in a large chamber (60 cm. dia. $\times$ 15 cm. deep) it may be from 10 to 15 minutes, while in even a small chamber at high pressure it will be still longer.

The dead time sets the time scale of controlled operation in the following sense: if we put τ_0 , the dead time, and $\bar{\tau}$ the mean waiting time, when the chamber is made operative until it is fired by a control pulse, the rate of photography is not greatly raised if $\bar{\tau}$ is made much smaller than τ_0 , while, on the other hand, if $\bar{\tau}$ is several times τ_0 , the total receptive time of the apparatus cannot be much increased by any further lengthening of $\bar{\tau}$. Thus under almost all conditions of control, whether the desired event is frequent or rare, whether spurious operation is or is not important the dead

time gives to order of magnitude the correct rate of operation. An apparent exception happens when an intrinsically rare event is selected with certainty, and τ is then great (say $\gg 10\tau_0$). In this case, however, some alternative selection may be used to carry out simultaneously some other experiment, the material of each experiment being identified by means of indicator lamp markings or otherwise, and provided that the rate of operation is still satisfactory ($\tau \sim 10\tau_0$) the efficiency with which each experiment is carried out will be substantially that with which it could have been carried out independently.

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THE USE OF ISOTOPES IN BIOLOGY

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ISOTOPES, both stable and radioactive, have been used widely during the past ten or twelve years to gain insight into biological processes hitherto not well understood. The results obtained are very extensive and it cannot be hoped to give even a brief summary of all the achievements in the various fields of biological sciences in a short article; only some of the most outstanding investigations will be selected with the object of illustrating the results obtained and of giving a broad outline of the methods and possibilities of the isotope technique.

The principle of the use of isotopes in biological sciences is the preparation of labelled atoms of elements which can be easily distinguished from the naturally occurring atoms of the same element. If such labelled atoms are in turn introduced in stable chemical positions into molecules, these molecules, or certain chemical groups in which they are combined, become labelled also.

After the constituents of food—proteins, fats and carbohydrates—have been digested and absorbed, they merge with similar compounds of the body and their fate, therefore, cannot be followed any further. The desire to label substances of biological importance in order to follow their metabolic pathways is an old one. Before isotopes became available the commonest labelling procedure was the introduction of halogen elements (e.g. I or Br) into unsaturated organic substances, or the use of benzene derivatives. Some even attempted to follow the paths of fats in the body by feeding animals with fats stained with dyes. Such labels, however, usually altered the physical, chemical and physiological properties of the substances in question and therefore their use in metabolic studies was limited.

The discovery and separation of the natural stable isotopes of biologically important elements, H, C, N, O and S, and later the artificial production of the radioactive isotopes, provided the ideal tools for labelling biological substances.

SOME ELEMENTARY PRINCIPLES OF NUCLEAR PHYSICS.—According to present-day conceptions atoms consist of three fundamental particles : (a) the proton, which has a mass of approximately one on the atomic weight scale and carries one positive charge ; (b) the neutron, the mass of which is almost equal to that of the proton but which is uncharged ; and (c) the electron (negatron), which is the carrier of one unit of negative charge, with a mass of only $\frac{1}{1836}$ of that of the proton. It follows that the mass of the atom is concentrated in the nucleus, which consists of protons and neutrons, and that the atomic weight (*A*) of any element in round figures is given by the sum of the protons and neutrons in the nucleus. The electrons revolve around the nucleus in various orbital shells which have—on an atomic scale—very large radii. The number of electrons in an electrically neutral atom is equal to the number of protons in the nucleus and determines the chemical properties of the atom. The number of protons in the nucleus is called the atomic number (*Z*). The number of neutrons in the nucleus has no effect on the chemical properties of the atom, but affects its physical characteristics (mass, diffusion constant, stability of nucleus).

These facts imply that atoms which have the same number of protons in their nuclei, but different number of neutrons, are chemically identical, although they differ in their atomic weight ; such atoms are termed isotopes.

The simplest atom is that of hydrogen which consists of one proton and one electron ($Z = 1$; $A = 1$). Using the accepted notation, the symbol for this hydrogen is ${}_1\text{H}^1$, where the left-hand subscript denotes the atomic number and the upper index the atomic weight. The addition of a neutron to the nucleus of the hydrogen atom will not change its chemical properties ; the new atom will still behave like hydrogen, but its atomic weight will be two (${}_1\text{H}^2$). Such hydrogen exists in nature and is the stable heavy isotope of hydrogen (deuterium, D). If a further neutron be added, one obtains ${}_1\text{H}^3$, tritium, which does not occur naturally but can be produced artificially and is radioactive. Water formed with these three hydrogen isotopes will have molecular weights of 18, 20 and 22 respectively. The proportion of stable isotopes in a sample can be measured in the mass spectrometer with a high degree of precision.

The radioactive isotopes are further distinguished by the emission of various forms of radiations. Radioactivity, both natural and artificial, is a property of the atomic nucleus and depends on the configuration of the nuclear particles. The binding forces between protons and neutrons are not fully understood, but apparently only certain proton-neutron combinations are stable. An excess of

either nuclear particle relative to what is required for stability leads to a redistribution of protons and neutrons in the nucleus until a stable configuration obtains. This is accomplished by the emission of various forms of radiations— α -, β -, and γ -rays. The α -rays are positively charged helium atoms (${}_2\text{He}^4$), i.e. they are helium nuclei without the electrons. The β -rays may be either positively charged electrons (positrons, or positive β -rays) or negatively charged electrons (negatrons, or negative β -rays). The γ -rays are electromagnetic waves of short wave length (or quantum energies). The emission of α -rays is confined almost entirely to heavy elements above atomic number 82. When an α -particle leaves the nucleus of an atom, the atomic weight of which is A and atomic number Z , a new element is formed with an atomic weight of $(A - 4)$ and atomic number $(Z - 2)$. The artificially produced radioactive isotopes of the lighter elements (below atomic number 82) emit negative or positive β -rays, either of which may or may not be associated with one or more γ -rays. When a negative β -ray is emitted from the nucleus, a neutron changes into a proton and thus a new element is formed with an atomic number $(Z + 1)$, but the atomic weight remains A . For example radioactive phosphorus, ${}_{15}\text{P}^{32}$, disintegrates by the emission of a negative β -ray and changes thereby to an element with atomic number 16 and atomic weight 32, which is stable sulphur, ${}_{16}\text{S}^{32}$. The reaction may be written : ${}_{15}\text{P}^{32} \rightarrow {}_{16}\text{S}^{32} + \beta^-$. The ejection of a positron from the nucleus on the other hand means the change of a proton into a neutron and thus the resulting element will have an atomic number $(Z - 1)$ and the atomic weight, A , remains unchanged. For example, the short-lived radioactive isotope of carbon, ${}_6\text{C}^{11}$, decays by the emission of a positron and thus changes into stable boron. This reaction may be written : ${}_6\text{C}^{11} \rightarrow {}_5\text{B}^{11} + \beta^+$. The emission of γ -rays means loss of energy and its mass-equivalent from the nucleus.

The rate of nuclear disintegration and the energies of the radiations emitted depend on the nuclear configuration of protons and neutrons and are characteristic for each radioactive element. Radioactive decay is a random event and obeys statistical laws ; it follows an exponential course and is measured conveniently by the half-life of the element. If the number of radioactive atoms in a sample initially is N_0 , then after a time, t , the number of radioactive atoms left, N , is given by the equation

$$N_t = N_0 e^{-\lambda t}$$

where e is the base of the natural logarithm and λ the disintegration constant characteristic for each element. After one half-life

$$N_t = \frac{1}{2} N_0$$

Readers interested in the production of radioactive isotopes and their assay are referred to the book of M. D. Kamen recently published (1947).

GENERAL REMARKS CONCERNING THE BIOLOGICAL APPLICATION OF ISOTOPES.—It is of the greatest importance, from the point of view of biological applications, that the isotopes of the same element, whether stable or radioactive, behave chemically alike, or very nearly so, not only in the test tube but also in the living organism. The proportions of the naturally occurring stable isotopes, except of those resulting from decay of naturally radioactive elements (*e.g.* lead), are the same all over the world. The ratios for a few naturally occurring stable isotopes together with the half-lives of some of the biologically useful radioactive elements are listed in Table I. That living organisms cannot differentiate between the light and heavy isotopes of the same element is shown by the fact that the abundance ratios, for example, of light and heavy hydrogen, are the same in organic compounds as in natural waters, *i.e.* 99.98 per cent. light hydrogen and 0.02 per cent. heavy hydrogen; or that the abundance of the two stable isotopes of nitrogen in proteins and amino-acids obtained from animals is the same as in the nitrogen of air (Schoenheimer and Rittenberg, 1939). If, therefore a substance, *e.g.* a fatty acid, in which 10 per cent. (instead of the natural 0.02 per cent.) of all the hydrogen atoms are of the heavy variety (deuterium), is introduced into a biological system, this substance will be treated exactly like its natural analogue, but it will be possible to distinguish it from other fatty acids of the same kind by its "label," *i.e.* its unusually high deuterium content. If such a fatty acid is burnt in a stream of oxygen to carbon dioxide and water, the water of combustion can be collected and will contain heavy hydrogen in the same proportion as it was present in the organic molecule. There are several methods available to measure the deuterium content of water with great accuracy.

When a suitable radioactive isotope, as well as the rare natural isotope, of an element is available, "labelling" may be achieved by either of them. For example, compounds containing carbon can be labelled either by the artificially produced radioactive carbon of mass 14 (${}^{14}_6\text{C}$) or by the enrichment of the samples in respect of the rare stable isotope of carbon of mass 13 (${}^{13}_6\text{C}$). The label in the first case will be the amount of radiation emitted by the sample (or the specific activity, which is the amount of radiation per unit weight of element), and in the second case—as in the previous example with deuterium—the high concentration of the rare isotope.

TABLE I

STABLE AND RADIOACTIVE ISOTOPES OF BIOLOGICALLY IMPORTANT ELEMENTS

Atomic Number.	Element.	Atomic Weight.	Natural Abundance (%).	Half-life.
1	{ Hydrogen	1	99.98	25 yrs.
	{ Deuterium	2	0.02	
	{ Tritium	3	—	
6	{ Carbon	11	—	20.5 mins.
	{ " "	12	98.9	
	{ " "	13	1.1	
	{ " "	14	—	
7	{ Nitrogen	14	99.62	4 - 8 × 10 ³ yrs.
	{ " "	15	0.38	
8	{ Oxygen	16	99.757	
	{ " "	17	0.039	
	{ " "	18	0.204	
11	{ Sodium	22	—	3 yrs.
	{ " "	23	100.0	14.8 hrs.
	{ " "	24	—	
15	{ Phosphorus	31	100.0	14.3 days.
	{ " "	32	—	
16	{ Sulphur	32	95.10	87.1 days.
	{ " "	33	0.74	
	{ " "	34	4.20	
	{ " "	35	—	
	{ " "	36	0.016	
17	{ Chlorine	35	75.4	10 ³ yrs.
	{ " "	36	—	
	{ " "	37	24.6	
19	{ Potassium	39	93.38	Naturally radioactive 4 × 10 ³ yrs.
	{ " "	40	0.012	
	{ " "	41	6.61	
	{ " "	42	—	
	{ " "	43	—	
20	{ Calcium	40	96.96	180 days.
	{ " "	42	0.64	
	{ " "	43	0.15	
	{ " "	44	2.06	
	{ " "	45	—	
	{ " "	46	0.0033	
	{ " "	47	—	
	{ " "	48	0.19	
26	{ Iron	54	6.04	4 yrs.
	{ " "	55	—	
	{ " "	56	91.57	
	{ " "	57	2.11	
	{ " "	58	0.28	
	{ " "	59	—	
				47 days.

The separation of practically all the biologically important stable isotopes is due largely to Urey and his collaborators. The discovery of artificial radioactivity by Irene and Frederic Joliot-Curie, and the subsequent production of radioactive isotopes in large cyclotrons and the uranium pile, provided biological workers with further valuable tools. Problems which could not be investigated before have become more accessible to the experimenter.

Historically, Hevesy in 1923 was the first to use isotopes for biological research. He labelled common (non-radioactive) lead nitrate with minute quantities of the nitrate of thorium B, which is a naturally occurring radioactive isotope of lead (Pb^{212}) and studied the uptake of lead by the roots, stalk and leaves of *Vicia faba*. In his studies of absorption and excretion of lead and bismuth in rabbits Hevesy (1926) used another naturally radioactive lead-isotope Ra D (Pb^{210}) and the radioactive isotope of Bi, Ra E (Bi^{210}) as labelling agents. After the discovery of deuterium by Urey, Brickwedde and Murphy in 1932, it was again Hevesy, with the collaboration of Hofer, who in 1934 employed heavy water to study water metabolism in man. They found that the excretion of heavy water rises rapidly up to about 2 hours after drinking, but is slowed down afterwards to a steady rate indicating that the drinking-water gets evenly distributed throughout the body. They calculated that the total volume of water in a 69-kg. man is 43 litres and that the average time a water molecule spends in the human body is 14 days.

These earliest experiments already indicated the great possibilities of the isotope technique in metabolic studies, but the really epoch-making announcement came from Columbia University by Schoenheimer and Rittenberg in 1935 (1935a), that they had "prepared several physiological compounds (fatty acids and sterol derivatives) containing one or more deuterium atoms linked to carbon, as in methyl or methylene groups." The physiological properties of these substances were indistinguishable from those of their naturally occurring analogues, but because of their deuterium content, their fate could be followed in the body even after considerable dilution.

A series of articles was published after this announcement, mainly in the *Journal of Biological Chemistry*, in which the principles of the use of deuterium as a labelling agent were laid down. The results of these investigations led to the concept of the "dynamic state of body constituents" (Schoenheimer, 1941). In health the body constituents show a constant composition, but it has been shown by the Schoenheimer school and by others, chiefly with the aid of

the isotope technique, that these constituents are really in a state of flux: breakdown and synthesis occur side by side, the two processes being so carefully balanced that the composition of the body in health remains constant. Much of this type of work was done with the aid of deuterium and heavy nitrogen of mass 15 (${}^{15}_7\text{N}$) by the Columbia workers.

INVESTIGATION OF FAT METABOLISM WITH THE AID OF ISOTOPICALLY LABELLED COMPOUNDS

Most of the body fats are deposited in the so-called adipose tissue under the skin, in the abdominal cavity and between muscles. This fat has been called the depot-fat or reserve fat, because it was thought that the body calls upon it only in need; the adipose tissue has generally been regarded as rather inert metabolically, although it was known that its composition could be altered by feeding large amounts of unusual fats—small amounts having no effect. It was, therefore, thought that fats ingested in small amounts were directly used for caloric requirements and were deposited in the adipose tissue only when the diet contained fats in excess of the immediate needs. It was, therefore, quite surprising to find that after deuterium-labelled fatty acids had been fed to mice and rats for 4 days (Schoenheimer and Rittenberg, 1935*b*; Stetten and Schoenheimer, 1940), 44 to 47 per cent. of the ingested fats were deposited in the adipose tissue even when the diets contained as little as 1–4 per cent. of this dietary constituent. The body water of these animals contained deuterium in a concentration which indicated that 20 per cent. of the dietary fats had been burnt during the experimental period. The internal organs contained only very little of the ingested fatty acids. The animals during these experiments lost no weight and therefore it had to be concluded that they had burnt an amount of depot-fat equal to that which had been deposited from the diet. This clearly showed that the fats burnt were not merely dietary fats but a mixture of about equal parts of adipose tissue fat and food fat. These results dispelled the idea of the inertness of depot-fat and showed that the fatty tissues are constantly involved in very active metabolic processes.

The diversity of biochemical reactions in which fatty acids are involved in animals was demonstrated further by Schoenheimer and his collaborators in experiments in which the fate of individual fatty acids was followed. Stetten and Schoenheimer (1940) fed the ethyl ester of deuterium-labelled palmitic acid to rats in a diet which contained also 6 per cent. butter. After a suitable time the

animals were killed and individual fatty acids isolated from the adipose tissue and their isotope content determined. Palmitic acid had the highest deuterium content which indicated the direct utilisation of the dietary palmitic acid in the formation of triglycerides in the fat depots. Further, stearic, myristic and lauric acids, palmitoleic and oleic acids all contained deuterium in concentrations higher than were found in the body water. This showed that these fatty acids were all derived directly from the administered palmitic acid—stearic acid by the condensation of palmitic acid with another compound to increase the carbon chain by 2; myristic and lauric acids by the degradation of the palmitic acid chain by 2 and 4 carbon atoms; and palmitoleic acid by the desaturation of palmitic acid. The isotopically labelled oleic acid was probably formed by the desaturation of the newly formed labelled stearic acid. It is significant that all these reactions took place in normal animals, which received in their diet an adequate supply of all the mentioned fatty acids in the form of butter. It has been shown (Schoenheimer and Rittenberg, 1938) that the conversion of stearic acid into palmitic acid also occurs in animals. That saturation and desaturation of fatty acids is a physiologically reversible process in animals had previously been demonstrated by Schoenheimer and Rittenberg (1936*a*), and Rittenberg and Schoenheimer (1937*a*).

In general terms all these experiments have clearly demonstrated that fatty acids are constantly being converted into one another and that all these conversions are biologically reversible. The only exceptions to this rule are the two highly unsaturated fatty acids linoleic and linolenic acids, which are not formed in animals.

The experiments quoted may be taken as illustrative of investigations in which the fate of an isotopically labelled substance is followed in the animal (*a*) by its deposition at certain sites and (*b*) the appearance of the "label" in high concentrations in other substances, indicating the biological conversion of one substance into another. Some further examples of this type of experiment will be given later.

When animals are given heavy water to drink, this will be evenly distributed throughout the body and therefore all chemical reactions will take place in a medium containing heavy water. Whenever a reaction involving the hydrogen of body water takes place, deuterium will be built into the newly formed molecules in the same ratio as it is present in the body. The only restriction from the point of view of experimental design is that the concentration of D_2O in the body water must not exceed 10 per cent., because biological reactions do not proceed at normal rates

at higher D_2O concentrations. If the concentration of heavy water in the body is kept constant (usually at 1-2 per cent. level), then it is possible to calculate the rate of synthesis of fatty acids (and of other substances also) from the rate at which deuterium appears in molecules in a stably bound form. Conversely, by measuring the rate at which the "label" disappears from the fats one can infer the rate of their destruction. Schoenheimer and Rittenberg (1936b) have carried out both experiments and have shown that the rate of synthesis of fatty acids is the same as the rate of their destruction (or utilisation) in animals which maintained their weight. In mice, for example, 63 per cent. of all the body fat was renewed in 3 days when the animals were kept on a wholemeal bread diet. Synthesis of fatty acids by the animal proceeded whether fat was supplied in the diet or not.

Rittenberg and Schoenheimer (1937b) found that deuterium was built into the saturated fatty acids faster than into the unsaturated acids, which meant that the former were synthesised more rapidly than the latter. The concentration of the isotope in the saturated fatty acids, when a state of equilibrium has been reached, was one-half of the concentration of deuterium in the body water. Two unsaturated fatty acids, linoleic and linolenic acid, contained no isotope at all when the animals were given heavy water to drink. It was found further by Stetten and Schoenheimer (1940) that linoleic acid contained likewise no isotope after labelled palmitic acid had been fed to rats. These results can only mean that animals are unable to synthesise these two highly unsaturated fatty acids either from smaller molecules or from the conversion of other fatty acids. The findings gain significance in view of the observation of Burr and Burr (1929, 1930), Burr, Burr and Miller (1932) and others that rats develop a deficiency disease when deprived of linoleic and linolenic acids, which appear, therefore, to be essential dietary constituents.

The regeneration (synthesis and destruction) of saturated fatty acids was found to be much faster in the liver than in adipose tissue—more than one-half of the saturated acids being renewed in 24 hours in the liver of the normal animal on a low-fat diet (Bernhard and Schoenheimer, 1940). This and other investigations give the liver a unique role in fat metabolism. It is doubtful whether regeneration of fats takes place actually in the fat-depots as the previously summarised results might suggest. It seems more probable that these complex reactions take place in the liver and that there is a steady state of flow of fatty acids from the liver to the adipose tissues and back.

STUDY OF CHOLESTEROL METABOLISM WITH ISOTOPES

Cholesterol is closely associated with fat metabolism, although its exact role is not known. Rittenberg and Schoenheimer (1937b) found that, when the body water of animals was enriched with D_2O , stably bound deuterium appeared not only in fatty acids but also in cholesterol. From the rate of uptake of deuterium into cholesterol it was calculated that approximately 50 per cent. of the body cholesterol in mice was regenerated in 3 weeks. This is not a surprising finding since it had been previously established in "balance sheet" experiments that cholesterol is synthesised and broken down in the animal body. But the isotope technique provided further evidence as to the mechanism of this biological synthesis. When a state of equilibrium had been attained, i.e. the deuterium content did not increase any further, the concentration of the isotope in the cholesterol molecule was one-half that in the body water. The saturated fatty acids of adipose tissue showed the same behaviour. This means that during the synthesis of both cholesterol and fatty acids, one half of all the hydrogen atoms are derived from body water, which in turn suggests that both fatty acids and cholesterol are built up by the coupling of small, possibly 2-carbon atom molecules. In later experiments Bloch and Rittenberg (1942) found that feeding deuterium-labelled acetic acid (CDH_3COOH or $CD_3H.COOH$) to mice and rats led to the formation of deuterio-cholesterol. Both the side chain and the cyclopentenophenanthrene nucleus of the cholesterol contained the isotope indicating that the entire cholesterol molecule was synthesised from acetic acid or from an active derivative of it.

When acetic acid labelled in the methyl group with deuterium and in the carboxyl group with C^{13} ($CD_3H.C^{13}OOH$) was fed to mice, both isotopes appeared in the cholesterol molecule and fatty acids, demonstrating further that both carbon atoms of acetic acid are utilised in cholesterol and fatty acid synthesis (Rittenberg and Bloch, 1945). It was shown by Bloch, Borek and Rittenberg (1946) that synthesis of cholesterol and fatty acids from doubly labelled acetic acid occurs also *in vitro* in surviving liver slices.

Structurally cholesterol is closely related to the bile acids and steroid hormones (sex hormones and the hormones of the adrenal cortex), and it had been suggested that cholesterol might be a precursor to some of these. Definite evidence of conversion of cholesterol to any of these substances was, however, lacking before the application of the isotope technique. Bloch, Berg and Rittenberg (1943) have given intravenously to a dog an emulsion of cholesterol

labelled with deuterium (1 g. of cholesterol per day for 3 days), an anastomosis between the gall-bladder and kidney pelvis having previously been made, thus allowing the bile to be excreted with the urine. After 3 days it was found that the cholic acid extracted from the urine had an isotope concentration of the same order of magnitude as the cholesterol in blood or bile, demonstrating the direct conversion of cholesterol to the bile acid. In another investigation Bloch (1945) administered deuterium-labelled cholesterol to a woman in the eighth month of pregnancy. Pregnanediol containing deuterium in high concentrations was excreted in the urine. Pregnanediol is known to be the metabolic end product of progesterone and thus it can be inferred that the deuterium-labelled pregnanediol was derived from labelled progesterone, the only source of which could be the ingested labelled cholesterol. With the aid of the isotope technique it has thus become possible to assign a definite metabolic role to cholesterol.

The experiments quoted indicate how the isotope technique has been applied to demonstrate (a) the direct biological conversion of one substance into another and (b) the occurrence of certain biochemical reactions (e.g. synthesis of a complex molecule like that of cholesterol from acetic acid). Acetic acid or acetate appears to be a metabolite involved in a number of important biochemical reactions: for instance, it is formed during oxidation of fatty acids and is then condensed to acetoacetate (Schwendseid, Barnes, Hemingway and Nier, 1942; Weinhouse, Medes and Floyd, 1944); it is also utilised in the formation of cholesterol and fatty acids (see above), and in the acetylation of foreign amino-acids or amines (Bernhard, 1940; Bloch and Rittenberg, 1944, 1945). It is not surprising then that Buchanan *et al.* (1943) found that acetic acid fed to animals is very rapidly metabolised. Krebs and Johnson (1937) have shown that acetic acid is formed in tissue slices from pyruvate, but there was no evidence as to its formation in normal animals. It seemed reasonable to assume that large amounts of acetic acid may be produced in the intact animal, but because of its rapid utilisation in a number of reactions "its concentration in animal tissues at any instant may be too small to permit detection . . ." (Bloch and Rittenberg, 1945). Bloch and Rittenberg have shown that acetic acid is the only acetylating agent for *p*-amino-benzoic acid and sulphanilamide. When labelled acetate is fed to animals and the product of a reaction—in which acetate is the sole precursor—is isolated, the concentration of the isotope in the final product will be a measure of the degree of dilution of the dietary acetate with the endogenously formed acetate. This ingenious

argument was proposed and applied by Bloch and Rittenberg to measure the amount of acetate formed per day in the normal rat. They fed rats with acetate labelled with deuterium and heavy carbon ($\text{CDH}_2\text{C}^{13}\text{OOH}$), and *p*-aminobenzenesulphonamide or *p*-aminobenzoic acid. Acetylsulphanilamide or acetyl-*p*-aminobenzoic acid were then isolated from the urine. While the concentration of D and C^{13} in the acetyl-group of the acetylated aromatic amines was lower than in the administered acetate (indicating dilution by endogenous acetate), the ratio of D/ C^{13} was the same as in the dietary acetate. This indicated that the whole molecule of acetate was used in the acetylation reaction. From the degree of dilution of the exogenous acetate they calculated that 15–20 millimols (0.9–1.2 g.) of acetate per 100 g. rat must have been formed per day. It is probable that this relatively large amount of acetate is derived from the oxidation of fatty acids. From the rates of synthesis and breakdown of fatty acids already determined by Schoenheimer and Rittenberg it could be calculated that an adult rat of 300 g. weight utilises approximately 3 g. of fatty acids per day, which when oxidised could give rise to 3 g. of acetate per day. Thus the two sets of experiments are in good agreement.

The mechanism of fatty acid oxidation by the process of “ β -oxidation-condensation” has been succinctly summarised in a previous number of this journal by Professor F. G. Young (SCIENCE PROGRESS, 1948, 36, 13–37). The evidence in favour of this process was obtained by Medes, Weinhouse and Floyd (1944) by the use of octanoate labelled with C^{13} .

INVESTIGATION OF PROTEIN AND AMINO-ACID METABOLISM WITH ISOTOPES

Deuterium and heavy nitrogen (N^{15}) have in recent years been employed with great success in the study of protein metabolism. Because of the great complexity of body proteins only limited information concerning their metabolism could be obtained with previous methods, and here the isotope technique proved particularly fruitful. This subject has recently been reviewed by Rittenberg and Shemin (1946) and therefore only selected examples of the use of the heavy isotope of nitrogen (N^{15}) will be given. In broad principle the “dynamic state of body constituents” was found equally applicable to the body proteins and many of the experiments, in which amino-acids labelled with N^{15} were fed to animals in nitrogen equilibrium, yielded results almost analogous to those obtained with labelled fatty acids.

There appear, however, to be some special exceptions to this principle of continuous regeneration of body constituents. Shemin and Rittenberg (1945, 1946*a* and *b*) showed recently that the amino-acid glycine is the nitrogenous precursor of the protoporphyrin of human and rat hæmoglobin, other amino-acids having been excluded as direct nitrogen donors. This investigation, apart from showing the utilisation of glycine for pyrrole synthesis, has yielded probably the most accurate value for the life of the human red blood corpuscle. One of the authors took by mouth 66 g. of glycine labelled with N^{15} in the course of 3 days. The concentration of N^{15} in the hæmin prepared from the red corpuscles first rose steeply, reaching the maximum value about 25 days after the labelled glycine was consumed, then remained practically constant for the next 70 days and then fell along an S-shaped curve (Fig. 1). The shape of this

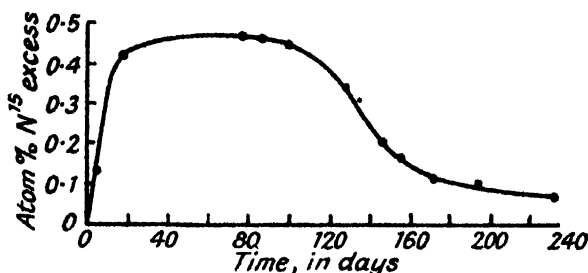


FIG. 1.— N^{15} concentration in hæmin after feeding N^{15} labelled glycine for 3 days.

(From SHEMIN and RITTENBERG, 1946, *J. biol. Chem.*, **166**, 627; part of Fig. 1, redrawn.)

curve is totally different from that found for cellular constituents which continuously regenerate. It is usual to find that the concentration of the isotope in such substances rises during the feeding of its labelled precursor and declines immediately thereafter along an exponential curve. "The curve for hemin cannot be described by a simple exponential function; it appears that the probability that a hemoglobin molecule may be degraded is a function of its age. Since the concept of 'ageing' is not applicable to a molecule, another explanation must be sought. If a constituent of a cell is not involved in the flux of synthesis and degradation, the presence of N^{15} in the constituent of this cell must be the result of synthesis of the component and its incorporation during the formation of the cell. The original molecules of this constituent will then remain with the cell until the cell disintegrates or dies" (Shemin and Rittenberg, 1946*b*, p. 629). The shape of the curve for the N^{15} content of hæmin is entirely compatible with this conception and it shows further that after the death of the red cell the porphyrin

moiety of hæmoglobin, unlike the iron, is not used again. If reutilisation occurred, a secondary rise should follow the decline of the curve. From the shape of the curve for hæmin, and from certain mathematical considerations, the average life of the human red cell was calculated to be 127 days, a value given by the distance between the midpoints of the ascending and descending parts of the curve.

It appears, therefore, that the hæmoglobin molecule does not undergo molecular regeneration once it has been formed, which is a unique exception among the body constituents. It has been shown, with the aid of N^{15} -labelled amino-acids, that even specific antibody proteins are subject to a continuous exchange of their amino-acids, not only during immunisation while the antibody titre in the serum rises, but also during the decline of the titre (Schoenheimer and Heidelberger, cited by Schoenheimer, 1941).

SOME APPLICATIONS OF RADIOACTIVE ISOTOPES

THE CHOICE BETWEEN STABLE AND RADIOACTIVE ISOTOPES.—

The choice between stable and radioactive isotopes for a particular investigation depends on a number of factors: (a) availability, (b) half-life of the radioactive isotope, (c) degree of dilution of the isotopically labelled substance expected in the biological system, (d) technical problems of assay.

For elements which have only one naturally occurring stable isotope, like phosphorus, $^{31}_{15}P$, the only choice for tracer work is a radioactive isotope. Phosphorus has several radioactive isotopes, but of these only one, $^{32}_{15}P$, is suitable for biological research. Its half-life is 14.3 days and the β -rays emitted are highly energetic and hence easy to measure. The other known artificial radioactive isotopes of phosphorus (P^{29} , P^{30} , P^{34}) have half-lives ranging from a few seconds to a few minutes, much too short to be useful. Generally the radioactive assay, to yield reliable information, has to be carried out before 6 to 10 half-lives have elapsed. Of course, this interval may have to be shorter if weak samples are available to start with. For elements like carbon the choice of a labelling agent lies between the rare natural stable isotope, $^{13}_6C$, and the artificial radioactive isotopes, $^{11}_6C$ and $^{14}_6C$. C^{11} has a half-life of only 20.5 minutes and, although some biological work has already been done with this element, any investigation carried out with it necessarily becomes a race against time. The half-life of C^{14} is estimated between 4000 and 8000 years, but it emits a rather soft β -radiation requiring special arrangements for its assay.

A substance labelled with a radioactive element can usually

suffer a higher degree of dilution and be measurable than a substance labelled with the stable isotope. This difference is inherent in the nature of the assay of radioactive and stable isotopes. During radioactive decay each disintegrating atom emits a radiation or radiations and with highly sensitive instruments it is possible to "count" a great proportion of the disintegrations; hence, radioactive assay may be regarded as the "counting" of atoms. Since the number of atoms in even submicroscopical amounts of matter is very great, it is evident that when studying substances labelled with radioactive isotopes dilutions of the order of $1:10^6$ or even higher may be permissible. With the stable isotopes ratios of the concentration of the common isotope to that of the rare isotope are measured; this condition entails that even with an instrument of the precision of the mass spectrometer at least a few micrograms of the rare isotope must be present in the sample for assay. In biological work a dilution factor of $1:10^3$ for a substance labelled with a stable isotope is quite high, but fortunately higher dilutions are seldom needed. The final choice for the use of any one particular isotope will be determined to a large extent by the specific problem to be investigated.

A good account of the determination of abundance ratios of stable isotopes by the mass-spectrometer can be found in the monograph edited by Wright Wilson (*Preparation and Measurement of Isotopic Tracers*, 1946).

RADIOACTIVE PHOSPHORUS (P^{32}) IN METABOLIC STUDIES

The greatest volume of research, involving the use of radioactive isotopes, has, so far, been carried out with P^{32} .

Phosphorus has a unique position in biological systems; apart from being a constituent of the mineral (inorganic) milieu of the animal organism, it forms energy-rich ester bonds with organic compounds of the greatest importance in the formation of substrates for regulated metabolism and coupled syntheses, as for example in the conversion of glucose to glycogen.

The results of investigations carried out with the aid of radioactive phosphorus and other radioactive elements have recently been reviewed by Hevesy (1947) and here only some special problems related to the use of P^{32} will be considered.

Organic phosphorus compounds do not exchange their phosphate group with inorganic phosphate. If, for example, an aqueous solution of adenylic or nucleic acid is shaken with inorganic phosphate labelled with P^{32} , labelled phosphate does not appear in

the organic ester. If, therefore, a labelled phosphoric ester is isolated from an animal which was injected with labelled inorganic phosphate, this can be taken as evidence that in the animal a synthesis has taken place which involved at some stage the inorganic phosphate of the milieu. This, however, does not necessarily mean that the phosphate group was transferred directly from the inorganic to the organic molecule. Several intermediates could be involved, of which only one might be the direct recipient of the phosphate group from inorganic phosphate.

A large number of investigations have been carried out on the "turnover rate" of organic phosphorus compounds with P^{32} as indicator. The rate of synthesis of a substance may be expressed either by the "turnover rate," which is the amount of that substance regenerated in unit time, or by the "turnover time," which is the time required for the regeneration of the entire amount of that substance present in the system. The determination of these for organic phosphorus compounds is often a difficult problem, because of the complexity or large number of reactions usually involved, and because of the high speed at which many of the phosphorylating reactions occur.

These difficulties are reflected in the diversity of means by which turnover rates for certain organic phosphorus compounds have been expressed. For example, a short interval (1-2 hours) after inorganic phosphate labelled with P^{32} has been injected to an animal, P^{32} can be found in the phospholipid molecules of the various tissues. Many investigators concerned themselves with the rate of phospholipid synthesis and destruction in various organs and expressed the "turnover rates" as the percentage of the injected inorganic P^{32} found in the phospholipids (cf. Perlman, Ruben and Chaikoff, 1937-38). These figures varied between wide limits among the various organs. Entenman *et al.* (1938) found that 6 hours after the injection of labelled inorganic phosphate into hens, 4.3 per cent. of the injected P^{32} appeared in the phospholipids of 100 g. liver tissue, while only 0.037 per cent. was found in the lipids of 100 g. leg muscle. Such a difference might be due to two factors: (a) that the "turnover rate" of phospholipids is much slower in the muscle than in the liver; (b) that the penetration of inorganic phosphorus into the muscle fibre is slower than into the liver cell, or both. If (b) is the major factor in the observed differences, it is clear that should the regeneration of phospholipids in the muscle fibre (by utilisation of the phosphorus compounds already available within the fibre) proceed at the same speed as in the liver, even then only small amounts of P^{32} could be expected to appear in the muscle

lipids. It would be, therefore, totally unjustifiable to conclude from the results quoted above, without knowledge of the respective rates of penetration of inorganic phosphorus into the liver cell and muscle fibre, that the turnover rate of phospholipids in muscle is slower than in the liver. Hevesy and Hahn (1940*a* and *b*) have shown that the permeability of the muscle fibre to PO_4 ions is in fact very much lower than that of the liver cell.

The mechanism by which the phosphate-ester linkages in phospholipids are formed is as yet unknown. The true "turnover rate" for phospholipids could be obtained only if the specific phosphate-donor (or phosphate precursor) of phospholipids were known; then from the ratios of the specific radioactivities of the precursor to that of the phospholipids at different intervals after the injection of labelled phosphate, the rates of formation of the phosphate-ester linkages in the phospholipid molecule could be calculated. The general mathematical treatment of such a problem was presented by Zilversmit, Entenman and Fishler (1943). Until the evidence for the specific phosphate donor of phospholipids is forthcoming, only a relative measure of phospholipid-turnover in the various tissues can be obtained by a method proposed by Hevesy and Hahn (*cf.* 1940*a* and *b*), a method which fixes the upper and lower limits of turnover rates. These authors maintained the specific activity of the plasma inorganic phosphorus constant for intervals ranging from a few hours to several weeks by either continuous infusion or repeated subcutaneous injections of labelled inorganic phosphate. After varying intervals the specific activity of the tissue inorganic-P was compared with that of organic-P. The ratio

$$\frac{\text{specific activity of organic-P at the end of experiment}}{\text{specific activity of inorganic-P at the end of experiment}} \times 100$$

gives the lower limit, and the ratio

$$\frac{\text{specific activity of organic-P at the end of experiment}}{\text{average specific activity of inorganic-P during experiment (corrected for changes during the experiment)}} \times 100$$

the upper limit of the percentage of organic-P renewed during the experiment.

If such ratios for the different organs are compared, a measure of the relative turnover rates of organic-phosphorus compounds in these organs can be obtained. This method, while still not adequate, is probably the best so far applied for the comparison of turnover rates of phospholipids in various tissues. Hevesy and Hahn (1940*a*) found by this method of calculation that in rabbits a maximum of 16.3–18.6 per cent. of the liver phospholipids, and 7.2–7.3 per cent.

of the muscle phospholipids were renewed in about 4 hours. It seems therefore that the turnover rate of phospholipids in muscle is definitely lower than in the liver, but the difference is not as great as the figures of Entenman *et al.* (see above) might suggest. The above arguments apply not only to studies of phospholipids but in general to all problems concerning the turnover rates of organic phosphorus compounds (*e.g.* nucleic acids, etc.), the specific precursors of which are not known.

On previous pages, it was pointed out that in a state of equilibrium the rate of synthesis of a substance must be equal to the rate of its destruction. "Turnover rates," therefore, can be determined also by measuring the rate of disappearance of a labelled substance. Such a method is particularly useful if the "turnover rate" of a substance in the circulating blood is to be determined; its advantages are that the knowledge of the precursor is not required and that the mathematical treatment of the problem, as developed by Zilversmit, Entenman and Fishler (1943), is simple. It is necessary only to introduce into the circulation the labelled substance (*e.g.* radioactive phospholipid in the form of plasma) and then to measure its specific activity at different time intervals.

Let

p = the rate of disappearance of substance A from the circulation

x = the amount of radioactive A present in the circulating fluid at any time

r = the total amount of A present in the circulating fluid (assumed to be constant);

therefore

$\frac{x}{r}$ = the specific activity of substance A , and

$\frac{r}{p}$ = the turnover time.

Then, the rate of disappearance of substance A from the circulation is given by

$$\frac{dx}{dt} = p \frac{x}{r}$$

On integration this becomes

$$\frac{x}{r} = ce^{-\frac{p}{r}t}$$

and by taking the natural logarithm on both sides of the equation

$$\ln \frac{x}{r} = \ln c - \frac{p}{r}t$$

Therefore a plot of $\ln \frac{x}{r}$ (= the natural logarithm of the specific activity of A) against time, t , will yield a straight line with a slope of $-\frac{p}{r}$, which is the reciprocal of the turnover time (with the negative sign).

Zilversmit, Entenman, Fishler and Chaikoff (1943) applied this method for the determination of the turnover rate and turnover time of plasma phospholipids in 3 dogs (see Fig. 2), and found

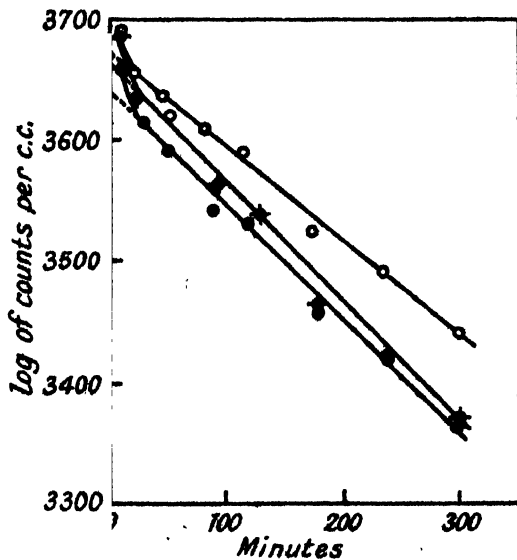


FIG. 2.—The logarithm of the counts per c.c. of plasma phospholipid phosphorus as plotted against time after transfusion of plasma containing labelled phospholipids. ○ = Dog 1B, ⊕ = Dog 2B, ● = Dog 3B.

(From ZILVERSMIT, ENTENMAN, FISHLER and CHAIKOFF, 1943, *J. gen. Physiol.*, 26, 333.)

turnover times of 7.2, 7.8 and 9.3 hours, and turnover rates of 5.2, 7.0 and 8.0 mg. phospholipid phosphorus per hour. These values are the most accurate by all present-day criteria.

THE APPLICATION OF ISOTOPES TO ANALYTICAL PURPOSES: THE ISOTOPE DILUTION TECHNIQUE

It is often necessary to determine *quantitatively* in biological materials some organic component for which there are no specific reactions available and the analysis of which could be carried out only after isolation of the substance in a *pure state*. These conditions are, more often than not, incompatible with one another

because the purification of an organic substance usually involves losses. The isotope dilution procedure requires the isolation in a pure state of only a small amount of the substance to be determined, the yield being unimportant. Losses, therefore, in the course of purification are permissible. The method is based on the principle that a compound which contains an abnormal concentration of a rare isotope is inseparable by the ordinary laboratory procedures from its natural analogue. It is required only to add to the unknown material a known amount of the isotopically labelled substance which is to be determined and, after thorough mixing, to isolate a pure sample of the substance from the mixture and to determine its isotope content. This method was first applied by Schoenheimer, Ratner and Rittenberg (1939) to the determination of the leucine content in protein hydrolysates of whole rats, and was further developed by Rittenberg and Foster (1940), and by Graff, Rittenberg and Foster (1940). It has also been described, apparently independently, by Ussing (1939). The method has been used especially for the quantitative determination of fatty acids and amino-acids (see also Foster, 1945; Shemin, 1945). An example of the application of this technique is quoted here from Rittenberg and Foster (1940). If deuterio-palmitic acid is added to a mixture of isotopically normal fatty acids, and then palmitic acid is isolated, this will be a representative sample of the mixture of the added deuterio-palmitic acid and the palmitic acid originally present. Let x be the amount of palmitic acid added; C_o the deuterium content of the palmitic acid; C_f the deuterium content of the finally isolated sample of palmitic acid. Then the amount, y , of palmitic acid originally present is given by

$$= \left(\frac{C_o}{C_f} - 1 \right) x$$

This equation, however, ignores the fact that the molecular weight of deuterio-palmitic acid (M_1) is greater than that of the natural analogue (M_2) and can, therefore, strictly be applied only if the ratio M_1/M_2 does not differ significantly from one. Recently Gest, Kamen and Reiner (1947) have developed an exact set of equations for the isotope dilution technique, which include corrections for differences in the molecular weights of the labelled and natural substance. The above equation with this correction would be:

$$y = \left(\frac{C_o}{C_f} - 1 \right) \frac{M_2}{M_1} x$$

The method can, of course, be used with substances labelled with radioactive isotopes as well. Here C_o is the specific activity of the

added compound and C , the specific activity of the compound isolated after mixing. Since the amount (in weight) of the radioactive element introduced as labelling agent is minute, no correction for differences in molecular weights is necessary.

It might be pointed out that in the experiments of Bloch and Rittenberg (1945), quoted earlier, in which the rate of acetate formation was determined in the intact animal, the principle of the isotope dilution technique was applied to the living animal.

The examples given have been selected to illustrate the diverse uses of isotopes in biological and particularly in biochemical researches. While the achievements are already great, the increasing supplies of both stable and radioactive isotopes will mean that even more can be anticipated. It might reasonably be expected, if one may make a forecast, that the wider application of the carbon isotopes, C^{13} and C^{14} , will yield particularly valuable information. The biochemist and organic chemist are especially interested in the utilisation of the carbon skeleton of one substance for the synthesis of another (*e.g.* synthesis of fats from carbohydrates). The labelling of organic compounds with deuterium is ultimately a labelling of the carbon atom to which the deuterium is attached, and the fundamental problems which have already been attacked successfully with this method emphasise the importance of labelling the carbon atom of molecules. Substances in which one carbon atom is labelled with deuterium and another with a carbon isotope (as in some of the quoted researches with doubly labelled acetic acid) might be regarded as the ideal indicators for many fundamental researches.

SUMMARY

The various types of investigations to which isotopically labelled substances can be applied may be summarised under the following headings: (a) the determination of the fate of a substance in a biological system; (b) the study of certain biochemical reactions (*e.g.* interconversions, breakdown and synthesis, determination of precursors); (c) the determination of "turnover rates" of body constituents; and (d) the quantitative analysis of biological material by the isotope dilution technique. These applications have been illustrated by examples from the literature.

Figs. 1 and 2 are reproduced with the permission of the authors of the respective articles and the Editors of the *Journal of biological Chemistry* and *Journal of general Physiology*, to whom my thanks are due.

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RECENT ADVANCES IN SCIENCE

ASTRONOMY. By A. HUNTER, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

A FULL solar cycle has now passed since the first intensive observational work on chromospheric flares was started. Interest in the geophysical effects of these remarkable solar "eruptions" (as they were then called) was early stimulated by some suggestive coincidences between their times of occurrence and those of short-wave radio fade-outs; and it quickened as geomagnetic disturbances, low-latitude auroræ and (more recently) bursts of electromagnetic radiation in the radio and radar frequency ranges were shown to be closely connected with flares. It is doubtless a result of commercial and military considerations that the relation between flares and telecommunications is better understood now than any of the other solar-terrestrial relationships, but that is not to say that even here, after some years of intensive research, the facts are well correlated with theory. The "ionospheric storms," which cause more trouble even than the more abrupt but evanescent fade-outs, are still something of a mystery, and the precise mechanism by which the ionosphere is disturbed by the ultra-violet radiation from flares or by their associated particle streams is frankly unknown.

The astrophysical side of the matter is in no better case—perhaps even worse, for the drive has been to elucidate the terrestrial effects of flares rather than to explain the solar phenomena themselves. The observational facts are tolerably well known now: limited regions of the bright hydrogen and calcium flocculi that normally surround a sunspot (invisible, it will be recalled, in white light, and only detectable in the monochromatic light of the appropriate atoms or ions) occasionally flash up in brightness by a factor of from 2 to 10 in about five or ten minutes and fade again more slowly, the whole phenomenon usually being over in an hour or so. The emitting particles are practically stationary, their spectrum lines showing no Doppler displacement wherever the flare occurs on the disk. There is thus no "eruption" in the volcanic sense,

and any lateral spread of the luminosity is an infection transmitted horizontally to neighbouring atoms, not a bodily overflow of the emitting matter. Nevertheless, dark absorption markings that appear close to disk flares at the moment of their peak intensity are probably eruptive surge-prominences seen in plan. It is inferred from geophysical evidence that the enhancement in the visible radiation is accompanied by a still greater enhancement of the ultra-violet, and that simultaneous radial emission of a neutral cloud of charged particles usually occurs in a wide cone, at a speed well above the velocity of escape (617 km./sec.). It is these radiations which cause, respectively, on the one hand the simultaneous radio fade-out over the Earth's sunlit hemisphere and the abrupt "crochet" recorded on magnetograms, and on the other (if the flare is fairly central on the disk) the world-wide magnetic storm, the auroral display in low latitudes and the prolonged interference in radio channels passing close to the polar regions.

But when we come to enquire into the trigger initiating these phenomena, the solar flare itself, we find very little is yet known. The physical processes possible in the combined electric and magnetic fields surrounding sunspots are as yet incompletely explored quantitatively, though many suggestive comparisons can be made with terrestrial analogues, both electrical and mechanical. One interesting working hypothesis of the solar flare has recently been put forward by D. S. Evans (*The Observatory*, 67, 218, 1947), who takes his analogy from meteorology. He points out that (as is now generally conceded) convection in the outer regions of the sun may be maintained by a process of alternate storage and release of energy through hydrogen ionisation and recombination in much the same way as terrestrial atmospheric convection is promoted by energy storage as latent heat of vaporisation of water. Disturbances in the normal convective flow certainly occur in the terrestrial atmosphere, he argues: should they not also be expected on the sun? Developing this line of reasoning, he suggests that the brightness of the flare flocculus is thermal in origin, sub-photospheric material being transported into the lower chromosphere by something in the nature of a solar tornado. In this the flare differs from a sunspot, which he relates to the much larger and more stable cyclonic depression. The relation between the two types of disturbance provides evidence for the hypothesis: just as tornadoes and depressions are topographically connected, so there is invariably a spatial relation between flares and sunspots. The small initial size, random appearance and short life of the average flare are also strongly reminiscent of the terrestrial tornado.

The rapid circulatory motion postulated by Evans in the parent flocculus is accompanied, as in the terrestrial case, by a reduction of pressure and a strong upward suction in the central regions of the flocculus, which combine to drag material from great depths to the solar surface. The part of the photosphere involved in the flare will be thrown upwards, and may escape from the sun as an eruptive prominence if its velocity is high enough. Even if this is not the case, the presence of upward-moving material accounts for the high velocities shown by the absorption markings seen in the neighbourhood of flares, whilst the observed downward motions are doubtless those of photospheric gases drawn into the centre of the vortex when the inherent instability leads to collapse. The surge prominences seen on motion-picture films of the chromosphere present exactly this appearance of emergent spikes of material being sucked back along their original paths.

As to the emission of charged particles, Evans here relies on Milne's mechanism by which (for example) calcium ions, caused to move outwards initially in the flare neighbourhood by this tornado eruption, are exposed to stronger and stronger radiation pressure as they "climb out" of the absorption lines by virtue of their increasing Doppler displacements. With an initial velocity of 300 km./sec. endowed by the tornado, calcium ions are calculated to reach a terminal velocity of 1850 km./sec. This leads to a sun-earth travel time of 23.8 hours, which corresponds closely to the average delay between flare and magnetic storm. The cone of ejection would also be expected to be limited by the vortex mechanism, as is indeed observed.

As its author points out, this tornado theory suggests a number of observational tests, by terrestrial analogy. For example, in the northern hemisphere tornadoes tend to appear in the south-east quadrant of a depression; do flares tend to occur preferentially in any particular quadrant of the associated sunspot area? And do they tend to move across the spot diametrically, as tornadoes do across terrestrial cyclones? Such questions as these will have to await analysis of the many observations made during the past cycle and to be made during the coming maximum of solar activity. Others, however, can be answered already, and not always favourably to the "tornado" hypothesis. First of all, the basic assumption that the brightness of the flare is due to high temperature, sub-photospheric matter being brought to the surface mechanically, seems at variance with the extreme rarity with which flares can be seen in integrated light. Since 1859 not more than half a dozen cases have been recorded (see *The Observatory*, 67, 156, 1947) where

the brightness of a flare extends to the continuous spectrum, as it surely should if gases from the deeper layers are sucked to the solar surface. Yet at solar maximum half a dozen flares a day are not uncommon. A flare is bright on a spectroheliogram or in a spectrohelioscope because the comparison is there made with the disk observed in the residual light at the bottom of a dark absorption line. In these circumstances an intensity of even 50 per cent. of the neighbouring continuum looks bright. Even the 300 per cent. which has been observed in bright flares refers only to the centre of the emission line produced by reversal, and this makes practically no difference to the integrated emission from the flare region.

Further, as Ellison points out (*The Observatory*, 68, 31, 1948), if the flare material is suddenly transported from great depths, its spectrum lines should show (in the early stages at least) some Doppler shift due to motion of approach when the flare is fairly central on the disk. Such shifts in the *emission* lines are, however, so rare as to be subjects of dispute when they are recorded, though the neighbouring absorption markings commonly show quite high velocities, first of approach and then of recession. If the flare is fundamentally a vortex, Ellison argues too that it should ordinarily show some rotational symmetry, whereas many flares on the disk show in plan as long thin filaments. Furthermore, a vortex seen at the limb should show opposite Doppler shifts in the light emanating from its two sides. This effect, though of not uncommon occurrence in the base of limb *prominences*, is not seen in flares.

Evidently the tornado hypothesis has its drawbacks as well as its attractive features—at this stage, as indeed its author confesses, all such theories must be regarded as tentative. Another and quite different approach to the subject is made by Giovanelli (*Monthly Notices of the Royal Astronomical Society*, in press; *The Observatory*, 68, 6, 1948), who prefers to consider the growth of the magnetic field of the associated sunspot as the fundamental factor in causing flares. Asserting that observationally the probability of occurrence of a flare is a function not of the maximum value of the spot's magnetic field, but of its rate of growth or decay, Giovanelli points out that during the growth of a spot the change of magnetic flux will induce an electric field in the neighbouring solar material. The resulting currents will of course be limited by the available supply of ions, i.e. by the electrical conductivity of the chromosphere. Now it is well known that in the solar atmosphere the electrical conductivity in the direction of the magnetic field is so much higher than that across the field that any currents are constrained to follow the lines of force. Giovanelli shows that

when the magnetic field exceeds about 1 gauss (as it must do near a spot) the gas is effectively an insulator across the field. This is graphically shown in most spectacular fashion by motion pictures of active prominences, where matter is seen to stream down curved trajectories (doubtless lines of force) towards centres of attraction (sunspots). But the magnetic field near a spot is the resultant of two fields, the spot field and the general magnetic field of the sun. There will therefore exist neutral points, at which the gas becomes conducting, at about 3×10^9 cm. from the typical spot. The suggestion is that currents of electrons flow through these neutral points down the lines of force to the sun's surface, and complete the circuit at deeper levels where the electron pressure is so great that the value of the magnetic field will not affect the transverse conductivity. Now whilst the induced electric field is weak, the electron energy will be limited by elastic collisions with positive ions. As the electron velocity increases, however, the mean free path increases still more rapidly, and the rate of gain of energy between collisions goes up. At the neutral points this rate of gain may become uncontrollable under suitably favourable conditions, the electron energy will no longer be limited by collisions, and a discharge, i.e. a flare, will occur at a critical value of the electric field, the electrons exciting the surrounding gas into luminescence. Even below this critical field, the random differences of electron velocity will give rise to sporadic but quickly suppressed excitation which may be the cause of the normal bright flocculi surrounding spots.

It is a consequence of this "electronic excitation" theory that flares should occur at loci related in a definite geometrical way to the combined magnetic field of the associated spot and of the whole sun. For a unipolar spot, for example, they may lie on the circumference of a circle passing through the neutral point and centred on the spot. It remains to be seen whether flares do in fact favour such locations. One attractive feature of the theory has already been pointed out by Ellison and Hoyle (*The Observatory*, 67, 181, 1947). It has long been a puzzle why the hydrogen lines in flare spectra should be so wide. Observed values at the time of peak intensity vary from 2 Å. for the less intense flares to 16 Å. for the brightest. If these widths are thermal Doppler effects, incredible temperatures have to be invoked—up to 10^6 degrees. Astronomers have already had to admit the existence of excitation temperatures of this order in the corona (SCIENCE PROGRESS, 34, 557, 1946); they will be reluctant to admit that they can occur in isolated patches of the chromosphere without producing other major effects (for example, visibility in white light) that are not normally observed.

They are confirmed in this view by the fact that the lines excited in a flare are not those to be expected with such a high temperature (e.g. the coronal lines) but are the lines of neutral and ionised metals commonly shown in the ordinary disk spectrum. Yet $H\epsilon$ and the H line of ionised calcium, which occur side by side in the ultra-violet, show very different widths, the former being wide, as are the other members of the Balmer series, and the latter narrow. On the face of it this could be taken as evidence for a thermal origin of the broadening (the light H atoms being naturally affected more than the Ca ions) as against, say, any macroscopic turbulence, which would hardly differentiate between the two kinds of atom. But Giovanelli's theory now gives a loophole of escape. Ellison and Hoyle suggest that the broadening is to be ascribed to the Stark effect due to the field of electrons. Estimating roughly the electron density in the flare from the requirement that electron-proton recombinations should lead to the observed increase (small but detectable) in the intensity of the continuous spectrum, they combine this estimate with one for the density of neutral hydrogen atoms in the first excited state to get an idea of the variation of the absorption coefficient with distance from the centre of $H\alpha$, assuming Stark broadening to be effective. The resulting half-width of 4.1 Å. is in good agreement with observation. The metallic lines (in particular the H and K lines, which are observed to be much sharper than the hydrogen lines) will of course be practically unaffected by Stark effect, but there is no difficulty in attributing their widths to thermal Doppler broadening.

The electronic excitation theory, attractive though it is in covering points like these, is already (like the tornado hypothesis) coming under fire from the observers. Newton (*The Observatory*, 68, 29, 1948) challenges its basic assumption that flares occur most frequently when the spot is growing or decaying rapidly. Whilst this may be so for minor flares, Newton shows, from an analysis of 58 of the greatest flares ever observed, that they actually tend to avoid the period of early growth and favour that of maturity or early decay of spot groups. It is in this latter period that the magnetic field of the spot tends to remain stable at its maximum value. With no change of flux there can be no induced electric field; and we are left with the choice of rejecting the major flares as atypical or withdrawing the main prop of the electronic excitation theory.

Clearly no finality nor even stability has yet been reached with hypotheses of flare production. This is perhaps hardly surprising when it is remembered that a flare is a fleeting disturbance in the

chromosphere, itself an entity the nature of whose excitation and support above the solar surface is still a matter of conjecture. But it is so frequently the case that further progress in a study depends on paying concentrated attention not to the commonplace but to the anomalous that no solar physicist will consider time ill spent which is devoted to more and more observations of these still mysterious solar flares.

PHYSICS. By F. A. VICK, O.B.E., Ph.D., F.Inst.P., The University, Manchester.

ELECTRICAL NOISE.—Spontaneous fluctuations of current and voltage have long been known. If, for example, the gain of an audio-frequency amplifier is increased, a point is reached at which there is a continuous "mush" of sound from the loudspeaker. It is from this phenomenon that the term "electrical noise" has arisen, but the effect may be appreciated just as well by, e.g. the irregular movement of the spot of a cathode-ray oscillograph connected to the output of an amplifier. The fluctuations are due to a number of causes, some only of which are well understood. The position ten years ago was made clear by E. B. Moullin in his original and stimulating book (*Spontaneous Fluctuations of Voltage, due to Brownian Motions of Electricity, Shot Effect and Kindred Phenomena*, 1938). The subject is not only of theoretical interest, but also of great practical importance since, for example, the practical limit to amplification by thermionic valves or other means is set by the "noise level."

If a conductor of any kind is in thermodynamic equilibrium at absolute temperature T , it is well established that, on open circuit, there is developed across it a fluctuating voltage V , whose mean square value is given by

$$\overline{V^2} = 4RkT\delta f \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where R is the resistance of the conductor, k is Boltzmann's constant, T the absolute temperature and δf the band of frequencies within which the voltage is measured. It will be observed that the mean square voltage is proportional to the frequency range chosen, and it is found that the equation holds for all frequencies such that hf is much less than kT , where h is Planck's constant. Since h is so small, even compared with k , the condition is satisfied for all frequencies within the normal audio and radio ranges. $\overline{V^2}$ is also proportional to T , and Lawson and Long have suggested (*Phys. Rev.*, 70, 220, 1946) that its measurement might be used to establish the thermodynamic scale at very low temperatures, using, for

example, a quartz crystal. The practicability of the method has been examined by E. Gerjuoy and A. T. Forrester (*Phys. Rev.*, **71**, 375, March 1947). Taking into account the conditions to be met in the measuring circuit and that other fluctuations arising (see below) must be less than the one to be measured, they conclude that the minimum measurable temperature, using available values and components, is about 2° K., and possibly down to as low as 0.1° K. using specially designed components under optimum conditions.

If a current is passed through a conductor, by the application of an e.m.f., the thermodynamic equilibrium is upset. For metallic conductors (except when in the form of very thin films) no extra "noise" is generated by the passage of the current, but this is not so for non-metallic conductors such as carbon and semiconductors. In addition to the fluctuation voltage given by equation (1), there is one which is apparently proportional to some low power of the mean current and inversely proportional to a low power of the frequency. This source of noise becomes small in comparison with the thermal agitation noise at high frequencies, but may greatly exceed it at sufficiently low frequencies. A similar effect had been noted previously in some thermionic valves and called the "flicker effect" (see below), so the extra fluctuations observed at low frequencies are often called "flicker noise." Interest in flicker noise has been stimulated in recent years by the increased use of amplifiers of high gain working at very low frequencies, e.g. for recording nerve and brain potentials, and also by various applications of semiconductors and barrier-layer rectifiers.

In thermionic valves, if it is assumed that the emission of electrons from the cathode is completely random and each travels to the anode independently of the others, it is apparent that the current to the anode should exhibit statistical fluctuations. It was shown by Schottky in 1918 that the mean square fluctuation of the current about the mean value is given by

$$(\delta i)^2 = 2eI\delta f \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where e is the electronic charge, I the mean current with components in the narrow frequency band between f and $f + \delta f$, assuming that all electrons from the cathode reach the anode. This fluctuation is generally called "the shot effect," and equation (2) has been found to hold experimentally for the saturated current from a pure tungsten filament in a diode valve. For oxide-coated and other complex cathodes, however, there is found to be superposed, especially at low frequencies, the "flicker noise" already mentioned,

and this was supposed to be due to the emission of electrons in bunches owing to changes at the cathode surface. Most recent papers have been concerned with determining how far equation (2) is applicable and with attempts to gain a better understanding of flicker noise when electricity is conducted through solids and through various types of discharge tube.

An excellent mathematical analysis of the statistical aspects of thermal noise and the shot effect has been given by S. O. Rice (*Bell System Techn. Journal.*, **23**, 282, 1944; **24**, 46, 1945). This paper is difficult to summarise, but will be used for reference by all who work in the subject. Experiments undertaken by R. Fürth and D. K. C. MacDonald (*Proc. Phys. Soc.*, **59**, 388, May 1947) gave results in good agreement with the theory for shot effect in a saturated diode and thermal noise in a tuned circuit. These authors were able also to extend the theory in regard to the rate of change from one value of the fluctuation to another.

As has already been mentioned, equation (2) was derived for a saturated diode, where all the electrons emitted thermionically reach the anode. In most practical applications, however, there is a space-charge cloud of electrons between anode and cathode acting as a barrier whose height increases with increasing current. Under these conditions, equation (2) must be modified :

$$(\delta i)^2 = 2eT\Gamma^2\delta f \quad . \quad . \quad . \quad (2a)$$

where Γ is less than one. If there is no barrier, *i.e.* no potential minimum between anode and cathode, $\Gamma = 1$, and this should be so when the anode is sufficiently *negative*, as well as sufficiently positive. An investigation of the validity of equation (2) under retarding field conditions has been described in another paper by MacDonald and Fürth (*Proc. Phys. Soc.*, **59**, 375, May 1947). Treating the retarding potential of the anode as adding to the negative potential barrier ϕ (the work function) to be overcome by the electrons evaporated from the interior of the metal we have

$$I = \text{const. } I_s \exp. (eV/kT). \quad . \quad . \quad . \quad (3)$$

where I is the current for a retarding potential V , and I_s is the saturation current from the cathode at the same absolute temperature T . This leads at once to an expression for R_a , the differential resistance of the valve, since

$$R_a = \frac{\partial V}{\partial I} = \frac{kT}{eI} \quad . \quad . \quad . \quad (4)$$

Thus $R_a I$ should be proportional to T , giving an experimental criterion for true retarding field conditions. MacDonald and Fürth

measured δi for a diode under retarding field conditions by comparison with a diode under saturation conditions and satisfying equation (2). They found that for I below a limiting value, depending on the valve employed, I' was indeed one, as predicted. They point out, too, that equation (4) leads to a method of determining cathode temperature, since

$$\frac{v}{I}(R_a I)$$

provided that the current I under the retarding field conditions is less than the critical value. The authors also give an approximate method of calculating this critical value.

The flicker effect has been observed in thermionic valves with oxide-coated cathodes, in photoconductive cells, carbon resistors, copper oxide rectifiers and carbon granule microphones. In valves, Schottky ascribed the effect to fluctuations in the adsorbed layer of atoms on the cathode surface, producing local changes in effective work function (*Phys. Rev.*, **28**, 74, 1926). As has already been described in this series (*SCIENCE PROGRESS*, **35**, 84, 1947), R. L. Sproull has postulated a diffusion process at the surface of oxide-coated cathodes to explain the rapid fall in emission when current is suddenly drawn from the cathode (*Phys. Rev.*, **67**, 166, 1945). G. G. Macfarlane examines this suggestion further, not only for such cathodes but also for the various cases in which there are contacts involving semiconductors. He assumes that a mechanism exists for the movement of polarisable atoms or ions in clusters to the surface of a crystal, where they alter the height of the potential barrier appreciably and then diffuse back into the body of the crystal as ions at a rate proportional to the strength of the electron current. The alteration of potential barrier in patches at the crystal surfaces would, on this view, alter the current flowing across the contacts in the case of rectifiers, granule microphones, etc., or the electron emission from the surface of a cathode, and produce the flicker effect. Macfarlane was able to derive expressions in reasonable agreement with experiment, but it is only fair to say that the details of the mechanism still form the subject of controversy. Further experiments are needed.

THE SEPARATION OF ISOTOPES IN QUANTITY BY ELECTRO-MAGNETIC MEANS.—In the previous article in this series (*SCIENCE PROGRESS*, **36**, 82, 1948), brief mention was made of the use of a mass-spectrograph for the separation of isotopes. Since that was written, a paper by L. P. Smith, W. E. Parkins and A. T. Forrester has appeared (*Phys. Rev.*, **72**, 989, Dec. 1947), giving an interest-

ing discussion of several electromagnetic methods of separating isotopes in reasonable quantity, including a mass-spectrograph of the Dempster or 180° type. A major limitation is the effect of the large concentrations of ions forming "space-charges" tending to spread the beams for each isotope and spoil the resolution. Fortunately, the potential distributions are such that electrons of low and moderate energies are attracted to the centre of each ion beam. Once in the beam they cannot move far from the beam axis, but they can move along it until they reach the ends, where again the potential distribution is such as to prevent them getting out. Except for this latter feature, and the reversal of sign, the process is reminiscent of the method used to focus the electron beam in a gas-focused cathode ray tube. The efficiency is increased in a 180° deflection type mass-spectrograph, since the magnetic field causes the electrons in the beam to execute longer paths, like a number of small cycloids. In effect, therefore, the positive ions are attracted back towards the centre of the beam by forces dependent on the electron concentration there. It would probably be worth while introducing electron emitters specially for the purpose.

Smith, Parkins and Forrester go on to discuss the influence of space charge on other methods of electromagnetic separation, including those involving time-variations of electric fields. They then describe a method which makes use of the divergence due to space charge to obtain the required separation. The divergence is such that lighter particles are carried to larger radii, so that a collector of particular radius could be arranged to collect ions of one mass only. An apparatus was constructed to test the principle, and consists of a central source from which ions are accelerated radially by concentric grids. Under the action of an axial magnetic field the ions describe orbits which depend on the masses so as to permit the collection of the heaviest ions on the outermost cylinder, the lighter fraction being turned back towards the centre and collected on other electrodes. Practical difficulties in constructing a workable separator are described in the paper, and the preliminary results showed the method to be a promising one. It is only fair to add that the paper was completed in 1942, though only now published.

MAKING USE OF THE PRESSURE OF LIGHT.—It is well known that the pressure of sunlight is about 4.5×10^{-5} dyne per sq. cm. at the earth's surface, and that extreme refinements of experimental technique were necessary to measure it. Practical application of the source of pressure seemed most unlikely, but one has just been suggested by J. W. Beams (*Phys. Rev.*, 72, 987, Nov. 1947). He observed that the frictional drag on a magnetically suspended

spinning spherical rotor could be made extremely small if the gas pressure surrounding the rotor is sufficiently low. For example, when such a rotor 1.59 mm. diameter spinning at 100,000 r.p.s. in air at 2×10^{-6} mm. Hg. was allowed to "coast," it took about an hour to lose about 0.1 per cent. of its rotational velocity. Consequently, if a few square centimetres of sunlight directed tangentially on the spherical rotor could be absorbed, it should be possible to drive it to its explosion speed if the air pressure is less than 5×10^{-7} mm. Hg. Preliminary experiments were sufficiently promising to encourage the planning of more extensive ones, including more accurate determinations of the pressure and angular momentum of light.

GENERAL AND PHYSICAL CHEMISTRY. By J. W. SMITH, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic.

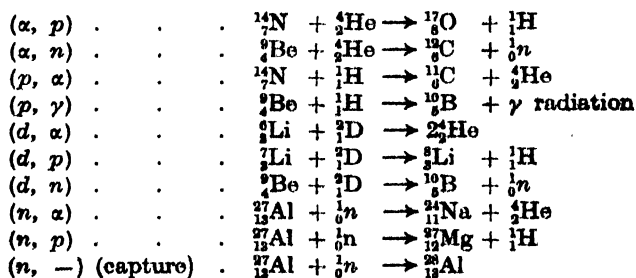
THE TRANS-URANIC ELEMENTS AND THEIR CHEMICAL SIGNIFICANCE.—Apart from the important part they played in respect of the Atomic Bomb, undoubtedly one of the outstanding scientific achievements of the present century has been the production of new "artificial" elements of higher atomic number and atomic mass than those known in the earth's crust. Fundamentally the procedure used for this purpose was based on the earlier observations of Rutherford and Chadwick and of Cockroft and Walton, who had found that when atomic nuclei were bombarded with small particles of high energy, such as α -particles, protons, or deuterons, the whole or part of the particle would coalesce with the nucleus, yielding a new atom, the excess energy being frequently emitted as γ -radiation.

This process generally leads to the production of a nucleus of different atomic number to the original nucleus, i.e. effects transmutation from one element to another. Such nuclear reactions may be classified according to the particle with which the bombardment is carried out and the small particle (if any) emitted as a residuum from the reaction. Thus an (α, n) reaction is one in which a nucleus is bombarded with an α -particle and a neutron is ejected. In considering the equations for such nuclear reactions, it must be remembered that the sum of the atomic weights of the resultant nucleus and resultant particle must be equal to the sum of the atomic weights of the initial nucleus and bombarding particle, whilst the same rule holds for the sum of the atomic numbers of the reactants and resultants. Thus, in the example mentioned, since the α -particle is a helium nucleus of atomic mass 4 and atomic number 2, whilst a neutron has a mass of unity but no charge,

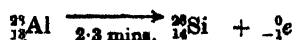
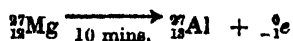
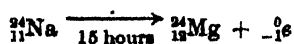
such a reaction will result in the production of nucleus of mass three units and atomic number two units greater, respectively, than those of the initial nucleus.

It was observed by Fermi that the neutrons so produced would also interact with atomic nuclei, either being captured and so yielding an isotope of the initial atom one unit greater in mass, or else with the emission of other particles. Hence the number of possible nuclear reactions is quite considerable.

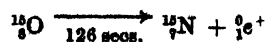
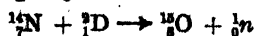
Some of the types of reaction which have been observed are listed below, together with classical examples of each.



The isotopes so produced have in many cases not been found to exist, in appreciable quantities at any rate, in the naturally occurring elements. These nuclei are frequently radioactive and by decay processes involving the emission of α -particles, β -rays (electrons), or positrons yield yet further nuclei of either known or new isotopes. Thus the three products which are obtained on the interaction of ^{27}Al with neutrons are all radioactive β -ray emitters of half-life periods 15 hours, 10 minutes, and 2.3 minutes, respectively, and by decay yield ^{24}Mg , ^{27}Al , and ^{28}Si .

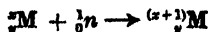


Similarly the (d, n) reaction with ^{14}N yields the radioactive ^{16}O isotope, which decays rapidly with emission of a positron to give ^{16}N



The investigation of similar bombardment of the heavier elements had already commenced prior to 1939. It was found that the high kinetic energy of the neutrons emitted from beryllium favoured the occurrence of the "degradation reactions" of the (n, α) and (n, p) types, whereas the capture $(n, -)$ reactions occurred scarcely at all

with light nuclei and only with a low probability for the heavier elements. It was soon appreciated, however, that the neutrons could be slowed down by passage through water, preferably heavy water (D_2O), paraffin wax, or graphite until they reach practically the normal thermal energy of gas molecules, in which state they would react with the heavier nuclei undergoing capture, yielding an atom isotopic with the capturing atom but of mass one unit greater.



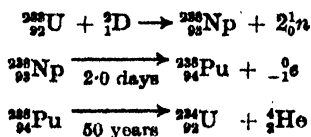
The study of the neutron bombardment of uranium was commenced by Fermi in 1934, and it was found that new unstable nuclei were formed which underwent radioactive decay. Subsequently no less than four β -ray active species were detected and, since β -ray emission causes an increase in atomic number, the emissions were regarded as due to trans-uranic elements produced by neutron capture and subsequent radioactive decay. This view was not very long lived, however, it being found that these products could be identified as radioactive isotopes of such elements as barium, lanthanum, iodine, and tellurium in the middle of the periodic table. Frisch and Meitner pointed out that this could only mean that the neutron bombardment of uranium had led to a new type of reaction, whereby the nucleus had split into two parts of approximately equal mass. They proposed the name "Nuclear Fission" for this phenomenon.

In spite of the overwhelming importance of these fission reactions and of their development as chain reactions, probably the more significant point, in so far as the purely chemical aspect of the matter is concerned, is that it has been found that, under suitable conditions, the simpler forms of nuclear reaction, such as had been observed with lighter elements, can also be effected with uranium, with the production of elements of atomic number greater than that of the latter element. The first four elements of this type, of atomic numbers 93, 94, 95, and 96, have already been identified and called neptunium, plutonium, americium, and curium, respectively.

Although the whole story of the nuclear reactions which have been studied, and the chemical properties of the products which have been investigated, has not yet been told, it is now possible to obtain a general picture of the situation, since, in addition to the Smyth Report (*Atomic Energy*, U.S. Government Printing Office, 1945), other information regarding the nuclear reactions, previously withheld, has now been released (Seaborg *et al.*, *Chem. Eng. News*, 1945, **23**, 2190; 1946, **24**, 1192; *Phys. Review*, 1946, (ii), **69**, 366, 367; Emeléus, *Some Aspects of Nuclear Chemistry*, Institute of

with uranium. Under reducing conditions the neptunium was precipitated from aqueous solutions on lanthanum fluoride as carrier, whereas under non-reducing conditions it was not precipitated, thereby indicating the insolubility of the fluoride in the lower oxidation state and its solubility in the higher state.

Plutonium.—Late in 1940, Seaborg, McMillan, Wahl, and Kennedy showed that the bombardment of uranium with deuterons in the cyclotron leads to the ($d, 2n$) reaction with the production of yet another isotope of neptunium, the β -ray emitting $^{238}_{93}\text{Np}$ of half-life 2.0 days which decays to an α -particle emitting isotope of element No. 94, of half-life period about 50 years. Following the convention used in naming neptunium this was called plutonium (Pu). The reactions involved can therefore be written



The more important isotope of plutonium, however, is that of mass 239, produced by the β -ray emission from the ^{239}Np isotope of half-life period 2.3 days and which was discovered by Seaborg, Segrè, Kennedy, and Lawrence in 1941. This is a fairly stable nucleus of half-life about 24,000 years, but it has very great significance owing to its undergoing fission with slow neutrons, similar to that occurring with ^{235}U . The Smyth Report discloses the astounding fact that from the Bohr-Wheeler theory of fission and certain empirical relations among the nuclei, Turner and others inferred, before ever this isotope had been produced, that it would be a fairly stable α -particle emitter and would probably undergo fission when bombarded by thermal neutrons. This prediction was so amply confirmed that it was used in at least one of the atomic bombs which were exploded in 1945. Its advantage for production on a large scale, as compared with ^{235}U , is readily appreciable when one considers that it is chemically different from uranium and therefore should be separable in a pure state by chemical means and without resort to the complex physical means required to separate ^{235}U from ^{238}U .

The system adopted for the production of plutonium was a controlled chain mechanism involving two main processes, production of neutrons by fission of ^{235}U and later by ^{239}Pu , and neutron capture by ^{238}U with eventual formation of ^{239}Pu . By arranging uranium slugs in pure graphite, which served to slow down the neutrons until their energy was only slightly greater

than that of thermal neutrons, a balance was kept between these processes, so that all excess neutrons over those required to maintain further fission reactions were absorbed in the ^{238}U . In this way ^{239}Pu accumulates in the slugs until it is destroyed as fast as it is formed. The slugs are then removed and the plutonium and uranium separated by chemical means.

Since the chemical properties of plutonium are very similar to those of uranium, it is evident that a full knowledge of them has extreme importance with respect to the method of separating it on a fairly large scale from uranium and from the fission products which also remain in the slugs. The first investigations were made by Seaborg and his collaborators using the ^{239}Pu isotope formed in the cyclotron. By tracer technique they were able to show that it has at least two oxidation states, even stronger oxidising conditions being required to oxidise it to the upper state than are necessary in the case of neptunium. These studies introduced the completely new technique of ultra-microchemical scale of operation, in which quantitative analysis was conducted using volumes of the order of 10^{-1} to 10^{-5} ml. in "test tubes" and "beakers" of internal diameter 0.1–1 mm. and the amounts of reagents and precipitates weighed were between 0.1 and 100 micrograms. As a result of this work separation processes capable of extension to large-scale operation were devised. The most useful process appears to involve the precipitation of the plutonium, reduced to the tri- or tetravalent state with a carrier of lanthanum fluoride. The precipitate is then dissolved by fuming with sulphuric or perchloric acid and the plutonium oxidised to the hexavalent state, after which the carrier is re-precipitated, together with carried fission products, leaving the plutonium in solution, since plutonyl fluoride, in which the plutonium is in the hexavalent state, is a soluble salt. Further purification can then be effected by reducing the plutonium to the trivalent state with an excess of sulphurous acid and precipitating it with ammonia as $\text{Pu}(\text{OH})_3$.

The recent publication of Harvey, Heal, Maddock, and Rowley (*loc. cit.*), although describing work carried out with only about 5 milligrams of ^{239}Pu , gives a fairly clear picture of the chemistry of the element. This shows that plutonium is a typical transitional element in that it exists in the three well-defined valency states of 3, 4, and 6. A pentavalent state has also been mentioned by Seaborg. It is most stable in the tetravalent state, but it also gives rise to whole series of compounds in which it is tri- and hexavalent, respectively. The trivalent state is a much less powerful reducing agent than in the case of uranium, and, indeed, it is less

reducing than tetravalent uranium, since plutonium can be reduced from the tetravalent to the trivalent state by means of tetravalent uranium. In aqueous solution plutonium can also be reduced to the trivalent state by means of sulphur dioxide, hydroxylamine hydrochloride, hydrazine hydrochloride, or iodides. The reduction can also be effected electrolytically at a platinum cathode or by shaking with mercury in chloride solution. The reduction by sulphur dioxide is slow at room temperature, but is complete in about 5 minutes in hot solution. The reduction by iodide ion is also rather slow at room temperature in both chloride and sulphate solutions, and hence the titration of the iodine liberated has been found not to be a good method for determining plutonium. Trivalent plutonium solutions are stable in an inert atmosphere, but are oxidised fairly rapidly by atmospheric oxygen. In acid solution potassium permanganate oxidises them quantitatively to the tetravalent state at room temperature, whilst at 60° they are oxidised to the hexavalent state. In addition to hot permanganate, potassium dichromate, argentic salts (including sodium persulphate in the presence of silver ions), ceric salts, and hot bromate solutions containing nitric acid all oxidise tetravalent plutonium to the hexavalent state. The reverse change is brought about by ferrocyanide and by hydrogen peroxide.

Trivalent plutonium salts are bright blue in solution, the sulphate, chloride, and perchlorate being readily soluble in the presence of dilute acids. On addition of an excess of ammonia a dirty blue precipitate of $\text{Pu}(\text{OH})_3$, readily soluble in dilute acids, is obtained. As previously indicated PuF_3 is insoluble in dilute acids.

With the exception of $\text{Pu}(\text{NO}_3)_4$ which gives green solutions, the solutions of tetravalent plutonium salts are pale pink. The oxide PuO_2 obtained by igniting the nitrate, iodate, or hydroxide at $500\text{--}600^\circ$ is dark brown. When prepared by gentle ignition it is readily soluble in hot concentrated sulphuric or nitric acid, but if ignited to a bright red heat it dissolves only in potassium bisulphate at a dull red heat. The hydroxide $\text{Pu}(\text{OH})_4$ is formed as a pale green gelatinous precipitate on adding ammonia to a solution of one of the salts.

Addition of hydrogen peroxide to an acid solution of a tetravalent plutonium salt gives rise to a red-brown colour and a bulky green precipitate of doubtful composition deposits slowly. Precipitation of the plutonium is most complete at pH 3-4.5 and the presence of sulphate ion seems to favour the reaction. A precipitate of the same nature is obtained on adding hydrogen peroxide to solutions containing hexavalent plutonium. This precipitate,

washed free from hydrogen peroxide and titrated with potassium permanganate solution, has a reducing power at ordinary temperature corresponding with $1\frac{1}{2}$ atoms of peroxidic oxygen per atom of plutonium, oxidation to the hexavalent state occurring only on warming and requiring two-thirds of the amount of permanganate required in the first stage. It has been shown that when the precipitate is washed three times with water and dried to constant weight over silica gel at room temperature, it has a composition which may be written $\text{PuO}_3 \cdot \frac{1}{3}\text{SO}_4$. It is suggested that the dried peroxide may differ from the original precipitate, and it is compared with the case of thorium which yields Th_2O_7 (or $\text{Th}_2\text{O}_7 \cdot \text{SO}_4$) which decomposes into ThO_3 . The peroxide is quite different from uranium peroxide (peruranic acid) and is insoluble in sodium hydroxide and hence is presumably not a per-acid. It dissolves in concentrated nitric acid with evolution of oxygen to give a bright blue solution which, on warming, evolves more oxygen, the colour changing to the pale green of $\text{Pu}(\text{NO}_3)_4$.

Tetravalent plutonium yields a soluble sulphate, nitrate, and iodate and insoluble fluoride, phenylarsonate, and *m*-nitrobenzoate. As plutonium is not co-precipitated with zirconium phenylarsonate or *m*-nitrobenzoate from strongly reducing solutions, it is inferred that these salts of trivalent plutonium are soluble.

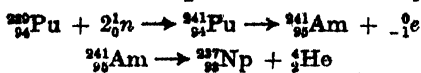
In the hexavalent state plutonium is amphoteric, yielding both plutonates, of which ammonium plutonate has been isolated as a brownish yellow precipitate, and plutonyl salts, of which $\text{PuO}_2(\text{NO}_3)_2$ yields solutions which are pink at low pH values and become more orange as the pH is increased. Plutonyl nitrate can be extracted with ethyl ether or nitromethane from its aqueous solutions saturated with ammonium nitrate. Plutonyl fluoride, unlike the lower fluorides, is soluble in the presence of an excess of hydrofluoric acid, this permitting the separation of plutonium from lanthanum.

In its various valency states plutonium also forms a series of complexes with inorganic ions and with organic compounds, many of which have been studied.

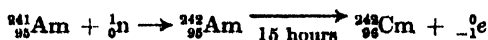
The presence of plutonium to the extent of one part in 10^{14} in pitchblende has also been reported. It was separated from this source in 1942 by Seaborg and Perlman, a feat which would have been impossible without the prior knowledge of its chemical properties obtained from the artificially produced material. It is supposed to be formed in the mineral through the spontaneous fission of ^{238}U yielding neutrons which bombard the ^{238}U .

Americium (Am) and Curium (Cm).—Isotopes of elements numbers 95 and 96 are reported by Seaborg as being formed on bom-

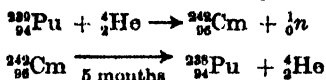
bombardment of ^{238}U and ^{239}Pu with 40 Mev. α -particles from a 60-inch cyclotron. The method of obtaining americium has not been published, but Emeléus suggests that it should be formed by successive neutron capture by ^{239}Pu , giving first ^{240}Pu and then ^{241}Pu , which by β -ray decay would give ^{241}Am . This isotope has a half-life period of 500 years and on α -particle emission yields ^{237}Np .



Neutron bombardment of ^{241}Am yields the isotope ^{242}Am , which decays rapidly by β -ray emission to give ^{242}Cm



This isotope was first produced by the (α, n) reaction on ^{239}Pu . It is an α -ray emitter of half-life 5 months yielding ^{239}Pu .



Curium has a second isotope ^{240}Cm which is again an α -ray emitter of half-life one month giving the lightest plutonium isotope ^{238}Pu .

THE "ACTINONS."—The study of the trans-uranic elements has had a fundamental effect on chemical theory in that it has confirmed what had previously been only a suspicion that in the last period of the periodic classification there is the beginning of a second series of elements analogous to the rare earth series. The full electronic structure of actinium is generally written $1s^2, 2s^2p^6, 3s^2p^6d^{10}, 4s^2p^6d^{10}f^{14}, 5s^2p^6d^{10}, 6s^2p^6d, 7s^2$. Its structure is therefore closely similar to that of lanthanum, but with an extra $s^2p^6d^{10}f^{14}$ electron group. One might anticipate, therefore, that actinium would be succeeded by elements in which the $5f$ levels begin to be filled instead of the $6d$ levels.

So long as the only known elements succeeding actinium were thorium, protoactinium, and uranium the matter was rather uncertain as these elements do actually resemble hafnium, tantalum, and tungsten in Groups IVa, Va, and VIa respectively, comparatively closely. The chemistry of protoactinium is not completely known, whilst there are some anomalies, particularly in the case of uranium, which one would expect on the basis of an external $6s^2p^6d^4, 7s^2$ structure to be almost exclusively hexavalent, whereas lower valency compounds are well known, and, indeed, UO_2 loses oxygen on heating to give U_2O_3 and perhaps U_3O_8 and is fairly easily reduced to UO_2 .

When one comes to consider the trans-uranic elements, however, neptunium, plutonium, americium, and curium should resemble

rhenum; osmium, iridium, and platinum, respectively. Hence neptunium should be almost exclusively heptavalent and plutonium should form a tetroxide and octahalides. No such compounds have been described, and, indeed, the most stable valency state tends to pass from higher to lower values as we pass up the series from uranium to plutonium.

The other interpretation is now more generally accepted, and by analogy with the term "lanthanides" for the rare-earth elements Seaborg has called this series the "actinides," although, owing to the ambiguity introduced through the suffix "-ide," and following recent usage in this country, they would be better called the "actinons."

One characteristic of the rare earth group is that throughout the series the most stable valency state is 3, but that other valency states of 2 or 4 are shown in some cases. In this connection it is interesting to compare the corresponding members of the lanthanon and actinon series (Table I).

TABLE I

No. of Electrons in Incomplete <i>f</i> Sub-group.	0	1	2	3	4	5	6	7	8
Lanthanon element .	La	Ce	Pr	Nd	Il	Sm	Eu	Gd	Tb
Valencies .	3	3, 4	3, 4, 5?	3, 4	3	2, 3	2, 2	3	3, 4
Actinon element .	Ac	Th	Pa	U	Np	Pu	Am	Cm	
Valencies .	3	4	5	3, 4, 5, 6	3, 4, 5, 6	3, 4, 5, 6	2?, 3	3	

Viewed in this way, cerium, with one *f* electron, shows a valency of 4 as well as 3, but with the tetravalent state having strong oxidising properties, whereas its analogue, thorium, is exclusively tetravalent. This circumstance is attributable to the fact that less energy is required to remove a 5*f* electron than to remove a 4*f* electron, and hence the higher valency states would be expected to be more pronounced in the actinons than in the lanthanons (Emeléus). This will also account for the fact that, whereas praseodymium has valencies of 3 and 4, and a possible valency of 5, only pentavalent compounds of protoactinium have been described, whilst uranium, neptunium, and plutonium can all have valencies ranging from 3 to 6, in which up to three of the assumed 5*f* electrons are readily given up. This being the case it is evident that trivalent states of thorium and protoactinium should be scarcely capable of existence. With increasing atomic numbers the stability of the higher valency states decreases, plutonium being most stable in the tetravalent state, so curium will probably be found to be exclusively

trivalent like gadolinium in the lanthanon series, the stability of this structure arising from the fact that each of the f orbitals is singly occupied.

Seaborg suggested the name americium for element No. 95 after the Americas by analogy with europium, also with six f electrons in its incomplete sub-group, which was named after Europe. No. 96 he called curium after Pierre and Marie Curie, pioneers in the field of radioactivity, by analogy with gadolinium, the corresponding member of the lanthanon series, which was named after Gadolin, the investigator who first studied the rare earths.

THE HALF-LIFE PERIOD OF TRITIUM.—Since the review of the properties of tritium (*SCIENCE PROGRESS*, 1948, **36**, 101) was written, two short notes have appeared giving the results obtained by investigators who studied the rate of decay of tritium in connection with the atomic energy project (*Phys. Rev.*, 1947, **72**, 972). By collecting and measuring the ^3He produced by the β -ray decay of tritium, Novick found the value of its half-life period to be 12.1 ± 0.5 years, whilst, by following the decrease with time of the ion current from an ionisation chamber containing tritium and protium, Goldblatt, Robinson, and Spence found the half-life to be 10.7 ± 2 years. The value of 31 ± 8 years obtained by O'Neal and Goldhaber is therefore much too high. This does not alter vitally any of the conclusions discussed previously, excepting that it means that, from Eidinoff's observations, the maximum tritium atom fraction which can be present in ordinary water must be much less than one part in 2×10^{17} , instead of one part in 10^{17} , as previously reported.

BIOCHEMISTRY. By W. O. KERMAK, D.Sc., LL.D., F.R.S., Royal College of Physicians' Laboratory, Edinburgh.

SPECIFICITY OF THE ACTION OF CHEMICAL COMPOUNDS ON LIVING TISSUES.—In recent years much attention has been paid to the action of toxic agents on living tissue. The chief problems are concerned with the highly specific nature of the action, which is sometimes to be observed. Certain substances affect cells of one class and leave others practically untouched. The existence of this selective action is of the greatest importance for practical applications; for example, the functioning of general anaesthetics depends on the fact that compounds exist which interfere with the proper working of the cells of the nervous system, but do not seriously inhibit any of the mechanisms of the body indispensable for life; insecticides have a specific action on some vital mechanism of insect metabolism, but are relatively harmless to mammals; chemotherapeutic agents must specifically damage the parasites which

cause disease, but at the same time be tolerably innocuous to the host.

It is broadly true to say that the practical successes achieved in these fields in the past have been the result of empirical experimentation and lucky chances rather than of calculations founded on rational theory. Even when systematic research in this field has been based on some plausible idea, this has usually been little more than vague analogy and tentative generalisation. Many working hypotheses have been advanced and these have stimulated much experimental work, in consequence of which many products have become available as insecticides, chemotherapeutic agents, anaesthetics, synthetic oestrogens and so on. But, more often than not, the work has led to results inconsistent with the hypothesis on which it was based, and it must be concluded that the practical advances were lucky by-products of much painstaking experimentation rather than deliberately designed effects.

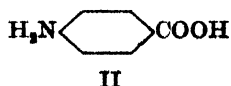
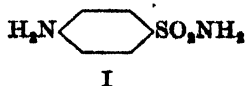
The essential problem is to understand the nature of specific as opposed to general toxicity. Such knowledge should enable us to devise specific toxic agents, deliberately designed to act on particular types of cells. It is perhaps scarcely to be expected that any simple recipe can be found applicable to all the very varied cases which present themselves. Sometimes we want a compound which acts specifically on some bacterium, but not on its human victim; sometimes one toxic to insects but not to other animals; sometimes one which picks out from a multicellular organism one variety of cell, for example tumour cells, and leaves all others intact. It may be that a type of specificity is called for even more subtle than this; we may require intracellular specificity in the sense that we want to inhibit temporarily one of the functions of the cell while avoiding any possibility of permanently damaging it. Anaesthetics and analgesics which killed or seriously injured the cells of the nervous system would be useless.

This leads us to the idea that what we really need are compounds which interfere with some particular chemical reaction within the cell, and apart from this one action have no effect. Such compounds might be specific, not only in the sense of inhibiting one type of cell activity; they might also totally incapacitate some cells and leave others unharmed, provided that the former were dependent for their healthy metabolism on the chemical reaction interfered with by the compound, whilst the latter were independent of it.

This idea of specific biochemical action was given definite form in the theory of Fildes and Woods, developed with reference to

antiseptic and chemotherapeutic agents. They suggested that such compounds interfered with "essential metabolites" within the cell of the micro-organism. Mercuric ions, for example, exerted their toxic action because they combined with the very important thiol, i.e. —SH, groups of the cell protein. If an alternative source of thiol groups were supplied, as for example by having thioglycollic acid present, the mercuric ions combined with these, the cell protein was thereby protected, and the mercuric ions lost their high toxicity.

This theory of Fildes and Wood was strongly recommended by the ease with which it accommodated the facts which came to light in the field of the then recently discovered sulphonamide drugs. It was shown by Woods (*Brit. Jour. Exp. Path.*, 1940, **21**, 74) that the compound present in yeast and other natural products which inhibited the bacteriostatic action of *p*-aminobenzenesulphonamide on streptococci was probably *p*-aminobenzoic acid. This was confirmed soon afterwards by the isolation of this compound from yeast by Rubbo and Gillespie (*Nature*, 1940, **146**, 838). One part of this amino-acid could antagonise the action of several hundred or even thousand parts of sulphanilamide on bacteria. The formulæ of these two compounds are remarkably similar (see I and II), and



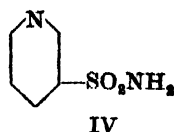
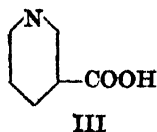
it was a natural suggestion that *p*-aminobenzoic acid was an "essential metabolite" of streptococci and other bacteria susceptible to the action of sulphanilamide, and that this drug exerted its action on these bacteria in virtue of this structural similarity. The idea was that the molecule of sulphanilamide, being very similar to one of *p*-aminobenzoic acid could, up to a point, take the place of the latter in some important biochemical mechanism within the bacterial cell, but that this power of proxy was strictly limited, and so the chemical mechanism jammed. The sulphanilamide molecules displaced those of *p*-aminobenzoic acid, but they were unable to function properly in their place, and in consequence the biochemical mechanism was deranged.

Much experimental work has been done with the object of testing this hypothesis as to the action of sulphanilamide on susceptible bacteria. The result has been broadly to confirm the hypothesis, though it appears that, at least with the more complicated "sulpha" drugs, such as sulphapyridine and sulphathiazole, biochemical effects other than interference with *p*-aminobenzoic acid metabolism are

involved. The implication of the theory, that *p*-aminobenzoic acid is an essential metabolite, is now generally accepted; this compound has come to be included as one of the vitamins of the B-group, although its significance for the higher mammalian organism is still obscure.

In the light of this work, a new method of approach to the problem of discovering novel chemotherapeutic agents suggests itself. The following plan seems promising: choose some known essential metabolite of the bacterial cell, for example one of the compounds needed for its growth. Many bacteria require the presence in the culture medium of small quantities of one or more of the "B" vitamins. Having selected a suitable growth factor, we prepare a compound whose structure closely resembles it. If we are lucky, we shall now have something which interferes within the bacterial cell with the chosen "essential metabolite" and prevents it performing its important function. The new compound may be expected to prevent the growth *in vitro* of micro-organisms whose metabolism critically depends on the selected compound, and if we are very lucky indeed, we may in this way hit on something which specifically affects a group of virulent micro-organisms, and is at the same time harmless to their human or animal hosts.

This line of thought has resulted in the publication of much interesting work, such as that of MacIlwain in this country and Woolley in America. Thus, selecting the anti-pellagra factor, nicotinic acid (III) MacIlwain (*Brit. Jour. Exp. Path.*, 1940, **21**, 136) prepared pyridine-3-sulphonamide (IV), and found that this com-



pound inhibited the growth *in vitro* of, for example, staphylococcus, the effect being antagonised by an excess of nicotinic acid in the medium. Similarly, certain micro-organisms, which require the anti-neuritic vitamin, thiamine, for their growth, are inhibited by the closely related pyrithiamine, an analogue in which the thiazole ring of the vitamin is replaced by pyridine. (Woolley and White, *J. Exp. Med.*, 1943, **78**, 489, and also Woolley, *Proc. Soc. Exp. Biol. Med.*, 1944, **55**, 179).

However, it is true to say that so far no really valuable phemo-therapeutic agent has been discovered by this new rational method. Perhaps the nearest approach to success is that recorded by MacIlwain and Hawking (*Lancet*, 1943, **1**, 449) who found that

pantoyltaurine (v), an analogue of the important vitamin, pantothenic acid, or pantoyl- β -alanine (vi), not only inhibited the growth



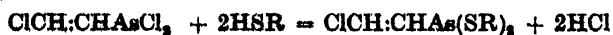
V



VI

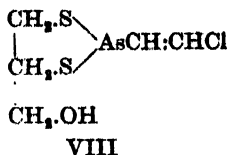
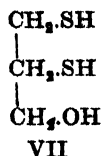
of hæmolytic streptococci *in vitro*, but prevented the death of rats inoculated with a lethal dose of these bacteria. But pantoyltaurine does not compete as a chemotherapeutic drug with the sulphonamides or with penicillin, both of which were discovered by chance rather than by rational design.

Interference with an essential metabolite may be regarded as a particular example of a whole class of interference phenomena, which in recent years have attracted much attention in biochemistry, pharmacology and chemotherapy. Another example is substrate competition for an enzyme. This refers to the inhibition of the action of an enzyme on its substrate by the presence of a compound closely resembling the substrate in its molecular architecture. The idea is that molecules of this similar compound can take the place of the molecules of the proper substrate on the active surface of the enzyme; however, they fail to undergo the reaction normally brought about by the enzyme, and so they remain blocking up the active centres, and preventing the approach of the proper substrate molecules (cf. SCIENCE PROGRESS, 1933, 110, 290). As another example of interference taken from a different field, we may refer to the action of B.A.L. (British Anti-Lewisite) in protecting the tissue of the body against lewisite and other trivalent arsenicals (see Peters, Stocken and Thomson, *Nature*, 1945, 156, 616). There is reason to believe that lewisite, ClCH:CHAsCl_2 , reacts with the tissues according to the equation:



where R represents the tissue protein carrying the important —SH groups. Now this reaction might be interfered with if we could present the arsenical with something with which it could combine more easily and more stably than it does with these tissue —SH groups. During the war the required compound was found, as the result of intensive research, in the form of 2:3-dithiolpropanol (vii). Compounds with a single —SH group, such as thioglycollic acid, were found to be ineffective, apparently because their union with the arsenical was too weak, and too easily reversible. The dithiol

compound, commonly known as B.A.L., forms a very stable ring containing arsenic with lewisite (VIII), and so very efficiently pre-



vents the latter attacking the tissue —SH groups. In fact, it was found that B.A.L., applied to lewisite wounds already developing could under favourable conditions actually reverse the course of the pathological process and bring about rapid recovery. This means that it is able actually to set the tissue —SH groups free from their union with the arsenical, and render the latter innocuous by combining stably with it. Though B.A.L. was discovered as a result of war research, it turns out to have important therapeutic uses in peace-time. As is well known, arsenicals are used for the cure of various diseases caused by spirochaetes and trypanosomes, and arsenical treatment sometimes gives rise to serious toxic effects, particularly in the form of arsenical dermatitis. This was a very intractable condition, but good results have been obtained by treatment with B.A.L. This compound, too, is able to combine with certain heavy metals other than arsenic, and it has been found very useful in some types of heavy metal poisoning. For example, already lives have been saved by the use of B.A.L. in cases of mercuric chloride poisoning, and toxic effects caused by gold treatment of rheumatoid arthritis have been alleviated by its use.

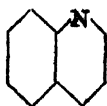
It is convenient to group together these and related phenomena in all of which there appears the common factor of the competition of two chemically similar compounds, either from combination with a third, or for a place in some chemical reaction or chain of reactions. We may conveniently speak of "chemical competition" and regard the examples we have cited above, interference with essential metabolites, enzyme competition, antitoxic action of B.A.L., as special examples of this more general phenomenon. Much of the modern work in the wide field which is concerned with the action of chemical compounds on living tissues plays variations on this basic theme of "chemical competition." An interesting example is presented by some recent work on the anti-fungal and anti-bacterial action of 8-hydroxyquinoline.

ACTION OF 8-HYDROXYQUINOLINE ON FUNGI AND BACTERIA.—8-Hydroxyquinoline (IX), commonly found in commerce as "chinosol" and conveniently known as oxine, was introduced as an

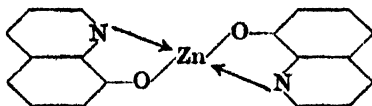
antiseptic some fifty years ago, and, in fact, on certain bacteria it exerts a powerful bacteriostatic action. For example in broth, at pH 7.3, it stops the growth of *Cl. Welchii* at a concentration of M/100,000. Besides being bacteriostatic, it inhibits the growth of many fungi.

More recently, oxine has come into prominence as a reagent for the detection and estimation of certain metals, with which it forms highly insoluble compounds. The efficiency of oxine as a reagent for zinc, for instance, is shown by the fact that it gives a precipitate with 1 gamma of Zn ions in 1 ml. of water. The chemical process involved is the formation of chelate compounds between the metal and oxine. This means that the metal ion forms a salt with the phenolic hydroxyl group in the usual way by replacing the hydrogen atom; a five-membered "chelate" ring is then formed by the nitrogen atom of the oxine sharing its two free electrons with the metal ion. Zinc, being a divalent ion, forms a primary simple salt with two molecules of oxine, and each nitrogen then forms its dative or co-ordinate link, so that the final complex molecule has the structure as represented in (x).

OH



IX



X

Oxine, then, possesses two rather remarkable properties: a surprisingly strong and rather specific antiseptic action, and the power to form insoluble chelate complex compounds with various metals. It is not surprising that the idea should have been put forward that these two properties are not unrelated, and that the first is, in fact, a consequence of the second. Suggestions in this sense seem to have been made about the same time by Albert (*Med. J. Aust.*, 1944, 1, 245) and by Zentmyer (*Science*, 1944, 100, 294). The idea is that in the cell of the micro-organism oxine will compete for some metal present probably only in traces, but essential for the proper functioning of the cell's biochemical mechanism. Such a metal might well be necessary for the activity of some important enzyme system.

Zentmyer (*loc. cit.*), who was interested in the fungistatic action of oxine, produced experimental evidence in support of this theory. He showed that its inhibiting effect on the growth of various fungi (*Fusarium oxysporum*, *F. lycopersici*, *Ceracostomella ulmi* and *Penicillium sp.*) was very much reduced if the pH of the medium was less

than 3.5. At these low pH values, the chelating power of oxine is also very weak. He also demonstrated that the action on the fungi (*F. oxysporum* and *C. ulmi*) was antagonised by the presence of zinc in the broth. With 0.5 mg. of Zn per litre of medium, *F. oxysporum* was prevented from growing by 30 mg. per litre of oxine; in presence of 5 mg. of Zn per litre, growth was prevented by 60 but not by 30 mg. of the compound. On such evidence, Zentmyer suggested that the fungistatic action of oxine depended on its ability to remove small traces of zinc from the fungal tissues by forming chelate compounds with the metal ions.

In a recent publication (*Brit. Jour. Exp. Path.*, 1947, 28, 69), Albert, Rubbo, Goldacre and Balfour adduce further evidence in support of this view as to how oxine acts. They show, for example, that, of various hydroxyquinolines tested, only those with the hydroxy-group in the 8-position have outstanding bacteriostatic activity, and also that it is only when the hydroxy-group is in this position that chelate ring formation takes place. This fits in well with the proposed theory, and incidentally disposes of the suggestion that the antiseptic action of oxine is simply a special example of the general antiseptic properties of the phenols, assisted perhaps by the presence of the quinoline ring.

Besides oxine, other quinoline derivatives, containing a hydroxyl group in the 8-position, form chelate complexes with metal ions. Many of these were tested and found to exhibit antiseptic action. A distinction should be made between the compounds which form chelate derivatives highly insoluble in water and those which form derivatives with considerable water solubility; oxine-5-sulphonic acid is an example of the latter type. The water solubility will decrease the competitive power of the compound for the metal ions as compared with a compound which forms insoluble complexes. The latter, by passing out of solution with the metal ions attached, are protected from any reversal of the combination, and the metal ion has consequently little chance of being recaptured by the enzyme protein or other tissue structure which requires it for proper functioning. Such derivatives of 8-hydroxyquinoline which form soluble complexes have a correspondingly weak antiseptic action.

In considering the chelating and precipitating power of various organic compounds, only data which refer to conditions likely to exist within cells are relevant. Observations to be found in the literature often relate to very artificial conditions such as obtain when these chelating compounds are used in analytical procedures. These conditions may be strongly acid or alkaline, and the temperature may be anywhere between 0° and 100°. Albert and Gledhill

(*Biochem. J.*, 1947, **41**, 529) and Albert and Magrath (*ibid.*, p. 534) have therefore observed the chelating powers of various compounds with metals under standard conditions, (pH 7.3, and 37°). One of the interesting results of this work is to show that under these conditions none of the many reagents used precipitates calcium or magnesium from dilute solution (0.0001M), though several of them are good analytic reagents for detecting and estimating these metals under appropriate conditions.

The chelating agents examined include many which are not derivatives of 8-hydroxyquinoline. Though some of these under the standard conditions show themselves as powerful as oxine in chelating action, none of them has a strong, specific antibacterial action. This is interpreted as showing that the ability to capture the metal ions within the cell depends on properties other than simple chelating power; the general molecular architecture is evidently important. It may affect the ability of the compound to penetrate within the bacterial cell and to reach the spot where the metallic ion is located.

Oxine in general acts more strongly on gram-positive than on gram-negative organisms. For example, it inhibits the growth of *Cl. Welchii*, *Str. pyogenes* and *Staph. aureus* in broth at pH 7.3 when present in concentrations of M/100,000, whereas with most gram-negative organisms concentrations 10 to 100 times as great are required. The gram-negative organisms, *H. pertussis* and *N. meningitidis*, are exceptions; they are both inhibited when the oxine concentration is as low as M/200,000.

If oxine acts by chelating with important metal ions, we should expect its action to be antagonised specifically by the addition to the medium of small quantities of the metal involved. Albert, Rubbo, Goldacre and Balfour (*loc. cit.*) tested the effect of small concentrations of various metal ions in respect of their power to antagonise the bacteriostatic action of oxine. The most clear-cut effect was shown by the gram-positive organisms *Str. pyogenes* and *Staph. aureus*, which, as just explained, are highly sensitive to the drug. Here, cobaltous ions prevented the oxine action very specifically. This result suggests that cobalt is a very important trace metal in the metabolism of these gram-positive organisms, and that it is by the removal of cobaltous ions that oxine exerts its bacteriostatic effects. With the gram-negative organisms, *B. coli* and *Proteus sp.*, reversal of the bacteriostatic action is brought about by zinc and iron; in the case of *B. coli* by copper as well. Here, the interpretation is more obscure; the much higher concentration of oxine required to act on these gram-negative organisms suggests

that its action on them is less specific and may involve a more complicated mechanism than simple competition for a single trace metal.

This recent work on oxine, like that on B.A.L., illustrates how the general idea of chemical competition may be of great use in biochemistry, and in a suitably particularised form may provide a working hypothesis for correlating observations and suggesting new experiments. The results obtained with B.A.L. have already borne practical fruit; the work on oxine is still in progress, but already clinical applications of the facts brought to light during the investigation are being considered. In particular, the low mammalian toxicity of 5-methyl oxine seems to justify the exploration of the chemotherapeutic possibilities of this compound. The results will be awaited with interest.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

VOLCANOLOGY.—A stimulating survey of the science of volcanology during the half-century ending in 1938 has been made by Howel Williams (Chap. in: "Geology, 1888-1938, 50th Anniversary Volume," *Geol. Soc. Amer.*, June 1941, pp. 367-90).

Another active worker in the field of volcanology is A. Rittmann, who has published a book (*Die Vulkane und ihre Tätigkeit*, Stuttgart, 1936) recently revised and translated into Italian (*Vulcani: Attività e Genesi*, Napoli, 1944). A summary of his views is given in a brief statement, "Mechanismus und Systematik der vulkanischen Tätigkeit" (*Geol. en Mijnb.*, Amsterdam, Jaarg. 1, No. 5, 1939, 122-7). Rittmann is a thorough-going advocate of gas action in volcanicity. He says that the most powerful intra-magmatic source of energy in volcanism is the gas which has been dissolved in the magma "since pre-geological times." To him the essential factor is the degasification and retrograde boiling of magma as it rises in the crust. Rittmann rejects the nomenclature for types of volcanic action embodied in the terms Strombolean, Hawaiian, Vesuvian, etc., and classifies volcanic action into seven types depending on the violence of gas discharge, proportions of materials produced (ash, lava), duration and permanence or otherwise of the activity.

The long and valuable article by L. C. Graton, "Conjectures regarding Volcanic Heat" (*Amer. Journ. Sci.*, 243-A, Daly Volume, 1945, 135-259), although so modestly entitled, is really a penetrating study and quantitative appraisal of the importance of heat and the volatile components of magma—the evanescent factors—in

volcanism. In the past gas has been accorded the major role in volcanism (see previous paragraph), but on the basis of his investigations Graton stigmatises gas theories of volcanic action as exaggerated and mystical. Exorbitant proportions of gas to molten matter, eruption without participation of molten lava, and the process of gas-fluxing, are also unacceptable. On the contrary, heat is recognised as the supreme factor in volcanism, since it furnishes the energy that is the very essence of volcanic action. As possible sources of volcanic heat radioactivity and exothermic chemical reactions are regarded as inadequate, and one has to fall back on the residual internal heat of the globe, as of old.

Graton's method of study is the application of straightforward physical and thermodynamic principles to a simplified concept of the volcano and its processes, and attempting to sample and evaluate the complex system at various stages from the focus to the surface. He starts with molten magma of granitic composition, with 9.4 per cent. of dissolved volatiles, at a depth of 40 km. and a temperature of 1200° C. The calculated falls of temperature on the way to the conduit are found to be much larger than often supposed; the ascending gas carries only a small fraction of the total sensible heat, and is kept hot only by constantly robbing the enclosing magma of heat. Volcanic gas is thus regarded, not as a heating agent, but as a powerful refrigerant.

The considerations put forward in this memoir on the relations of magma, gas and heat in igneous action have a bearing on the hypotheses of granitisation which have been so much in vogue of late. Another point which emerges from this study is that the sources of primary magmas and of volatiles are more deep-seated than usually supposed.

J. Verhoogen, in a paper on "Volcanic Heat" (*Amer. Journ. Sci.*, 244, 1946, 745-71), distinguished alike for its mastery of mathematics and its difficulty of reading, is critical of Graton's attempt to solve the problem of volcanism "in terms of a single magma endowed with more or less standard properties, which may well be a creation of petrologists rather than of Nature." Verhoogen points out that volcanic activity is: (1) A small-scale, almost insignificant phenomenon when compared to the earth as a whole, and (2) a process that is extremely diversified, and more or less random, both in space and time.

For example, the water content of magmas varies between wide limits, rare elements are distributed most unevenly, magmas appear to originate at various levels in the crust, and the distribution of volcanoes is quite irregular. The author believes that this very

randomness and diversity of volcanic and magmatic action is one of its most striking features. He presents a theory of "statistical melting," based on possible local chance variations of various physical properties, giving rise to "flaws" in a potential magma-layer, wherefrom magmas might be generated and begin to spread.

Verhoogen also suggests that there may be extensive diffusion of water and other volatiles from the deep interior of the earth, which might be responsible for much volcanic, magmatic, and metallogenic activity towards the surface. He states that he puts forward these suggestions merely as starting points for investigation. He concludes that "volcanic activity, a rather insignificant phenomenon, results probably from the simultaneous operation of a number of factors."

The Hawaii Division of Hydrography has produced of late years a number of magnificently illustrated memoirs, of which the three listed below are the most recent:

H. T. Stearns and G. A. Macdonald, "Geology and Ground-Water Resources of the Island of Maui, Hawaii," *Hawaii Div. Hydrography, Bull.* 7, 1942, 344 pp.

H. T. Stearns and G. A. Macdonald, "Geology and Ground-Water Resources of the Island of Hawaii," *ibid.*, *Bull.* 9, 1946, 363 pp.

H. T. Stearns, "Geology of the Hawaiian Islands," *ibid.*, *Bull.* 8, 1946, 106 pp.

These memoirs are an inexhaustible reservoir of volcanological facts, and provide illumination on the origin of lava formations, the structures and features of lavas in flows and vents of past geological times, which have been difficult of explanation. The last-named memoir gives a synopsis of present knowledge regarding the geology of each island, as well as tables of the rock formations, and geological maps. "The volcanoes pass through four major phases between birth and extinction and are built around one minor and two major rift zones. The volcanoes began their history above sea level in the Tertiary. Most of them became dormant either before or during the early Quaternary. Activity was renewed in the late Quaternary. Mauna Kea was glaciated in the late Pleistocene." In addition, a brief summary of the geography, climate, and geomorphology is given. It is impossible to overpraise the quality of the plates and the elucidatory diagrams.

The other two memoirs are detailed studies of Maui and Hawaii. In the general memoir and the Hawaii memoir there are extraordinary pictures of fissure eruptions in progress, which should dispel the last remaining doubts of the reality of this mode of erup-

tion and of its efficacy in building up piles of "flood basalts." There is much material of great value to volcanologists, particularly in the Hawaii volume, including discussions of craters, calderas and domes, the classification of explosive activities, the thickness of flows, the features of lava-lakes, rift-zones and dikes. The geology of Mauna Loa, incomparably the greatest volcano in the world; and of Kilauea on its flank, the most accessible and amenable of volcanoes, with its lava-pits, is given in much detail.

G. A. Macdonald gives a detailed account of "The 1942 Eruption of Mauna Loa, Hawaii" (*Amer. Journ. Sci.*, **241**, 1943, 241-56). On April 26 the volcano erupted in and near the summit caldera. Two days later lava broke out along the north-east rift at 9200 ft. on the flank, and for the first few hours a nearly continuous curtain of liquid lava fountains played along a fissure half a mile to a mile in length. As the activity declined it was localised at points and gave rise to cinder and spatter cones. A quiet issue of lava lower down the slope was probably fed by a tube in older lava. The main lava flowed for 16 miles towards Hilo, and was bombed to produce a diversion but without success. The eruption ended on May 10.

J. Verhoogen describes "Les volcans Virunga, et l'éruption du Nyamagira de 1938 (Note préliminaire)" (*Annal. Soc. Geol. Belgique*, **LXII**, 1939, 326-55), and "New Data on Volcanic Gases: The 1938 Eruption of Nyamagira" (*Amer. Journ. Sci.*, **237**, 1939, 656-72), which he regards as one of the grandest geological events of our epoch. The volume of basaltic lava erupted within a year was of the order of 1000 million cubic metres. The eruption was of Hawaiian type but presented certain novel features, notably the displacement *en bloc* of the apparatus of eruption, including the lava lake, some distance from the principal crater. Observations made over a stay of several months throw doubt on the fundamental role attributed by some authors to gas. The emission of gas at Nyamagira is feeble and its thermal action negligible, which facts support Graton's view (see p. 293).

The "Bulletin of the East Indian Volcanological Survey for the Year 1941 (Bulls. 95-8)" (*War Emergency Edit.*, 1943, 110 pp.) was written and published by R. W. van Bemmelen while in captivity under the rigours of the Japanese occupation of Java. The circumstances of its production are detailed in a review by G. W. Tyrrell (*Geol. Mag.*, **LXXXIV**, 1947, 110-13).

The Bulletin is in two parts: General and Special. The general part contains; a Register of the Localities of Volcanic Activity in the East Indian Archipelago, and a Preliminary Historical

Register of Volcanic Activity in the East Indian Archipelago (A.D. 1000-1941), in which 1952 eruptions are listed. The special part records observations at a number of volcanoes during 1941, mainly in Java, and deals with the geology of some of them in detail. Many new chemical analyses of the lavas are recorded, although little petrographical detail is given. The volume is full of valuable new volcanological facts collected under the most adverse circumstances.

A summary of the volcanic history of the Hakone volcano is given by H. Kuno (*Proc. Sixth Pacific Sci. Congr.*, 1939, 875-9). It is situated in central Japan two hours by rail from Tokyo, and is, with the adjacent Izu Peninsula, one of the most complicated centres of Tertiary volcanism in the Japanese Islands. Hakone consists of two sommas and seven central cones. Its outstanding feature is a caldera about 11 km. in diameter in the centre of a former conical volcano. This caldera is surrounded by the outer and older somma, and is concentric with an inner somma at a lower level, which is intact only on the eastern side of the complex. The central cones are arranged within the caldera along a north-western to south-eastern line, and one of them, Kamiyama (1438.5 m.), forms the highest point of this volcanic complex.

"The 1945 Eruption of Ruapehu (New Zealand)," described by C. A. Cotton (*Geogr. Journ.*, CVII, 1946, 140-3), began early in 1945, reached a maximum of intensity in the latter half of the year and then declined. From March 1945 to January 1946 the eruption passed through five phases: a steaming phase (from a crater lake); a tholoid (emergent dome) phase; a lava phase; an ash phase; and finally, a solfataric phase. The dome of lava grew until it occupied the whole of the crater and progressively displaced the lake, the water of which escaped by a subterranean spillway.

A good deal of valuable work has recently been done in the United States on the surface and structural features of lava flows, which will help to elucidate certain unexplained phenomena in ancient volcanic formations. Thus R. L. Nichols describes "Flow-Units in Basalt" (*Journ. Geol.*, XLIV, 1936, 617-30) as tongue-shaped structures from 150 to 300 ft. long, and 10-20 ft. in thickness, within a larger lava flow. The simplest flow mechanism is one in which the lava moves as a single unit; but flow-units evidently indicate a greater complexity of flow in some. Tongues of lava break through the crust of the main flow at the front and laterally. Crusts form on them in turn, and they are buried by later tongues. Thus the original unitary flow becomes multiple.

This observation in the McCarty basalt flow (New Mexico)

helps to explain the entry and disappearance of small subsidiary scarps in ancient lava formations which give rise to "trap-featuring," as in the Carboniferous and Tertiary lava fields of Scotland.

At the same time viscous lava may be extruded through vents and cracks up to the surface of a solidifying flow. These are called "squeeze-ups" (R. L. Nichols, *Journ. Geol.*, XLVII, 1939, 421-5); they may be classed as bulbous or linear types. Squeeze-ups are probably formed after the crust of the flow has attained a thickness sufficient to sustain open cracks. Many are hollow owing to drainage of lava from the interior after the crust of the squeeze-up has hardened.

R. L. Nichols also describes "Surficial Banding and Shark's-Tooth Projections in the Cracks of Basaltic Lavas" (*Amer. Journ. Sci.*, 237, 1939, 188-94). The walls of vertical cracks in the top of the McCarty lava-flow (New Mexico) are veneered with bands of glassy lava alternating with bands of duller, less-glassy lava. The veneers are only about $\frac{1}{4}$ in. thick. The bands are parallel to the surface of the flow. They are believed to be due to successive refusions of the walls by hot gases emitted from below during the progressive deepening of the cracks. The "shark's-tooth" projections occur on both walls of the cracks and always point in the same direction. These projections and ridges are formed by the scraping of the walls against one another while the lava was still stiffly viscous.

R. L. Nichols has described and figured remarkable examples of "Grooved Lava" from the McCarty basalt flow (*Journ. Geol.*, XLVI, 1938, 601-14). These are ascribed to the breaking-up of the thin crust of the flow into cakes which were up-ended, capsized, and forced over other blocks while still comparatively soft. Wedge-shaped masses of lava with vertical grooving are formed by plastic material being pushed up into a widening medial crack with irregular sides, past which the lava was thrust.

Grooved lavas with shark's-tooth projections have also been found on the surfaces of many thin flows from the Big Craters in the Craters of the Moon Monument, Idaho (R. L. Nichols and C. E. Stearns, *Amer. Journ. Sci.*, 238, 1940, 22-31). These features are, however, ascribed to the falling or rolling of blocks, bombs and lapilli, on to the surfaces of molten flows.

Discussing the "Layering in Yellowstone Rhyolite," A. D. Howard (*Journ. Geol.*, XLVII, 1939, 658-66) states that it consists in both a wavy lamination and a coarse stratification. Iddings attributed the layering to the spreading under flow stresses of a heterogeneous lava, and the author presents evidence in support of

this suggestion. Late shearing in a highly viscous lava, together with contemporaneous crystallisation, may have accentuated the fluxional banding. The author also suggests that the crystallisation of the dense bands may have been induced by vapours which traversed the rock after consolidation.

Among the after-effects of the Katmai eruption of 1912 in Alaska were enormous landslides from Falling Mountain at the head of the Valley of Ten Thousand Smokes. C. N. Fenner, in describing these falls, states that the mountain was, at first, the site of intense fumarolic activity which afterwards diminished (*Amer. Journ. Sci.*, XXXV-A, 1938, 35-48). On examining the talus it was found that the siliceous andesite of which the mountain was composed was riddled with large and small cavities and channels, in which a profusion of tridymite crystals had been deposited, with smaller amounts of cristobalite and hæmatite. Fenner suggests that the solution channels and cavities, as well as the secondary minerals, were due to the action of the fumarolic gases.

Dr. E. S. Shepherd's report on "The Gases in Rocks and Some Related Problems" (*Amer. Journ. Sci.*, XXXV-A, 1938, 311-52) first reviews work on volcano gases and goes on to discuss gases extracted from lavas, and from a small number of plutonic rocks. The following inferences are made from the tabulated facts: (1) The distribution of the volatiles in rocks and lavas is largely fortuitous. (2) The composition of the primitive magmatic exhalations cannot yet be deduced because of uncertainties about unavoidable contamination suffered on the way to the surface. (3) Approximate figures can be given for the minimum volatile content of unaltered rocks and lavas, and some light may be thrown on questions of condensation, re-distillation and the unpredictable enrichment in one or more elements. (4) The chemistry of the volatile constituents shows the predominance of water, 80 per cent. or more, nearly complete oxidation of the gases, and the absence of notable amounts of hydrocarbons and rare gases.

For his Presidential Address to the Geological Society of America, Dr. A. L. Day chose to review "The Hot-spring Problem" (*Bull. Geol. Soc. Amer.*, 50, 1939, 317-36). This is a close-knit essay which is difficult to summarise briefly, but it ranges from the studies of Hague forty years ago in the Yellowstone Park, to the recent studies by Allen, Day, Fenner, and other workers; and ends with a discussion of the mechanism of geysers. This is an illuminating address full of valuable suggestions.

T. F. W. Barth has assembled many data, as well as his own observations, on the geology, behaviour, discharge, and temperature

distribution of Geysir in Iceland (*Amer. Journ. Sci.*, **238**, 1940, 381-407). An appendix deals with various theories of the periodic eruption of geysers, including special consideration of Thorkelsson's theory that a reduction in pressure caused by gases and steam as they rise in narrow conduits is the main factor in geyser action. Dr. Barth gives a valuable bibliography of Geysir and geyser action.

E. G. Zies discusses the "Temperatures of Volcanoes, Fumaroles and Hot Springs" in the book "Temperature: its Measurement and Control in Science and Industry" (*Amer. Inst. Physics*, 1941, 372-80). Fumarole temperatures in the Valley of Ten Thousand Smokes (Alaska) ranged up to 645° C., but in fumaroles located on lava flows up to 800° C. The temperature of the lava lake at Kilauea was measured "after heart-breaking attempts" by Perrett and Shepherd at 1050° C. The temperature of a moving lava-flow was measured with an optical pyrometer over a period of two months at between 1070° and 1185° C. Using the same type of pyrometer at the Nyamagira volcano, Verhoogen measured lava temperatures of from 1039° to 1160° C.—a very satisfactory agreement (see p. 295).

In his comprehensive memoir on "Calderas and their Origin," Howel Williams (*Univ. of Cal. Publ., Bull. Dept. Geol. Sci.*, **25**, No. 6, 1941, 239-346) defines calderas as large volcanic depressions, more or less circular or cirque-like in form, the diameters of which are many times greater than those of the included vents. He classifies calderas into six main groups: (1) Explosion calderas, rare and small (e.g. Tarawera, Bandai-San); (2) collapse calderas, due to the withdrawal of magmatic support at the roots of the volcanoes. Five sub-types are described: Krakatau, Kilauea, Katmai, crypto-volcanic and Glen Coe. In addition, there are certain miscellaneous collapse calderas; (3) erosion calderas, enlarged by epigene agencies (e.g. Tahiti); (4) volcanic graben, straight-walled depressions; (5) volcanic rents or fissure-troughs, i.e. the severance of volcanic cones by more or less horizontal sliding movements (Java); (6) major volcano-tectonic depressions (N.Z., Sumatra).

Professor Williams regards the withdrawal of magmatic support in depth as the fundamental cause of nearly all calderas. Withdrawal of support results from: (1) loss of gas and contraction of magma on cooling (crypto-volcanic); (2) Rapid outflow of lava from fissures far below the summit vents (Kilauean calderas); (3) Rapid eruption of great volumes of new magma as pumice and ash (Krakatau type). Calderas are also formed by ring-fracture stoping (Scotland and New England), and by changes of shape and

volume of magma bodies in depth, especially by the injection of dike-swarms.

A detailed study of many dikes in the Pacific North West [of the United States], ranging in composition from basalt and diabase to aplite and pegmatite, shows evidence for two contrasting mechanisms of dike formation: (1) dilation concomitant with magmatic injection, and (2) replacement (G. E. Goodspeed, *Journ. Geol.*, XLVIII, 1940, 175-95). Numerous criteria for the recognition of both types are given. For the replacement type transitional facies are emphasised, but in their final stages the definite characteristics of an earlier phase may be partly or wholly obliterated. The implications of simple magmatic injection are very different from those of the complex processes of metasomatism.

PEDOLOGY. By G. V. JACKS, M.A., Commonwealth Bureau of Soil Science, Rothamsted Experimental Station, Harpenden.

THE FORMATION OF HUMUS.—While plants are the primary source of nearly all humus, it is now recognised that, besides the excreta of higher animals, the excreta and dead bodies of micro-organisms are important secondary sources of humic substances in the soil, and may play a specific and extremely important role in the soil system. What proportion of the total humus consists of microbial body substance is not known. It has, however, been established that microbial substance, when humified, is very resistant to further microbial decomposition. In course of time, therefore, a large proportion of soil humus may come to consist of substances derived directly from the bodies of micro-organisms. The possible extent to which micro-organisms contribute to the total mass of soil organic matter is shown by the work of N. A. Krasilnikov (*Mikrobiologia*, 1944, 13, 144-6), who estimated the mass of bacteria in the top 25 cm. of a soil carrying three-years-old lucerne in Central Asia to be 6000 kg. per hectare, in a Moscow podzol carrying three-years-old clover 900 kg., and in the same soil under wheat 6 kg. (within the root zone).

I. V. Tiurin (*Pedology*, 1946, 11-30) found that the percentage of total humus present as living organisms varied from 0.16 in a chernozem to 0.6 in a serozem. The percentage was inversely related to the humus content of the soil. This small living fraction is active in the soil mainly in controlling the fixation and mobilisation of the plant nutrients in the region of the rhizosphere. Tiurin's estimates are very much lower than Krasilnikov's.

Much work has been done in the past with the object of tracing the origin of humus to one or more specific groups of plant consti-

tments. Analogy was drawn between the formation and properties of "natural" humic acid and of the black, acid substances formed by treating sugars with strong acids, or by interaction between sugars and amino-acids. Subsequently the view swung round to the belief that it was not those substances (carbohydrates and protein) which were most susceptible to microbial attack that were likely to be the precursors of humus, but those which were most resistant, especially lignin. Evidence of the close relationship between lignin and humic acid has been provided by X-ray studies (I. D. Sedletsky and B. Brunovsky, *Compt. Rend. Acad. Sci. (URSS)*, 1935, 4, 279-81) which have indicated very similar molecular structures. H. Pallmann (*Kolloid-Ztschr.*, 1942, 101, 72-81), following Sedletsky and Brunovsky, believes that humic acid is a slightly crystalline complex intermediate between non-crystalline lignin and crystalline graphitic acid. Graphitic acid has a layer-lattice structure which is only slightly developed in humic acid and is missing in lignin. Amino-acids and peptides can enter between the graphitic-acid layers, maximum sorption occurring at pH 6. H. Hamdi (*Kolloid Beih.*, 1943, 54, 554-634) obtained physico-chemical data indicating a close similarity in the benzene-ring structure of lignin, humic acid and graphitic acid, but considerable differences in detailed structure due to different degrees of oxidation.

I. D. Sedletsky (*Probl. Sovet. Pochvoved.*, 1936, 1, 125-34) showed that the humic acids obtained from coal, peats and soils gave identical X-ray diagrams, and concluded that they were identical in constitution. N. I. Gorbunov (*Pedology*, 1947, 240-48) could find no signs of crystal structure in X-ray diagrams of humic acid from peat and freshly prepared sodium and calcium humates. Debye rings were apparent only in samples containing more than 2 per cent. of mineral salts. The diffuse rings sometimes obtained with humic-acid preparations are caused by intramolecular diffraction. S. Mattson and E. Koutler-Andersson (*Lantbr. Högsk. Ann.*, 1944, 12, 20-100) obtained almost identical titration curves from a soil humus of the chernozem type and the product of the alkaline autoxidation of lignin. This is taken as strong evidence that the humic acidoids of the soil are formed by autoxidation of lignin.

W. S. Gillam (*Soil Sci.*, 1940, 49, 433-53), by methylating and acetylating lignins and humic acids, obtained evidence that the latter, but not the former, contained carboxyl groups. The methoxyl contents of the extracted humic acids were low (1.5 per cent.) in comparison with lignin (13-15 per cent.). Gillam assumed that humic acid was formed by combination between a slightly modified lignin complex and nitrogenous compounds of proteid nature.

Humic acids from prairie, chestnut and chernozem soils appeared to be very similar in properties and constitution.

In his book *Soil Organic Matter* (Leningrad, 1937), I. V. Tiurin regards the formation of humic acid from lignin as an enzymatic oxidation proceeding concurrently with the breakdown of lignocellulose into its components. He divides humus into (1) humic acids, (2) fulvic acids, (3) humins, (4) bitumen, fats and waxes. He considers that in soil humus humic and fulvic acids are united by an ester linkage resistant to acids but hydrolysed by alkalis, and that the humins which are not hydrolysable by acids or alkalis represent polymerised, ester-like products of humic and fulvic acids. Fulvic acids were shown to be nitrogenous complexes of equivalent weight about 300.

HUMUS AS A PRODUCT OF BACTERIAL AUTOLYSIS.—F. Y. Geltser (*Sovet. Agron.*, 1940, Nos. 11-12, 22-29; *Pedology*, 1943, Nos. 9-10, 62-72) identified the "active" fraction of humus—i.e. the fraction capable of uniting with the mineral soil to form stable aggregates—with the products of autolysis of the bodies of dead bacteria. She prepared such substances *in vitro*, and showed that they were resistant to further microbial decomposition and were capable of cementing clay particles into aggregates. Pointing out that the first processes of the decay of vegetation were fungal and took place in an acid medium, she assumed that these processes were superseded, especially in neutral, structured soils which are rich in active humus, by a predominantly bacterial process, in which the fungi are consumed by bacteria which subsequently die and autolyse, producing active humus. According to this theory, the effect of lime in promoting the formation of a soil structure is due more to its effect in changing the microbial population from a fungal to a bacterial type, thus leading to the second stage in the formation of active humus, than to its physico-chemical effect in flocculating dispersed soil colloids. In acid soils, e.g. podzols, the second stage is never reached, and a stable soil structure is not produced.

This theory is novel, and has certain attractions to the soil scientist. It should be noted, however, that no other worker has yet corroborated Geltser's findings.

Geltser suggested that active humus might be allied to the mucilages produced by the bacterial decomposition of cellulose and other carbohydrates. S. Winogradsky (*Ann. Inst. Pasteur*, 1929, 43, 549-633) came to the conclusion that cellulose-decomposing organisms were the most important producers of organic colloids in the soil. He also considered that these colloids, despite their colourless appearance, should be classed as humus, since (1) they

were soluble in dilute alkali from which they were precipitated by acid, (2) they were dispersible in water, (3) they contained about 2 per cent. of organic nitrogen (derived from the solution of autolysed bacterial bodies in the mucilage) and (4) they were resistant to microbial attack. Winogradsky saw no reason why there should not be colourless humus the presence of which is usually masked by the accompanying dark-coloured components. Cellulose-decomposing fungi did not produce any mucous matter, but a dark-coloured substance which, unlike the mucilage, was not a gel.

J. G. Shrikhande (*Biochem. Journ.*, 1933, 27, 1551-74) investigated the formation of mucilage from sterilised oat straw inoculated (1) with pure cultures of fungi, (2) with pure cultures of bacteria, (3) with fungi and 48 days later with the cellulose-decomposing bacterium *Spirochaeta cytophaga*, (4) simultaneously with fungi and bacteria. Although 30-40 per cent. of the straw was decomposed by fungi in 48 days, no mucilage was formed. A relatively small amount of mucilage was formed by the pure cultures of bacteria, but very much more by bacterial action following fungal action. Treatment (3) was easily the most effective in producing mucilage. The simultaneous inoculation with fungi and bacteria produced little mucilage, presumably because the presence of bacteria inhibited the fungal process. The formation of mucilage was an aerobic process, and more was produced the more rapid the decomposition.

Assuming, therefore, that the bacterial mucilages are related to Geltser's humus, Shrikhande's work lends some support to the hypothesis developed later by Geltser.

URONIC ACIDS IN SOILS.—These bacterial mucilages contain uronic-acid complexes, and an important development in the investigation of humus has been the demonstration of the presence in it of such complexes, sometimes in considerable abundance.

K. I. Rudakov (cited by M. M. Kononova, *Pedology*, 1947, p. 597) has advanced the hypothesis that the structure-forming "active humus" is a complex formed from nitrogenous products of the autolysis of bacterial bodies and uronic acids derived from plant-root secretions by the action of bacteria, which enter the plant tissues and decompose pestic substances.

E. C. Shorey and J. B. Martin (*J. Amer. Chem. Soc.*, 1930, 52, 4907-15) showed that from 5.15 to 9.25 per cent. of the total organic carbon in peats, and from 12.8 to 28.4 per cent. of that in soils, could be accounted for as uronic carbon. U. Springer (*Bodenk. PflErnähr.*, 1940, 18, 129-67) determined the uronic content of stable manure and peats of various origins. Two samples of the

manure contained from 13 to 14 per cent. of uronide, and five peat samples 6.5–9.8 per cent. In a study of the distribution of apparent uronic carbon in 20 soil profiles, A. G. Norman and R. P. Bartholomew (*Soil Sci.*, 1943, **56**, 143–50) found that about 10–15 per cent. of the organic carbon of surface soils seemed to be present in uronide groupings. The proportion increased with depth, particularly in podzolised soils in which a marked accumulation was found in the B horizon. In one soil sample 32 per cent. of the total carbon was in uronic form.

The apparent resistance to decomposition and consequent accumulation of uronide groupings cannot, as yet, be accounted for. The susceptibility of pure polyuronides, such as pectin and alginic acid, to microbial degradation is in marked contrast to the apparent resistance of the uronide groups in plant residues. The uronic content of decomposed residues is frequently no lower than that of the parent material. S. A. Waksman and H. W. Reuszer (*Soil Sci.*, 1932, **33**, 135–51) have shown that the uronic-acid complexes appear to be more resistant to rapid decomposition than cellulose or the pentose grouping of the hemicelluloses.

To account for this resistance, three theories have been advanced : (1) the soil uronic-acid complexes are, as such, more resistant to decomposition ; (2) the uronic-acid content is maintained by the formation of synthetic polyuronides by the microbial population ; (3) the uronic-acid complexes are stabilised by association with some other soil component (*e.g.* lignins). There is, however, no direct evidence of the presence of uronide groups in soils. Indirect evidence is provided by the fact that the carboxyl group of the uronic-acid molecule is the only one known to yield CO₂ on treatment with 12 per cent. HCl. No uronic acids have, as yet, been isolated from soils, although Shorey and Martin (*loc. cit.*) achieved the separation from soils of colloidal materials having all the properties of uronic complexes.

THE CLAY-HUMUS COMPLEX.—It has long been recognised that the separation of soil colloids into organic and inorganic components is a matter of convenience rather than of fact. A satisfactory technique for investigating the colloid complex as a whole has, however, yet to be devised, though it may be expected that X-ray studies will throw further light on the important question of the nature of the union between the inorganic component, or clay, and the organic component, or humus, of the soil colloids. Recent work has indicated that the nature of the union has an important bearing on the fertility of the soil.

A. F. Tiulin (*Soil Sci.*, 1938, **35**, 343–57) separated soil colloids

into two main groups differing in their peptisability by neutral salts. "Group I" colloids are peptised by saturated sodium-chloride solution, "Group II" colloids are not. Group I is electronegatively charged, Group II is iso-electric or weakly electronegative. Group I predominates in chernozems and is essential to the formation of a stable crumb structure, Group II forms practically all the colloids in podzols. Tiulin assumed that in Group I a very active form of organic matter with high nitrogen and phosphorus contents is adsorbed on the gel surface, whereas in Group II all the organic colloid is firmly bound in the interior of the gel particles in an inactive form. Group I has relatively high, and Group II relatively low, sesquioxide and organic-matter contents.

A. F. Tiulin and M. V. Dankova (*Sovet. Agron.*, 1946, No. 2, 39-45) postulate that the primary soil particles are enveloped in gels which may be predominantly of Group I or Group II. When flax was grown in sand cultures, addition of increasing quantities of soil in which the ratio of Group I to Group II gels was high increased the yields, but addition of soils with low ratios decreased the yields. The roots grown in soils with a high proportion of Group II colloids were coarser in structure, much less branched and less well provided with root hairs than those grown in soils rich in Group I colloids. H. J. Atkinson, R. C. Turner and A. Leahey (*Soil Sci.*, 1944, 57, 243-6) obtained highly significant relationships between yields of barley and clover on seven Canadian soils and the quantities of Group I colloids obtained from the soils. M. A. Vinokurov and A. V. Koloskova (*Pedology*, 1942, Nos. 5-6, 8-18) showed that the improvement in fertility of podzol soils by mixing the A and B₁ horizons is accompanied by an increase in the quantity of Group I colloids in the cultivated layer.

L. Meyer (*Forschungsdienst Sonderh.*, 1941, 17, 38-41) stated that of the clay minerals of the montmorillonite, mica and kaolinite groups only the first two can form stable clay-humus complexes. He suggested that the bonding might be due either to secondary valencies (this appears to be Tiulin's view), or else exchangeable calcium and magnesium ions might act as bridges (polar bonds) between the mineral and humus components. It was not possible, as some workers have asserted, to incorporate humic acid between the layers of the montmorillonite lattice.

D. Khan (*Dokl. Akad. S.-Kh. Nauk*, 1946, Nos. 1-2, 11-14) shook neutral solutions of ammonium humate with ground askanite, kaolin, orthoclase, microcline and mica. Askanite absorbed much more humic acid than the other minerals; kaolin absorbed least. The data obtained suggested that there were two types of absorption,

in one of which (kaolin) humic acid was absorbed on the crystal surface, and in the other (askanite) it entered the interior of the crystal.

E. Jung (*Bodenk. PflErnähr.*, 1943, 32, 325-36) shook potassium- or calcium-saturated montmorillonite powder with increasing amounts of sodium-humate sol, and found that all the humate added, up to 2 per cent. of the clay, was held by the clay, but larger amounts were not held at all. The limiting value was almost independent of the pH. The base-exchange capacity of the montmorillonite-humate complexes was equal to the sum of the exchange capacities of the components, but complexes formed from a soil colloid and the same humate showed a reduced exchange capacity. This reduction was attributed to the action of sesquioxides which Jung regarded as important agents in binding humates and inorganic colloids.

PLANT PHYSIOLOGY. By PROFESSOR WALTER STILES, Sc.D., F.R.S.,
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WATER RELATIONS OF PLANT CELLS.—For more than half a century the theory of the water relations of the plant cell developed by Pfeffer and De Vries was accepted almost without question by all botanists. On this theory, as is well known, the plant cell is regarded as a system in which a solution (the vacuole) is contained within a semi-permeable membrane (the protoplasm or its limiting layer) which is itself surrounded by a completely permeable and elastic envelope (the cell wall). The suction pressure, the pressure with which water is drawn into the cell from an external aqueous solution, is $P - P^1 - W$, where P and P^1 are the osmotic pressures of vacuole and external solution respectively and W the inwardly directed pressure of the stretched and elastic cell wall. When the osmotic pressure of the external solution is such as just to bring about incipient plasmolysis, W is zero and $P = P^1$. This simple relationship has formed the basis for many years of determining the osmotic pressure of cell saps by what is generally referred to as the plasmolytic method.

Now if this classical theory is correct the plasmolytic method should give the same value for the osmotic pressure of the cell sap as the cryoscopic method, in which the sap is expressed from the cell, the freezing point of the expressed sap then measured, and the osmotic pressure calculated from the observed depression of the freezing point. Some twelve years ago, however, T. A. Bennet-Clark, A. D. Greenwood and J. W. Barker ("*Water Relations and Osmotic Pressures of Plant Cells*," *New Phyt.*, 35, 277-91, 1936) found that this was not always so. They determined the osmotic

pressures of the cells of five different kinds of tissue by both methods and found that in three of them, beetroot, swede root and the petiole of *Begonia rex*, the values of the osmotic pressure found by the plasmolytic method were considerably higher than those calculated from the observed depression of the freezing points of the expressed sap. This means that in these particular tissues the osmotic pressure of the vacuole is considerably less than the actual pressure with which water is drawn into the cells. In the expression $P - P^1 - W$ for the pressure with which water is drawn into the cell the osmotic pressure of the cell P must, therefore, be replaced by A , the gross water absorption pressure. In the *Begonia rex* petiole, for example, the average value of the osmotic pressure was found to be 5.4, while the gross water absorption pressure was found to be 8.0. The conclusion appears inevitable that the classical theory of Pfeffer and De Vries does not provide a complete or adequate explanation of the water relations of the plant cell.

In a later paper T. A. Bennet-Clark and D. Bexon ("Water Relations of Plant Cells. III. The Respiration of Plasmolysed Tissues," *New Phyt.*, **42**, 65-92, 1943) offer an explanation of the movement of water into the cell in such tissues. The movement of water across a membrane from a solution to the pure solvent was observed by Dutrochet 120 years ago, and has formed the subject of enquiry of more than one investigator since. Such a movement of water, generally referred to as anomalous osmosis or electrosmosis, is regarded as related to electric charges on the surface of the membrane and in the walls of narrow channels in the membrane, which is assumed to have a porous structure. These charges can arise in various ways, as, for example, by either cation or anion being adsorbed to a greater extent than the other on the walls of the pores, or by either cation or anion having a greater mobility than the other. Where, for instance, there is a positive charge on the solution side of the membrane and the liquid in the capillaries has a negative charge, the movement of water from solvent to solution will be greater than could be accounted for by normal osmosis.

It has, moreover, been shown by W. J. V. Osterhout and J. W. Murray ("Behaviour of Water in Certain Heterogeneous Systems," *Journ. Gen. Physiol.*, **23**, 365-90, 1940) that a movement of water unrelated to differences in osmotic pressure can take place in systems which do not involve a membrane. In one of their experiments, guaiacol saturated with distilled water was placed in a U-tube and equal amounts of water saturated with guaiacol placed above the guaiacol in each arm. On their adding trichloroacetic acid to the

water in one arm, not only the acid, but water as well, passed through the guaiacol into the water in the other arm of the U-tube, the water thus passing from the solution with an osmotic pressure due to trichloroacetic acid to the water without the acid. The movement of water may be due to the formation of some loose association of water and acid. To this movement of water Osterhout and Murray have given the name "anaphoresis." It has this in common with electrosmosis that the movement of water is related to the movement of a dissolved substance.

Based on such considerations as these, Bennet-Clark and Bexon have suggested a possible mechanism to explain the gross water absorption pressure exceeding osmotic pressure. Their suggestion is that in respiration sugar contained in the vacuole is broken down with formation of malic acid, which then diffuses through the internal plasmatic membrane into the cytoplasm, where it is re-formed into sugar. There would then be a continuous movement of electrolyte through the inner plasmatic membrane. If this membrane carries a negative charge on its inner surface and a positive one on its outer side, while the liquid in the capillaries of the membrane carries a positive charge and the surface of the capillaries a negative one, positively charged liquid will move towards the negative or inner side of the membrane, so that there will be electrosmosis of water from the cytoplasm into the vacuole.

The movement of water into the cell would thus appear to be conditioned, at any rate in part, with anomalous osmosis, which is itself related to movement of solutes involved in metabolic processes. Similar conclusions regarding the relationship of water intake and metabolism have been reached by L. Brauner, M. Brauner and M. Hasman working in Istanbul. The first two of these workers have reported ("Further Studies of the Influence of Light upon the Water Intake and Output of Living Plant Cells," *New Phyt.*, **39**, 104-28, 1940) that exposure of disks of carrot tissue to light for periods ranging from 5 to 120 minutes brought about an increase in the rate of water absorption. The water loss in hypertonic glucose solutions was found to be uninfluenced by exposure to light. Weaker salt solutions (0.1N potassium chloride, 0.02N phosphate buffer) reduced the effect of light, the reduction being greater with higher concentration. Thus if the rate of water intake in the dark from pure water is taken as 100, the rate after 30 minutes' exposure to light was 198.3. This in 0.02N phosphate buffer was reduced to 104.2 and in 0.1N phosphate buffer to 93.2. L. and M. Brauner, like Bennet-Clark and Bexon, consider that electroosmotic effects, conditioned in part by the differential rates of movement of

cations and anions through the pores of the membrane, are involved in the interchange of water between cell and external medium. They suggest that light affects the relative mobilities of the two sorts of ions, and so by altering the diffusion potential resulting from the different mobilities of cation and anion affects the electrosmotic pressure. This would also explain why salts reduce the light effect, since, when the external medium is water, the movement of ions is from tissue to medium, whereas in a salt solution the chief movement of ions is in the opposite direction, with the result that the movement of water due to electrosmosis will be in the reverse direction, that is, from tissue to solution. The effect of light will thus also be in the reverse direction from that observed when the tissue is in water.

In a paper by L. Brauner, M. Brauner and M. Hasman ("The Relations between Water Intake and Oxybiosis in Living Plant Tissues," *Rev. Fac. Sci. Univ. Istanbul, Sér. B.*, 5, 266-309, 1940) evidence is brought forward to show that absorption of water by disks of potato tuber is related, at least in part, to metabolic processes. In the first place, whereas water absorption under aerobic conditions continues for many days, under anaerobic conditions water absorption, which from the start proceeds at a slower rate than in presence of oxygen, stops altogether after 24 hours. Under aerobic conditions water intake follows at first an approximately logarithmic course, after which the rate of intake remains more nearly constant. The first phase is interpreted as conditioned by physical factors, the second is regarded as depending at least partly on metabolic action. Incidentally this interpretation is in agreement with the observations of the present writer on the absorption of salts by storage tissue slices, where a biphasic course in the absorption is also observed, and where the two phases can be explained as determined by physical factors and metabolic action respectively.

The suction pressure of the tissue, after falling with absorption of water during the first day or two, remains approximately constant thereafter, although water is continuously absorbed. This may perhaps be explained by the fact that the concentration of hexose sugar in the tissues increases, presumably by starch hydrolysis, thus raising the osmotic pressure of the cells.

In a later paper L. Brauner and M. Brauner ("The Relations between Water Intake and Oxybiosis in Living Plant Tissues. II. The Tensility of the Cell wall," *Rev. Fac. Sci. Univ. Istanbul, Sér. B.*, 8, 30-75, 1943) measured the extensibility of plasmolysed and turgid cylinders of potato tuber and from the results obtained concluded that immersion of the tissue in water, under both aerobic and anaerobic conditions, renders the cell walls more plastic, but that

the effect is greater under aerobic than under anaerobic conditions. This, by reducing the wall pressure of the cells, would also tend to raise the level of the suction pressure.

In another paper by the same authors ("Studies in the Relations between Water Permeability and Electric Charge in Membrane Models and in Living Cells," *Rev. Fac. Sci. Univ. Istanbul, Sér. B*, **8**, 264-310, 1943) experiments with both non-living membranes and living cells are described, in which an attempt was made to evaluate the importance of electrical effects and filter effects in the passage of water into cells. Non-living membranes of cellophane, the testa of *Phaseolus multiflorus* and calf's bladder were used, while the living tissues employed were those of potato tuber, beet-root, the epidermis of onion bulb scales and the anthocyanin-containing epidermis of *Phlomis tuberosa*, a member of the Labiatae. To eliminate the diffusion potentials and electrosmotic effects due to them, the non-living membranes were placed in dilute buffer solutions covering a range of hydrogen-ion concentrations and the liquid forced through the membrane by hydraulic pressure. Under such conditions there appear to be two factors in the membrane which may determine the rate of flow of water through it, the size of the pores and the electric charges on the walls of the pores and the liquid passing through them. As regards the former, the swelling of cellophane membrane increases, though not greatly, with increasing pH, and it may be assumed that the area of the pores, that is, of intermicellar material, increases correspondingly. In regard to the charge on the walls of the pores, L. and M. Brauner hold that the micelles constituting the pore walls are negatively charged and that hydrogen-ions adsorbed to the walls will thus lower the charge, so that with increasing pH of the medium the electrostatic force of attraction between the micelles of the wall and the dipole molecules of water, which acts as a brake on the movement of water, will also increase.

The degree of swelling of the membrane in the various buffers and the rate of passage of the liquid through the membrane under unit pressure were determined, and it was found with membranes of cellophane and the testa of *Phaseolus* that, as the hydrogen-ion concentration increased, the resistance of the membranes to the passage of water decreased; that is, the permeability of the membranes to water increased. This result is interpreted as due mainly to a reduction of the electrostatic braking force to which the moving water is subjected; the variations in the total area of the pores should produce the reverse effect, and in any event these variations are not big enough in comparison with the total pore

area available to have much effect. With the calf's bladder membrane the state of affairs was found to be different. Here the swelling is much more influenced by pH, minimum swelling being at pH 4.6. But minimum permeability of the membrane was also observed at this point, and the greater the degree of swelling the greater was the permeability of the membrane to water. In this membrane, where the pores are small, it was therefore concluded that the size of the pores (the "mechanical filter effect") was more important than electrostatic forces in determining the flow of water.

The conclusions derived from these experiments with non-living membranes were applied to experiments carried out with living cells, in which the rate of water absorption by tissues immersed in buffer solutions of various hydrogen-ion concentrations was examined. The most rapid intake of water with both potato tuber and beetroot occurred at a pH of 6.0, which is probably the isoelectric point of the principal protein of the plasma membrane. The maximum permeability to water thus corresponding to a minimum charge of the membrane, the position resembles that of the cellophane membrane, in contrast to that of the calf's bladder, and so it is concluded that the permeability of the cells to water is largely regulated by the electric charge on the micellæ of the membrane. Similar results were obtained, and conclusions drawn, from experiments on the plasmolysis and deplasmolysis of the epidermal cells of the onion bulb scale and the stem of *Phlomis tuberosa*.

The problem of whether the suction pressure of plant tissue involves an electro-kinetic component has been examined for various tissues by L. Brauner and M. Hasman ("Untersuchungen über die anomale Komponente des esmotischen Potentials lebender Pflanzenzellen," *Rev. Fac. Sci. Univ. Istanbul, Sér. B*, **11**, 1-37, 1946). To this end the water absorption by carrot root tissue from sucrose, potassium sulphate and calcium chloride solutions of the same osmotic pressure (2 atmospheres) was measured. It was found that considerably less water was absorbed from the calcium chloride solution than from the others; similarly, the value for the suction pressure of the tissue when determined by means of a graded series of solutions of calcium chloride was 19 per cent. less than the value obtained when potassium sulphate was used. With potato the value for the suction pressure obtained with potassium chloride was nearly the same as that obtained with sucrose, but the value when potassium sulphate was used was about 3.7 per cent. lower and that with calcium chloride about 9 per cent. lower.

These results are attributed to the effect of the ions present in

the medium on the electric charges of the cell membranes, and which control the electrosmotic movement of water. The adsorption of ions by the boundary layers would alter the magnitude of the electrostatic forces and so the electrosmotic pressure. Assuming that the reduction of suction pressure by calcium chloride is brought about in this way Brauner and Hasman by a system of extrapolation estimate that the membranes would be completely discharged in a 0.25M solution and that this concentration of calcium chloride would lower the suction pressure, as exhibited in pure sucrose solution, by about 10 per cent. They therefore conclude that the fraction of the suction pressure of potato tuber tissue attributable to electrosmosis is about 10 per cent. of the whole. The increase in rate of water absorption by carrot tissue in dilute solutions of glucose, lithium chloride and potassium sulphate, as compared with that in distilled water, can also be accounted for by electrosmosis. The rate of absorption of water by this tissue in a calcium chloride solution as dilute as 0.001M was, however, less than from distilled water.

Further work on the same subject by Brauner and Hasman has been reported in a contribution ("Weitere Untersuchungen über die anomale Komponente des osmotischen Potentials lebender Pflanzenzellen," *Rev. Fac. Sci. Univ. Istanbul, Sér. B*, **12**, 210-54, 1947). They report that, when cells of *Spirogyra neglecta* are plasmolysed with solutions of sucrose and magnesium chloride of the same osmotic value, the contraction of the protoplast which occurs in the magnesium chloride is greater than that in the sucrose. The course of contraction of the cells indicates that there is no appreciable entry of solute into the vacuole. The final osmotic concentration of cells plasmolysed in magnesium chloride must thus be higher than that of the cells plasmolysed in sucrose, although both are in equilibrium with external solutions exerting the same osmotic pressure. If it is assumed that the magnesium chloride in the concentration employed has eliminated the electrical charges of the cell membranes and so the electrosmotic component of the osmotic pressure exerted by the living cells, it follows that the difference between the final osmotic values of the cells plasmolysed in sucrose and magnesium chloride is due to electrosmosis. Measurements made with tissue that had absorbed water for long periods indicate that electrosmosis increases with prolonged water absorption under aerobic conditions, although the total suction pressure has decreased from its original value. This observation affords further evidence of the connection between anomalous osmosis and metabolic action.

For many years a usual method of determining the suction pressure of a tissue has been to find that solution of sucrose in which a piece of the tissue undergoes no change in weight. The osmotic pressure of that solution should equal the suction pressure of the tissue. E. Ashby and R. Wolf ("A Critical Examination of the Gravimetric Method of Determining Suction Force," *Ann Bot.*, *N.S.*, **11**, 261-8, 1947) have now produced evidence of an error inherent in the method. They determined the suction pressure of *Iris* leaf tissue, carrot root and potato tuber by both the weighing method and by finding a solution of sucrose of such concentration that immersion of the tissue in it brought about no change in refractive index of the solution. The values obtained by the weighing method were always higher, and sometimes very much higher, than those obtained by the refractive index method. Thus the two values, in terms of molar concentration of sucrose, were respectively 0.20 and 0.53 for *Iris* leaf, 0.24 and 0.38 for carrot root and 0.20 and 0.24 for potato tuber. The explanation of the divergence is to be found in the partial infiltration of solution into the intercellular spaces of the tissue when this is immersed in the solution. For if this is so, when the tissue undergoes no change in weight after immersion in a solution a quantity of water must have passed out of the cells to balance that which has been absorbed into the intercellular spaces. Consequently, since exosmosis of water has taken place from the cells, the solution must possess an osmotic pressure greater than the suction pressure of the tissue.

ENTOMOLOGY. By A. D. LEES, M.A., Ph.D., Agricultural Research Council, Unit of Insect Physiology, Cambridge.

THE STRUCTURE OF THE INSECT CRANIUM.—Although the primitive segmental nature of the insect head is not in doubt—it is attested by the condition or occurrence of the embryonic appendages, neuromeres and coelomic sacs—the cranial exoskeleton itself bears few traces of segmentation easily recognisable as such. Nevertheless, according to G. F. Ferris, whose views are based on a study of the cranial "sutures" (*Microentomology*, 1942, 7, 25; 1943, 8, 8), such an interpretation is possible.

An issue of fundamental importance in this discussion concerns the status of the Y-shaped "epicranial suture" which runs longitudinally over the vertex of the head with the arms of the Y diverging over the facial area. In a few adult insects the arms or "post-frontal sutures" connect up with the temporal suture and run back over the sides of the head. Ferris maintains that this "great suture" then divides the antennal and mandibular segments.

of the head, the assumption being made that the intercalary segment is completely obliterated. Certain inconsistencies arise in this interpretation, however. In many holometabolous larvæ the arms of the "epicranial suture" pass anteriorly between the antennæ and are identified as the "clypeo-frontal suture"; this is then regarded as separating the antennal and pre-antennal segments. There is also difficulty in assigning a definite meaning to the terms clypeus and frons. The area enclosed below the "clypeo-frontal suture" is called the clypeus, yet, were the term not abandoned by Ferris, the area below the epicranial suture in its post-antennal position would be regarded as the frons.

E. M. DuPorte (*J. Morph.*, 1946, 79, 371), in a study of the facial area in all the principal orders, arrived at a contrary conclusion. The position of the so-called epicranial suture was by no means constant and in many adult insects was either absent or associated secondarily with other sutures. This point of view has been greatly developed by R. E. Snodgrass (*Smithsonian Miscell. Coll.*, 1947, 107, 52 pp.). The epicranial suture or "ecdysial cleavage line" of immature insects is in reality a preferred line of weakness along which the cuticle splits at moulting. The cleavage line is shown to be very variable in different insects, occupying at least five distinct facial positions. Indeed, there is a strong presumption that the cleavage line may take up any unoccupied space between the areas of insertion of the mandibular and facial muscles, which it invariably separates.

Although the frons and clypeus in most insects are not divided by a sulcus, their limits are defined internally by their muscle attachments. That the cranial muscles do in fact maintain the same relative positions in different orders of insects is established by E. F. Cook (*Microentomology*, 1944, 9, 1). The upper group of facial muscles (including the labral and hypopharyngeal muscles) are attached to the frons, the lower (cibarial muscles) to the clypeus. Adaptation to different feeding habits may, however, involve a change in proportions. In sucking insects, for example, the cibarial muscles are enlarged and the clypeus extends upwards at the expense of the frons.

According to the view advanced by Snodgrass, the frontal ganglion occupies a further "fixed point" in the cranial anatomy, since it lies consistently between the upper and lower groups of facial muscles. This may have been the primitive position of the frontal ganglion, which probably originated as a preoral nerve centre of the ventral cord. Its connections are now with the tritocerebrum, the primitive ventral ganglia of the premandibular segment. When

these ganglia were added to the brain their connectives may have been pulled round the œsophagus, leaving the frontal ganglion little changed in position. During development the latter takes its origin from the extreme anterior region of the stomodæum. Clearly, therefore, the clypeus can be regarded as preoral and the frons as postoral.

SCALE PIGMENTS AND THEIR BEARING ON THE SYSTEMATICS OF THE LEPIDOPTERA.—Modern classifications of the Lepidoptera make use of a wide range of significant structural characters. Nevertheless, it would be very advantageous were it possible to bring forward evidence of an entirely different nature, which would thus provide a means of checking independently the affinities deduced on structural grounds. Evidence of this kind, which may be said to derive from the classical researches of the late Sir F. Gowland Hopkins, has been described by E. B. Ford in a recent series of papers devoted to the chemistry and distribution of scale pigments, with particular reference to the families Pieridæ and Papilionidæ. In such work, where large numbers of species are sampled, many specimens will be rarities from museum collections which must not be destroyed. Therefore particular attention has been given to devising sensitive tests which can be applied to single specimens, where possible without damaging them.

As is well known, most of the white and yellow colours in the Pieridæ are due to the presence in the scales of pterins, pigments distantly related to uric acid. Employing a murexide test, Ford (*Proc. R. ent. Soc. Lond.*, (A), 1947, 22, 72) has been able to show that members of all four subfamilies possess pterins, whilst the representatives of ten other families of butterflies, including the closely related Papilionidæ, are without the pigment. In a special instance, that of the rare African species *Pseudopontia paradoxa*, the chemical evidence proves decisive in determining the systematic status. The affinities of this butterfly are highly obscure, but the demonstration that pterins occur in the scales (*Proc. R. ent. Soc. Lond.*, (A), 1947, 22, 77) confirms the sound judgment of systematists who had previously assigned the species to the Pieridæ.

The anthoxanthins, a group of sap-soluble plant pigments, ivory to deep yellow in colour, are widely distributed within the Lepidoptera, but are never common. Animals seem to be unable to manufacture these pigments so that, where present, they must be obtained from the food. Anthoxanthins are rare in the Pieridæ, but occur in the aberrant South American family, Dismorphiinae, where they are present in addition to, and not in the place of, the pterin pigments. The decision of systematists to unite this family with the

Palearctic "Wood Whites" (*Leptidia*) may have appeared bold, but the affinity is now confirmed, for all three species of *Leptidia* possess anthoxanthin pigments (*Proc. R. ent. Soc. Lond.*, (A), 1941, 16, 65). Among the 50 species of *Dismorphia* tested, 12 possessed anthoxanthins, and these species formed a compact group corresponding to the series previously prepared by systematists. There was one exception, *D. thermisia*, which was without the pigment. On a closer examination of the structural characters, Ford was able to show that there was good reason to suspect that this species had been misplaced.

The precise chemical nature of the red pigments in the Lepidoptera is still in doubt, but on the basis of chemical tests at least five different pigments can be distinguished (E. B. Ford, *Proc. R. ent. Soc. Lond.*, (A), 1942, 17, 87). The species of Pieridæ that are marked with red possess two pigments, "D" and "E." Both yield a positive murexide reaction, but are separable by the colour changes undergone when the wing is fumed successively with hydrochloric acid gas and with ammonia. Within the large genus of Indo-Australian butterflies, *Delias*, the distribution of these pigments in 71 red-marked species was not random, but was closely related to the present classification. Thus the great majority possess pigment D. However, in the Pasithœ group, comprising some six species, this is replaced by pigment E. There were only two exceptions, two species possessing both pigments.

The presence of anthoxanthins in the Papilionidæ seems to be a primitive feature, for they occur in several distantly related genera. They are characteristic of the "Kite Swallowtails" (*Graphium*), are universal in *Parnassius* and are present in vestigial amounts in members of the American section of the genus *Atrophaneura* (the genus *Battus* is revived to receive the latter). As these pigments also occur in primitive members of the Pieridæ (*Dismorphia*), the common ancestor of the Papilionidæ and Pieridæ may have possessed anthoxanthins. Two red pigments "A" and "B" are found in the Papilionidæ (*Proc. R. ent. Soc. Lond.*, (A), 1944, 19, 92). They are distinguished by their response to hydrochloric acid gas but, since they fail to give the murexide reaction, they do not appear to be uric acid derivatives. Pigment A is widely distributed in the Lepidoptera (occurring, for example, in the Arctiidæ) and is found throughout the Papilionidæ, save in the most specialised genera (*Battus* and *Papilio*), where it is replaced by pigment B. The classification of the Papilionidæ is discussed in a further paper (E. B. Ford, *Trans. R. ent. Soc. Lond.*, 1944, 94, 201), where the pigmentary, as well as the structural characters, are

brought to bear in elucidating the generic affinities within the family.

THE FOOD-FINDING ACTIVITIES of WIREWORMS.—Wireworms under pastureland will always have a supply of food immediately available ; but even when the vegetation is more sparsely distributed—as in a field of germinating cereals, for example—it is surprising how rapidly the plants are found and attacked. Two recent papers by W. H. Thorpe, A. C. Crombie, R. Hill and J. H. Darrah (*J. exp. Biol.*, 1947, **23**, 234) and by A. C. Crombie and J. H. Darrah (*J. exp. Biol.*, 1947, **24**, 95) have shown very clearly that wireworms seeking food are responding to chemical stimulation by substances released from the plant rootlets and root hairs into the soil. And much progress has been made in relating the activity of different substances to their chemical constitution, and in identifying the chemoreceptors in these insects. Any experimental work involving feeding encounters a difficulty at the outset, for wireworms pass through non-feeding phases, determined probably by the time relations of the moulting cycle (A. C. Evans and H. C. Gough, *Ann. appl. Biol.*, 1942, **29**, 168). However, this obstacle could be overcome by the careful selection of insects which had entered a feeding phase.

Although wireworms occasionally walk on the surface of the soil, they do not appear to perceive airborne odours. But they display two distinct types of response to chemical stimulation by substances in solution. The first is a *biting* response and it is elicited if the active substances are absorbed on filter papers: the wireworms will bite these and the number of bites provides a measure of the biting activity. The second is an *orientation* response. This is well shown if the insects are confined in a choice chamber filled with soil or sand which is moistened on one side with the active solution. The wireworms then soon aggregate within the "active" side. The behaviour in the presence of substances causing either biting or orientation is somewhat similar. In the first place, burrowing is less active ; and secondly, turning movements occur if a steep gradient of the active substance is presented. When experiencing the latter situation, the larva burrows straight ahead when entering the active region, but turns or constructs a side burrow if it chances to leave this zone. Nevertheless, in spite of some similarity in the movements, there is no doubt that biting and orientation are distinct responses, for at certain seasons wireworms will orient, although refusing to bite.

Biting is elicited by extracted plant juices and by many pure substances, including the common sugars, some fats (e.g. triolein) and some proteins. The latter are all of animal origin ; none of

the typical plant proteins, such as gliadin or potato protein, causes biting. A wide range of carbohydrates, including both mono- and tetrasaccharides, is bitten. If the aldehyde grouping is altered and the ring structure destroyed (as in sorbitol) the substance is still active. Probably therefore the response depends on the polyhydric alcohol grouping, as is the case with the sweet taste in man. But in the wireworm, which reacts to dextrin but not to glycerol, there seems to be a shift in sensitivity towards larger numbers of hydroxyl groups.

The biting response is elicited by substances in relatively high concentration (*e.g.* by 2 per cent. sugars). The same substances will also elicit orientation at this concentration. In addition, however, orientation (but not biting) is brought about by very dilute watery solutions of another class of compounds. Among the substances with a threshold activity of the order of 1×10^{-7} g. per 100 c.c. are a number of amides, including the plant amides asparagine and glutamine. Wireworms will also orient to many dicarboxylic acids like succinic and aspartic acids. The activity seems here to be conferred by the dicarboxylic acid skeleton, for it remains unimpaired after the amino groups in the molecule are blocked.

The wireworm possesses two types of chemoreceptors, thin-walled peg organs on the labial and maxillary palps and galeæ; and a peculiar multi-celled basiconic sensillum on the antenna. It is possible to show by amputation experiments that the peg organs are stimulated by sugars in high concentration and that their stimulation leads to both biting and orientation; whereas the antennal organ is stimulated only by the substances which elicit orientation at a low concentration. It is pointed out that the threshold sensitivities of the peg and antennal organs are of the order usually associated respectively with organs of taste and smell. The antennal organ may function as a distance receptor, responding to dissolved substances in very dilute solutions in much the same way as do the olfactory organs of aquatic animals.

What role do these reactions play in food-finding? It is known that the root systems of growing plants release amides such as asparagine and glutamine, and that these substances, which are of universal occurrence in plants, reach a high concentration in seedlings as a result of the breakdown of seed protein. The root system of a plant may be regarded in consequence as an extended trap which restrains the wireworm from further random progress through the soil. In locating the centre of the plant the wireworm may be assisted by the fact that the growing roots tend to form channels which it can follow. Once the grain or tuber is encountered random

biting will release active substances, such as sugars, in sufficiently high concentration to cause the insect to remain and feed.

INSECT REPELLENTS.—In recent years a great measure of success has attended the search for chemicals which will be effective in repelling biting insects, when applied in some way to the person. Many chemicals will drive off insects, but few combine a high grade of repellency with other properties necessary in a good repellent; unless, for example, the chemical exerts a lasting effect, is non-injurious and has an inoffensive smell, it is usually of little practical value.

The war-time need for an effective repellent for personal use by troops engaged in malarious areas resulted in research being directed particularly towards the development of mosquito repellents. Oil of citronella—the “classical” insect repellent—was found to afford inadequate protection under such conditions.

Some quantitative work on synthetic chemicals had been undertaken in the United States prior to the war (*e.g.* P. Granett, *J. econ. Ent.*, 1940, 33, 563, 566). Subsequently, a very large range of compounds, which included many already produced for the commercial market, were tested systematically at the Orlando Laboratory of the Bureau of Entomology. This led to the recognition of at least three repellents with outstanding properties, namely dimethyl phthalate, Rutger's 612 (2-ethylhexane-1:3-diol) and indalone, a proprietary preparation described as *n*-butyl mesityl oxide acetate. These are all light oily liquids which spread easily on the skin and have no unpleasant or toxic effects. When suitably applied they confer protection for over six hours against *Aedes*, but are decidedly less efficient against anopheles mosquitoes (E. F. Knipling and W. E. Dove, *J. econ. Ent.*, 1944, 37, 477). For general all round use by the American armed forces a mixture containing all three substances was recommended. Nevertheless, dimethyl phthalate has probably been most widely used, as it is easily manufactured and is in large supply (being employed extensively in the plastics industry); moreover, it is stable, suitable for application and has the right volatility for a lasting repellent.

Work on mosquito repellents was proceeding simultaneously in England and an extended account of these investigations has recently been published by S. R. Christophers (*J. Hygiene*, 1947, 45, 166). Methods of evaluating the repellents under standardised conditions have been greatly refined. And a large range of compounds and preparations have been tested for repellency. Three classes of substance were found to possess particularly strong repellent properties: (1) Unsaturated long-chain alcohols, aldehydes and phenolic

compounds of the type characteristic of most essential oils, such as citronellal, the active principle of citronella oil. (2) High boiling-point esters of the lower methyl series with various aliphatic or aromatic acids of high molecular weight. Dimethyl phthalate is the best-known example, but other methyl, ethyl, propyl or butyl esters of citric, adipic or cinnamic acids are often as effective. (3) High boiling-point alcohols, particularly diols. The duration of effect is closely related to the boiling point. A chemical of low boiling point may be extremely repellent, but will usually volatilise from the skin too rapidly to confer any lasting protection. It was found that any substance giving protection for two hours or more must have a boiling point of at least 250°C . at atmospheric pressure. Chemicals with boiling points much over 300°C ., whilst showing a lasting effect, were apt to possess a low grade of repellency.

Repellents of the type described above have been stated to be useful also in preventing the bites of fleas and of blood-sucking muscids, such as *Stomoxys*. No doubt many other uses will be found for them in civilian life. However, one of their most important applications also has a wartime background and concerns the discovery that dimethyl phthalate, when smeared on the skin or clothing, was extremely effective in preventing the attachment of Trombiculid mites. Tests in America against the chiggers *Acariscus* and *Eutrombicula* showed that, when the socks of the subject was suitably treated, infestation by the larvæ was reduced to negligible proportions for as long as 30 days (A. H. Madden, A. W. Lindquist and E. F. Knipling, *J. econ. Ent.*, 1944, 37, 283). In the Far East mites of the genus *Trombicula* are the vectors of scrub typhus, a disease which assumed a considerable military importance whenever troops happened to be stationed in localities where the disease was endemic. It is generally considered that the measures taken to impregnate clothing with dimethyl phthalate (and later with dibutyl phthalate, which resists washing of the clothing for a longer period) played a most important role in reducing the incidence of the disease.

Most of the modern high boiling-point repellents appear to act largely, but not entirely, as contact repellents, in contrast to substances such as citronella which repel insects at a distance. Of the powerful vegetable insecticides, pyrethrum is known to repel insects strongly after contact, but rotenone appears to lack this property. It was thought previously that DDT, in the form of a dry film, was also non-repellent. As regards mosquitoes, however, this has now been proved incorrect. Several groups of observers working under semi-natural conditions (e.g. J. B. Gahan and others,

J. econ. Ent., 1945, **38**, 231) have reported that mosquitoes exposed to DDT-treated surfaces showed a marked restlessness and tended to move away towards the light. And J. S. Kennedy (*Bull. ent. Res.*, 1947, **37**, 593) has demonstrated under controlled conditions that contact with DDT acts as an irritant: alighting mosquitoes frequently leave a toxic film before receiving a fatal dose through the tarsi. Moreover, many insects subsequently recover after exhibiting preliminary symptoms of DDT poisoning. Whilst in no way depriving DDT of its outstanding effectiveness as a contact insecticide, this is clearly a factor which has to be taken into consideration when planning the control of mosquitoes with residual DDT sprays.

NOTES

The Mechanism of Absorption of Sound on Porous Materials (R.A.S.)

Porous materials, such as rock-wool, felt and soft furnishings, are well known to be good absorbers of sound, and it is of obvious practical importance to understand in detail the manner in which the absorption takes place. Lord Rayleigh investigated the behaviour of a hypothetical porous material made up from a closely packed bundle of tubes and he showed that, for this type of material at least, part of the incident sound is transmitted as a rapidly damped wave in the air included in the material. Most porous materials used for sound absorption are fibrous in structure and are usually isotropic in their acoustical properties. The detailed results of Rayleigh's theory are, therefore, not applicable to such materials.

A considerable number of people have formulated general theories relating to the propagation of sound in isotropic porous material (*Rev. Mod. Phys.*, 1944, 16, 69). These theories rest on the assumption that an acoustic disturbance continues as a wave in the air in the space between the fibres in much the same way as in free air, but with characteristics which may be modified by the fibres. Thus the air in the space may be regarded as having an effective compressibility K and an effective density ρ . The fibres may also be expected to provide a viscous resistance to an oscillatory flow, as in the analogous case of steady air flow through the material. This can be expressed by a coefficient of porous resistance r . The theory of an acoustic disturbance in such a material can be formulated by the ordinary methods of acoustics and expressions obtained for the velocity and the rate of attenuation of a wave in such material. The mathematical form of the theory and the results are closely analogous to those of the theories of optical dispersion and of eddy-current distribution.

So far the theory is hypothetical and might well remain so but for the facility with which accurate acoustic measurements can be made by modern electro-acoustical technique. Beranek in America (*J. Acous. Soc. Amer.*, 1947, 19, 420) and Scott in this

country (*Proc. Phys. Soc.*, 1946, **58**, 165) have studied typical porous materials such as rock-wool in attempts to determine whether the general theory is applicable to these materials. The most effective method of approach is to measure with special exploring microphones the velocity and the attenuation of plane waves in the material and the ratio of oscillatory pressure to oscillatory velocity in such waves (the characteristic acoustical impedance of the medium). These three independent measurements give, at any one frequency, all the data needed for the calculation of the parameters K , ρ , and r . The results of such calculation, carried out over a range of frequency 250 to 5000 cycles per second, are of considerable interest and their interpretation is not difficult.

The value of the porous resistance r rises smoothly with frequency, much as would be expected from the contraction of the boundary layer at the higher frequencies. At low frequency the value agrees closely with that measured independently for steady flow in the same material. The effective density ρ of the contained air falls smoothly from about twice to once that of normal air as the frequency rises. At high frequency the inertia of the fibres must, therefore, be such that their slight movement under the forces imposed by the sound contributes inappreciably to the effective mass of the moving air. At low frequencies this contribution is such as would follow if the fibres took up about 1 per cent. of the amplitude of the air. Microscopical investigation of the moving fibres shows that this is indeed the case. The effective elasticity K proves to be in general complex, which means that the acoustic pressure is out of phase with the "condensation." At low frequency the elasticity is mostly real and numerically closely equal to the atmospheric pressure P ; at high frequency the elasticity is again mostly real and of magnitude closely equal to $1.4P$. The evidence, therefore, appears to show that condensation, in this porous medium at least, is isothermal at low frequency and adiabatic at high frequency.

Miscellanea

The New Year Honours List contained the names of the following scientists and others associated with scientific work: *G.C.I.E.*: Sir Maurice Gwyer, vice-chancellor of the University of Delhi. *G.B.E.*: Sir Edward Mellanby, secretary of the Medical Research Council. *K.B.E.*: Sir James Irvine, principal and vice-chancellor of the University of St. Andrews. *Knights*: Prof. F. C. Bartlett, professor of experimental psychology in the University of Cambridge. H. Claughton, recently principal of the University of

London ; Dr. J. D. Cockcroft, director of the Atomic Energy Research Establishment ; V. Z. de Ferranti, chairman and managing director of Ferranti, Ltd. ; Dr. W. Ivor Jennings, vice-chancellor of the University of Ceylon ; the Hon. C. J. Lowe, chancellor of the University of Melbourne ; H. R. Ricardo, chairman and technical director, Ricardo & Co., Ltd. ; Dr. R. V. Southwell, rector of the Imperial College of Science and Technology. *C.B.* : Dr. G. M. Bennett, Government Chemist. *C.S.I.* : W. T. Hall, chief conservator of forests, United Provinces, India. *C.M.G.* : A. P. Mitchell, director of land surveys, Palestine ; W. A. Robertson, forestry adviser to the Colonial Office. *C.I.E.* : J. Petty, chief conservator of forests, Sind ; W. D. West, director of the Geological Survey of India ; Dr. R. E. Mortimer Wheeler, director-general of archæology, India. *C.B.E.* : H. W. Bennetts, principal of the Animal Health and Nutrition Laboratory, Western Australia ; G. L. T. Brough, deputy director of electrical engineering, Admiralty ; J. C. Bugher, director of the Yellow Fever Research Institute, Yaba, Lagos, Nigeria ; B. G. Crewe, assistant comptroller, Patent Office ; W. J. Dawson, late metallurgical director of Hadfields, Ltd. ; W. S. Gordon, director of the Agricultural Research Council Field Station, Compton ; J. Henderson, formerly director of the New Zealand Geological Survey ; Mrs. S. Sutherland Isaacs, psychologist, London Clinic of Psychoanalysis ; Dr. L. A. Jordan, director of the Research Association of British Paint, Colour and Varnish Manufacturers ; Prof. W. Kerr, Royal Technical College, Glasgow ; F. Lambert, president of the Museums Association, director of the Walker Art Gallery, Liverpool ; W. L. S. Mackintosh, director of Veterinary Services, Uganda ; R. O'F. Oakley, assistant secretary, Department of Scientific and Industrial Research ; Prof. G. W. Robinson, professor of agricultural chemistry, University College of North Wales, Bangor ; Prof. C. Y. Shephard, professor of economics, Imperial College of Tropical Agriculture, Trinidad ; G. F. Stebbing, honorary secretary of the Radium Commission ; Miss M. V. Taylor, for services to archæology ; J. S. Teasdale, for services to agriculture in Western Australia.

The anniversary meeting of the Royal Society was held on December 1 last, when the Royal Medals of the Society for 1947 were presented to Prof. C. N. Hinshelwood, Dr. Lee's professor of chemistry in the University of Oxford, for his distinguished work on the mechanism of chemical reactions, and to Dr. F. M. Burnet, director of the Walter and Eliza Hall Institute for Medical Research, Melbourne, for his outstanding work on bacteriophages, viruses and

immunity, and for his contributions to the study of infectious disease as an ecological phenomenon. The following medals were also awarded : Copley Medal to the late Prof. G. H. Hardy, emeritus professor of pure mathematics in the University of Cambridge, for his outstanding part in the development of mathematical analysis in Britain ; Davy Medal to Prof. L. C. Pauling, director of the Gates and Crellin Laboratories, California Institute of Technology, for his distinguished contributions to the theory of valency and their application to systems of biological importance ; Buchanan Medal to Sir Edward Mellanby, for his distinguished researches on the physiology of nutrition, especially in relation to the causation of deficiency diseases ; Hughes Medal to Prof. J. F. Joliot, director of the National Centre of Scientific Research, Paris, for his outstanding contributions to nuclear physics, particularly the discovery of artificial radioactivity and of neutron emission in the fission process.

The following were elected officers of the Society : Sir Robert Robinson, president ; Sir Thomas Merton, treasurer ; Sir Alfred Egerton and Sir Edward Salisbury, secretaries ; Prof. E. D. Adrian, foreign secretary.

The council of the Geological Society has made the following awards : Wollaston Medal to Sir Edward Bailey, formerly director of the Geological Survey of Great Britain ; Murchison Medal to Dr. J. Phemister, of the Geological Survey of Great Britain ; Lyell Medal to Prof. A. H. Cox, professor of geology at University College, Cardiff ; Prestwich Medal to the Abbé Henri Breuil.

Other medals awarded include : the Trail Award and Medal of the Linnean Society of London for 1942 to Dr. Honor B. Fell, of the Strangeways Laboratory, Cambridge, and for 1947 to Dr. C. D. Darlington, director of the John Innes Horticultural Institution ; the Hanbury Gold Medal of the Pharmaceutical Society of Great Britain for 1947 to Dr. Hans Flück, professor of pharmacognosy in the Faculty of Pharmacy of the Federal Technical Institute, Zürich ; the Faraday Medal of the Institution of Electrical Engineers to Prof. M. L. E. Oliphant, Poynting professor of physics in the University of Birmingham.

The Lord President of the Council has appointed Sir Frank Engledow, F.R.S., Draper's professor of agriculture in the University of Cambridge, to be chairman of the Food Investigation Board, Department of Scientific and Industrial Research, in succession to the late Sir Joseph Barcroft.

Dr. R. L. Smith-Rose, superintendent of the Radio division of

the National Physical Laboratory, has been appointed to the new post of director of radio research in the Department of Scientific and Industrial Research.

Prof. H. Hartridge, F.R.S., has retired from the university chair of physiology at St. Bartholomew's Hospital Medical College, to become director of the Medical Research Council's newly constituted research unit on the physiology of vision.

We have noted with great regret the announcements of the death of the following scientific workers: Prof. E. C. C. Baly, C.B.E., F.R.S., emeritus professor of inorganic chemistry in the University of Liverpool; Prof. B. Bavink, an eminent German teacher of natural philosophy; Dr. Charles Bolton, C.B.E., F.R.S., consulting physician to University College Hospital, London; Prof. J. N. Brønsted, professor of chemistry in the University of Copenhagen; Dr. N. Murray Butler, formerly president of Columbia University; Sir John Fraser, K.C.V.O., principal of Edinburgh University; Sir Robert Greig, who rendered valuable services to agricultural education and research; Prof. G. H. Hardy, F.R.S., emeritus professor of pure mathematics in the University of Cambridge; Prof. Henryk Hoyer, emeritus professor of comparative anatomy in the Jagellonian University, Cracow; Dr. C. C. Hurst, geneticist; Prof. W. S. Lewis, professor of geography at the University College of the South-West of England, Exeter; Prof. F. R. Lillie, emeritus professor of embryology in the University of Chicago; Dr. G. G. MacCurdy, emeritus professor of prehistoric archaeology in Yale University; Dr. John Marshall, reader in mathematics in the University of London, Bedford College; Dr. C. A. Mitchell, formerly editor of *The Analyst*; Prof. Paul Monroe, emeritus professor of education in Teachers' College, Columbia University; Sir Leonard Pearce, C.B.E., engineer-in-chief to the London Power Company; Mr. W. H. Pick, of the Meteorological Office; Prof. H. A. Prichard, emeritus professor of moral philosophy in the University of Oxford; Rt. Hon. Lord Rayleigh, F.R.S., emeritus professor of physics in the University of London, Imperial College of Science and Technology; Sir Bernard Spilsbury, honorary pathologist to the Home Office; Dr. Cyril Strickland, formerly professor of medical entomology and director of the School of Tropical Medicine and Hygiene, Calcutta; Prof. A. N. Whitehead, O.M., F.R.S., emeritus professor of philosophy in Harvard University.

The Eighteenth Session of the International Geological Congress will be held in London from August 25 to September 1, 1948, and

meetings will be held in the Royal Albert Hall and the lecture theatres of the Royal Geographical Society and the Imperial College of Science and Technology. The Rt. Hon. Sir John Anderson, P.C., F.R.S., has accepted the office of Honorary President of the General Organising Committee, and Prof. H. H. Read, F.R.S., President of the Geological Society of London, has succeeded the late Sir Thomas Holland as President of the General Organising Committee and President-Designate of the Congress. The Headquarters and Office of the Session are in the building of the Geological Survey and Museum, Exhibition Road, London, S.W.7.

The International Congress on Mental Health, organised by the National Association for Mental Health, will be held in London from August 11 to 21, 1948. The Congress will consist of three separate International Conferences on Child Psychiatry, Medical Psychotherapy and Mental Hygiene, each under the auspices of the appropriate international body. Meetings will take place at the Central Hall, Westminster, and the Congress Office is at 19 Manchester Street, London, W.1.

The work of the Boron Agricultural Bureau was brought to a close at the end of December 1947. Its comprehensive collection of literature on the uses and functions of boron in agriculture is available for consultation in the Library of the Rothamsted Experimental Station, Harpenden.

The use of electro-magnetic couplings between the Diesels and gearbox of geared Diesel propulsion drives for ships has many advantages, including the protection of the gearing from injurious torque variations. An account of their design and operation is given in *B.T.H. Activities* for October 1947 (British Thomson-Houston Co., Ltd., Rugby).

The coupling consists of two concentric members, the rotor and the outer-half. The rotor comprises a laminated core with squirrel cage bar winding and is bolted to the engine crankshaft. The outer-half is bolted to the input shaft of the gearbox and is fitted with salient D.C. wound poles. The coupling is in effect an induction motor, its operation requiring a slight difference in speed between its two members.

As the simple opening or closing of excitation switch is all that is required to bring an engine in or take it out of service, this type of coupling is a great aid to the manoeuvrability of a ship. The coupling provides a most effective shock absorber, as the only tie between the "driving" and "following" members is the

magnetic force across an air gap. Its operation might well be compared with that of the fluid fly-wheel. Arrangements are also made for the coupling to "break step" and remove the engine from service at 150 per cent. full-load torque, a useful provision if the propeller were to foul.

The phenomenon of electrolytic brightening or polishing of metal surfaces was probably first observed on the anodes in electroplating baths many years ago. An account of some recent investigations into the effect is contained in the *Australian Journal of the Council for Scientific and Industrial Research* for May 1947.

The surfaces obtained by electrolytic polishing are in many cases superior to those obtained by mechanical polishing, being entirely free from scratches as well as deformation and Beilby layers. Use has been made of this method in industry, with particular success for polishing stainless steels and aluminium, as well as in metallographic work.

The first serious study of the effect was made by Jacquet, who explained the phenomenon by postulating the formation of a film on the anode surface. As a result of the "hills" on the anode surface, the film, being thinner at such points, would have a lower resistance. Consequently the current density would be greater at the "hills" and would result in their preferential solution and a general smoothing or polishing of the surface as a whole. More recently Elmore has suggested that the rate of diffusion of ions through the film is the more important factor.

In his work on copper, Jacquet found that the curve obtained by plotting the cell voltage against the current density (in a cell of orthophosphoric acid with copper electrodes) showed a definite flat portion. Over this flat portion good electrolytic polishing was obtained. If the current density was too low, etching resulted, while if it was too high polishing occurred but was spoilt by the bubbles of gas which were formed. The authors of this recent paper have developed a method of measuring, instead of the voltage across the cell, the anode potential relative to the solution. The curves obtained when this potential is plotted against current density, although often similar to those obtained by Jacquet, are more characteristic of the phenomenon. This is to be expected as the polishing occurs as a result of anode reactions, and reactions in other parts of the cell are irrelevant.

The effect of changes in the concentration and temperature of the electrolyte were investigated. It was found that for very dilute solutions and also for hot solutions no polishing occurred, although

the anode potential-current density curve still showed a flat region in each case.

The paper ends with a discussion of the theories which have been suggested to explain the mechanism of the polishing. Whilst the film theory explains most of the effects, it does not appear to be the only factor, and it seems likely that an oxide layer exists at the anode surface which may play a significant role.

The *Bell Laboratories Record* for November 1947 contains a detailed description of the special testing laboratory which was established at Whippany during the war. The object of the laboratory was to test, under the exacting conditions encountered in modern warfare, electrical equipment which had been designed for the armed services.

The testing equipment installed was able to reproduce artificially the very different climatic conditions found in the world, varying from the arctic to the tropics. Tests were also made which simulated the vibration of transport and the shock of exploding bombs or shells, while equipment for use in aeroplanes and ships required their own special test conditions.

Three "stratosphere" chambers were built, and in them the air temperature could be varied from -70 to $+160^{\circ}$ F. and the relative humidity raised to 95 per cent. Observations were made through multi-pane windows built into the walls. The walls were heat insulated and, to withstand the enormous pressures on the outside, were reinforced by steel members. To "flight test" equipment the air pressure could be lowered to that corresponding to an altitude of 60,000 ft., while the equivalent "rate of climb" averaged 6000 ft. per minute up to 50,000 ft. These chambers were also used to test special packing methods designed to prevent the infiltration of moisture into apparatus stored in the holds of ships or in warehouses in the tropics.

Vibration machines of different sizes were used in which accelerations as high as ten times that of gravity, at frequencies up to 60 cycles per second, could be obtained. The rather unusual roll-shock-vibration machine was able to simulate the pitch and roll of a ship at sea, and could do nine 45° rolls per minute, representing a heavier sea than any likely to be experienced. On this machine equipment could be shock tested by slamming the table top against a wooden bumper by means of a pneumatic ram. In another shock-testing equipment, a 400-pound hammer was dropped from heights up to 5 ft. on to the anvil plate on which the unit under test was mounted.

To check the qualities of corrosion-resistant metals and of protective finishes, a salt spray testing cabinet was devised by the Navy. This was found to be so destructive that it was often called the "disintegrator." In a few days it could produce the effect of many months or even years at sea.

The tests described above, and many others, showed up any weaknesses in the equipment before it passed into service. By carrying the tests beyond the requirements for acceptance by the armed forces and sometimes even to the point of destruction, it was sometimes possible to see how the design of a piece of equipment could be even further improved.

The French scientific journal *Atomes* for November 1947 contains an article by Pierre-Louis Klotz on the problem of helping the deaf to hear. Besides giving a general account of the problem, the article includes a description of some of the modern methods of diagnosing the cause of deafness and of their use in the design of hearing aids.

After a preliminary description of the ear, Klotz points out the great importance of the fact that sounds can be transmitted to the inner ear by two routes, one in air and the other in bone. He describes the simple experiment of holding a watch on the mastoid bone behind the ear when the characteristic tick can be plainly heard as soon as the watch makes contact. In certain cases of deafness, while the aerial route is damaged, the bony route is still quite normal. People suffering with this type of deafness cannot follow a conversation but can hear with ease on the telephone.

Totally deaf people are very rare. Except in cases where there are deep injuries to the inner ear or auditory nerve, a little hearing remains. To help diagnose the cause of deafness and to find to what extent a deaf person's hearing is impaired, the limit of audibility, i.e. the volume of sound that can just be heard, is measured at different frequencies. A curve is then plotted, for both the aerial and bony paths, of the limit of audibility against the frequency. By comparing this curve with a similar curve for someone with normal hearing, it is possible to find at which frequencies and to what extent the deaf person's hearing is below normal.

A perfect hearing aid would be one which raised the deaf person's curve of audibility back to normal. While the amplification must be sufficient, uniform amplification at all frequencies would not be satisfactory and in fact would be most uncomfortable, as it would raise parts of the hearing into the region of painful audibility.

The amplification must therefore be selective, being greatest at those frequencies where the hearing is the most affected.

It is also important that the amplifier should cover a wide range of frequencies. So that a conversation can be followed without fatigue the minimum frequency range required extends from 512 to 4096 cycles per second. The higher frequencies are particularly important as it is by means of them that our ear is able to distinguish between different consonants. For the enjoyment of music the frequency range should be even wider and should bring the resultant hearing as nearly back to normal as is possible. The amplifier must be free from amplitude and frequency distortion, to both of which the ear is particularly sensitive.

One type of hearing aid, which was first introduced over thirty years ago and is still in use, concentrates the sound on to a small area (without actually amplifying it). In its more modern form it consists of two microphones arranged in series and adjusted so that the overall response is greatest at the required frequencies. The equipment is run off a small battery, and is found to be particularly useful to those deaf people who can hear on the telephone. Unfortunately above 3000 cycles per second the frequency and amplitude distortion are very bad. It is interesting to recall that it was while experimenting with a similar device, to help one of his deaf relations, that Graham Bell invented the telephone.

Hearing aids have benefited greatly by radio research carried out during the war, and in the up-to-date equipment the sound is received on a microphone and amplified electronically before being fed to the earpiece. To obtain good reproduction a quartz microphone is used. The amplifier, which gives a gain of about 1000, uses three miniature pentodes and is run from a small battery. The amplifier is fitted with a tone control so that the user can choose the particular band of frequencies to be amplified. The choice depends on the particular type of deafness and on the type of sound to which the deaf person is listening, *e.g.* a male or female voice. Two types of earpiece can be used either working on the bone behind the ear or straight into the auditory canal. Whilst the former is less conspicuous the overall response to the higher frequencies is comparatively poor.

By using miniature components, including the new American type of miniature valves, it has been possible to make the apparatus very compact, light, and robust. Unfortunately the equipment is very expensive, but as their use becomes more universal it is to be expected that the price will drop. With their aid, what might be called the "social rehabilitation" of deaf people is possible. It

is to be hoped that the prejudice that deaf people themselves have against wearing the appliance and thus publicly admitting their infirmity will go, and that their use will become as universally accepted as the wearing of glasses.

"Whatever may be the contribution of other factors to Britain's economic recovery there can be no doubt that nothing will help so much towards the creation and maintenance of a high standard of life as the fullest use and application of scientific discovery." Thus Sir John Anderson begins the editorial in the first number of the new scientific journal *Research*, which was published last October by Butterworth's Scientific Publications Ltd. The journal aims at helping to bridge the gap between pure research and its practical development, between the scientist and the industrialist. In its pages the fruits of research will be quickly brought to the notice of the industrialist so that he may make full use of them, while the scientist will be kept in touch with recent advances in his own and other branches of science as well as being helped to appreciate more readily any possible practical applications of his own work. To produce a journal which will appeal to both classes is a difficult problem but, judging by the high standard of the first number, one that has been solved. That there is a great need for a journal of this type is stressed by many eminent scientists in their letters to the Editor, welcoming this new venture.

Among the articles in the first number of the journal is one by Sir Clifford Paterson and R. E. Leeds dealing with the preparation of fine diamond dies for wire drawing. Although as long ago as 1819 a patent was granted to Brookedon of London for wire drawing through diamond dies, such dies had not been made in large numbers in this country before 1939. When it became necessary to manufacture them in England during the war, the whole art had to be redeveloped, as very little had been written on the subject and the secrets of die making were carefully guarded by the continental manufacturers.

Wire drawing is the process of pulling wire through a tapered hole in a hard material so that, by plastic deformation, the diameter of the wire is slightly reduced. By the use of dies, each of successively smaller diameter, it is possible to produce extremely fine wire, even down to a diameter of the order of a seventh of that of human hair. If the dies are made of hardened steel, their life, due to wear, is very limited even for comparatively soft metals, while for hard metals such as tungsten and molybdenum their use is impossible. The use of the diamond, the hardest material known, solves the

problem of wear, but the manufacture of the die, due to the diamond's hardness, is in turn all the more difficult.

The first problem is the selection of suitable stones, care being taken to see that they are free from internal flaws and, with the help of polarised light, to endeavour to discard any with internal stresses. The latter is difficult without cutting the stones, which can be done only after purchase.

The first operation on a stone is to cut and polish two parallel working faces and at right-angles to these faces one or two viewing windows. The contour of the tapered hole to be cut depends on the metal to be drawn through the die. The two methods available for cutting the hole in the die are either to burn or to grind it out. The latter, or diamond cut diamond process, is at present the one more universally adopted.

As the size of a suitable stone is of the order of 3 mm. long, 2.5 mm. wide and 1.25 mm. thick, most of the machines are fitted with microscopes so that the processes can be controlled with greater accuracy. Great care is taken to see that the stone is correctly centred before any drilling operations are begun. First the point where the diamond is to be pierced is carefully marked. It is then mounted in the chuck of a miniature lathe and the mark is observed through a microscope. The position of the stone in the chuck is then adjusted so that, when the lathe is rotated, the mark still appears stationary.

After the stone has been centred up, a conical-shaped incision is first made by cutting with a diamond sliver held in a pair of pliers. In the boring which follows a revolving needle is used with diamond powder. The needle serves to keep the hole straight, but quickly wears and requires resharpening. The diamond powder used is obtained by mechanically crushing low-quality diamonds, the powder obtained being graded by means of fine sieves, centrifuges and other means. It is interesting to note that, when the finest grades of powder were examined under an electron microscope, they all showed sharp fragments with little or no signs of rounding off at the edges.

After drilling, the die is mounted in a metal block and, in order to reduce friction in use, the tapered hole is polished. Diamond dies are now being manufactured by this method to meet all requirements. Development work is, however, in progress on more rapid methods of piercing the stones, using either an electric spark or by burning in gas, and may possibly result in the production of cheaper and even better dies.

ESSAY REVIEWS

MATHEMATICAL PHYSICS. By H. S. W. MASSEY, M.Sc., Ph.D., F.R.S., Goldsmid Professor of Mathematics in the University of London, University College. Being a Review of **Methods of Mathematical Physics**, by HAROLD JEFFREYS, M.A., D.Sc., F.R.S., and BERTHA SWIRLES JEFFREYS, M.A., Ph.D. [Pp. x + 679.] (Cambridge: at the University Press, 1946. 63s. net.)

And thus to empyrean height
Of every kind of lore
In search of wisdom's pure delight
Ambitiously we soar.

W. S. GILBERT, *Princess Ida*.

THE subjects now legitimately included under the title of Mathematical Physics cover a very wide field—quantum theory, special and general relativity, supersonics are recent very extensive and rapidly expanding extensions. It is a remarkable and fortunate fact that many of the mathematical methods which are employed in dealing with most of the field form a close unity. Thus Rayleigh's *Theory of Sound* forms an excellent introductory textbook for a student of wave mechanics, and in recent years the inverse relation has been exhibited of a treatise on sound dealt with from the view point of a quantum theorist. The new subject of nuclear dynamics borrows very considerably from classical hydro- and thermodynamics. Nuclear fission, a topical enough phenomenon, bears much resemblance to the break up of a liquid drop under surface tension. The theory of the oscillation of such a drop, developed in 1879, was made use of by Bohr and Wheeler in their analysis of the conditions for fission. It is only because of this unity that it is possible to write a book with the title *Methods of Mathematical Physics*, which is of value both to the specialist in aerodynamics and in atomics, to name but two.

There is perhaps some initial confusion in one's mind when seeing such a title. Does the book deal with the mathematics used in physical applications or with the way in which it is possible to formulate the problems of physics in mathematical terms? There is a very real and important technique associated with the latter,

but the Jeffreys' book is concerned with the mathematics itself. They would perhaps suggest that the other aspects would fall within the scope of theoretical rather than mathematical physics, and as long as that is understood we can approach the book with a clear expectation of what we are likely to find there.

It does not take long to discover that the authors have produced a book of the greatest interest and usefulness. One can browse for hours in it, lapped in its erudition, or one can have recourse to it as a reference book for some point arising from a research problem or for the preparation of a lecture course. In fact, for the latter purpose it is somewhat of a godsend.

In planning a book for the use of physicists an important question soon arises. How rigorous is the treatment to be? This is always a matter of opinion, but many will agree that a happy mean has been struck. While the treatment in the book could not be objected to by the strictest mathematical purist, the emphasis is never on the abnormal functions which by their very nature would not be physically significant. In the writer's experience Honours students of physics find a reasonably rigorous proof satisfying to a degree that they could not achieve otherwise.

One of the most difficult feats to achieve in any subject is that of standardising notation once it has got out of hand. A magnificent example is provided by vector and tensor analysis. At the outset there is difference of opinion as to whether the Clarendon notation is necessary seeing that, for vector analysis in more than three dimensions, the suffix notation is obligatory. But the proponents of the Clarendon notation exhibit a partisanship on the matter of the $\cdot$ and $\times$, $\cdot$ and $\wedge$ and bracket representations of products which is worthy of an Egyptologist clinging to his particular transliteration of a hieroglyph. Although the views of one of the authors on the matter of the suffix notation are well known, they expound three-dimensional vector and tensor analysis in both Clarendon type and suffices. This would seem to be a wise decision. Thus many would agree that the suffix notation has its disadvantages for theoretical, as distinct from mathematical, physics—the Clarendon notation does help one to think physically in terms of vectors. On the other hand, there is no dispute about the essential role of the suffices in tensor analysis in four or more dimensions.

It is impossible to deal with the product notations in this neutral fashion. The $\cdot$ and $\wedge$ have been adopted. A nationalistic tendency is now becoming manifest—no American would use other than the $\cdot$ and $\times$, no one but a Briton uses the $\cdot$ and $\wedge$, while the bracket notation still survives in the ancient Continent.

It is gratifying to find a chapter devoted to matrices—an interesting and useful chapter at that. One regrets though that, having come so far, the authors did not say something of group theory—a technique which has been used with great power in many branches of physics. Algebra has not been a fashionable branch of mathematics in this country for many years, but it is eminently applicable mathematics. British mathematical physics has undoubtedly suffered from this and the gap still remains unfilled.

It is gradually being realised that one of the most important methods of mathematical physics is that of numerical calculation. There are very few fields of research which do not involve, at some stage, extensive arithmetic. Yet it has not been regarded as necessary to teach prospective mathematical physicists anything of the technique of calculation. Signs are multiplying of a change of heart, but much more must be done before adequate training is available. It is gratifying to find that the ninth chapter of the Jeffreys' book is devoted to Numerical Methods. It is to be hoped that, as well as the students studying from the book, the teacher planning his lecture course with its aid will give due weight to the importance of this chapter.

The calculus of variations does not usually occupy a place in the middle of any text on mathematical physics, yet it richly deserves to do so. Most laws of physics of any generality are capable of expression as a variation principle, and for theoretical research this is often the most convenient form. The man concerned with numerical applications also depends very much on variation methods. This has become clear from its wide importance in atomic physics, but in all types of problem involving oscillatory phenomena it provides the most powerful method of approximating to the normal modes in complicated cases. One feels that the central position occupied by the Chapter on the Calculus of Variations is well chosen, as is the subject matter of the chapter.

The three chapters devoted to the exposition of the theory of functions of a complex variable are very well planned to provide the maximum of useful technique combined with clarity and rigour. As the idea of the book arose from an earlier book by one of the authors on operational methods, the opportunity is naturally taken of relating contour integration to operational methods, already introduced in earlier chapters. These methods are of established importance either in the form favoured by Professor Jeffreys or in the guise of the Laplace transform. They have not yet penetrated to most University courses in mathematical physics and it is surely time that they should. Their value in developing the properties

of special functions is also manifest, even though there may be room for argument about the validity of the deduction in any particular case. Whether Jeffreys' method or any equivalent one is the best seems to be somewhat irrelevant. The important thing is that the operational approach to the various problems for which it is appropriate should become commoner knowledge to British students.

What can one say about the remaining eleven chapters? They cover a wise and extremely important field—Fourier's Theorem, Second Order Differential Equations, the Wave and Heat Conduction Equations and the Special Functions associated with them. All these subjects are adequately dealt with, providing a wealth of reference material for teachers and research workers, a new world for the student just beginning to glimpse the immense power and beauty of the technique for dealing with the boundary problems of mathematical physics.

The technique for determining the solutions of second order differential equations satisfying boundary conditions is of basic importance in many physical problems. Yet how few books give any idea of the way in which such solutions can be obtained except for the simplest cases. It is a pleasure to note that the Jeffreys actually include a discussion, with examples, of cases in which the series solution is given by a three-term recurrence relation.

Another very important technique which receives very slight attention in most textbooks is the method of steepest descents or its equivalent, the method of stationary phase. It is by means of this that the transition from wave to geometrical optics or mechanics can be made. As a means of obtaining asymptotic expansions of functions under certain conditions it is unrivalled. At last a good systematic account of the method is to be found in *Methods of Mathematical Physics*, Chapter 17. It is, by the way, an interesting point that so many of the series which the practical mathematician employs are divergent.

Long before p. 668 has been reached the reader will have become convinced that once more we have a mathematical classic in the famous Cambridge style. Just as every mathematical physicist either begs, borrows, steals or, not infrequently, even invests in a copy of *Modern Analysis*, so also will he be prepared to acquire by hook or by crook the new volume by Harold and Bertha Jeffreys. There are bound to be occasions when recourse to it for assistance will be unrewarded. Applicable mathematics forms a vast storehouse of machinery for tackling the complex and various problems which Nature sets for man. Nevertheless, there will be many times when the required hint or the sought-after formula will be forthcoming.

The budding mathematical physicist would do well to include the book as a major item of his regular reading, for it is essential that he should have a detailed knowledge of what it contains.

Valuable indeed as a book of this kind is, there still remains the need for a companion volume of similar dimensions which should be rather more concerned with the methods of theoretical physics—the way in which the physical problems are put into mathematical form. The incentive to some industrious writer to fill the gap should be much greater now that so much of the mathematics applicable in physics has been made available in so comprehensive a manner in the present book.

GERMAN WAR RESEARCH. By W. E. CURTIS, D.Sc., F.R.S., Professor of Physics in the University of Durham, King's College, Newcastle-upon-Tyne. Being a Review of **German Research in World War II: An Analysis of the Conduct of Research**, by LESLIE E. SIMON. [Pp. xii + 218, with 73 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 24s. net.)

THE aim of this book is to give a picture of the organisation and achievement of German research during the war. Although, in fact, it deals almost exclusively with weapon research, this covers such a very wide field, and the effort involved was so immense, that few readers are likely to cavil at the omission of other topics, such as medicine and chemistry.

The author, as Director of the Ballistic Research Laboratories at the Aberdeen Proving Ground, was well qualified to undertake the investigation of German scientific establishments, and his critical survey of their organisation is of great interest and importance. It certainly deserves the closest study by our own authorities. The German scientific war effort, launched in 1932, was conceived and executed on a scale unparalleled in history, except for the special case of the atomic bomb development, and, although it achieved much, there is little doubt that, had it operated at full efficiency, it would have had a far greater influence on the course of the war, and might even have reversed the result. Having regard to their five years' start, the lavish scale of their equipment and accommodation, and the degree of concentration of their scientific manpower on war research and development, it is surprising that the German achievement in this field did not far surpass our own, hampered as it was by the lack of pre-war preparation, the improvised character of much of our organisation, shortage of technical manpower and supplies, and other difficulties. There is no single explanation, but the main factors would appear to have been undue concentration

on spectacular but indecisive weapons such as V1, V2 and long-range guns, defective organisation of development work and poor co-ordination with research, and the over-confidence induced by the early war successes, leading to a diversion of industrial support from the scientific effort and to conscription of a considerable number of scientists. Many of these were subsequently released, but too late to be of any use. The Nazi pre-war anti-scientific policy, leading to the persecution and exile of so many of the ablest German scientists, both Jewish and non-Jewish, must also have had very serious consequences for them in the field of fundamental research.

The research and development work associated with the V2 long-range rocket was the outstanding example of the German obsession with the spectacular or "stunt" weapon. The establishment at Peenemünde alone cost three million pounds and employed some 2200 professional scientists and technicians, and in addition much subsidiary work, involving elaborate and expensive wind tunnels, was carried out elsewhere.

The research organisation of the German Air Force was on a far larger scale than those of the other two services. It was brilliantly planned, and operated vigorously and regardless of cost. The guiding group, known as Fo Fö (Forschungsföhren), consisted of four senior scientists and controlled eight large establishments as well as the Technical Academy of the Luftwaffe (TAL). One of the former, for example, which carried out research on aircraft weapons and engines, covered an area of 2½ square miles and employed 1200 scientists. TAL consisted of thirteen institutes which carried out fundamental and technical research of the most advanced character and on the largest scale. These institutes were essentially autonomous in scientific matters, reporting directly to Fo Fö and having the prerogative of initiating research. In planning them, the Germans recognised and overcame a difficulty inherent in all large-scale research organisations, namely, how to prevent the director becoming so overburdened by administrative duties that he is unable to exercise his proper functions of guiding research. They achieved this by limiting each institute to a size which permitted the director to know every scientist personally and to keep in touch with all the work in progress. Administration was the responsibility of the head of a group of institutes. There must have been certain difficulties and disadvantages in practice, and the organisation might not have commended itself to our Treasury as a tidy one, but the scientific advantages must have been immense. The waste involved in the employment during the war

of so many of our best scientists in mainly administrative work was undoubtedly a very serious matter.

These establishments lacked nothing in facilities and apparatus which money and ingenuity could provide. Every weapon development group had fully equipped firing ranges attached to it, and measuring instruments of all kinds were available in profusion. Yet they were mainly the products of established instrument-making firms, so that there were few fundamentally new types. In particular, there was a notable failure to take advantage of electronic methods of measuring short time intervals. On the other hand, the technique of telemetering, especially in the transmission of instrument reading from projectiles in flight to ground stations, was highly developed. This was, of course, because of its value in connection with the work on the V2 rocket. At times, too, the German passion for fine workmanship deflected effort from the end to the means, as for example in the development of an elaborate stereo-photographic technique for the direct measurement of bomb craters, for which we found a tape-measure quite adequate.

Nevertheless, there were some outstanding enemy achievements in addition to the V1 and V2 missiles. The most formidable of these, the R4M rocket, fortunately just failed to get into full production by the end of the war. A battery of 48 of them was to be fitted beneath the wings of the Me 262 fighter and fired in succession at 50 millisecond intervals. In an operational trial six fighters so equipped shot down 14 B 17 E Flying Fortresses without loss to themselves. As the Me 262 was at least the equal of the allied jet fighters, it is likely that the bombing of Germany would have become an extremely hazardous undertaking. The non-rotating, fin-stabilised shell was another interesting innovation. The disadvantage of spin-stabilisation is that the length of the projectile is restricted to about 8 calibres, and that the penetrating power of a "hollow charge" head is much reduced by rotation. The new projectile was successfully employed against the Russians, but not against the Western Allies, apparently for fear lest we should copy them. An entirely new system of shell stabilisation, depending upon the production of shock waves by a specially shaped nose, was also worked out, although its application would have been limited to rather short ranges. Other revolutionary weapons were less successful, and the effort put into them must have appreciably diminished the German war potential. Examples are the super gun, a battery of which near Calais was intended to subject London to a continuous rain of 150-lb. projectiles, a wind gun intended to

fire a "plug" of air, and a sound gun. All three of these failed for lack of range. It is interesting also to learn that the Germans failed in several important projects in which the Allies were successful, notably the (American) proximity fuse and the (British) "sabot," or sub-calibre high-velocity projectile.

The book is excellently produced, well illustrated and adequately indexed. It should prove most valuable to those concerned with the organisation and execution of armament research, but it contains much of interest also to the general physicist.

REVIEWS

MATHEMATICS

Advanced Calculus. By DAVID V. WIDDER. [Pp. xvi + 432, with 39 figures.] (London: Constable & Co., Ltd., 1947. 36s. net.)

It is rare nowadays for a book to appear on advanced calculus. The usual course in mathematics takes the student through elementary calculus only, the rest of his time being occupied in discussion of the philosophy rather than the technique of analysis. This may be appropriate for the pure mathematician, but it often leaves the applied mathematician ill-equipped with technique.

There is much of interest and use in this book. The treatment of partial differentiation and its applications is very much fuller than in most available texts. Many students become familiar with the more theoretical aspects of functions of two or more variables without being capable of manipulating partial derivatives or evaluating a double integral. Reference to the examples discussed in this book would help to remedy this unfortunate situation—the chapters on Multiple Integrals, and on Line and Surface Integrals are also both sound and applicable.

Unusual features are the presence of chapters on Stieltjes Integrals and on the Laplace Transform and its applications. The subject-matter of these chapters, as indeed of the book as a whole, is well chosen and should provide valuable material for both pure and applied mathematicians.

In addition to the above-mentioned chapters, there are others dealing with Vectors, Differential Geometry, Limits and Indeterminate Forms, Infinite Series, Convergence of Improper Integrals, The Gamma Function and Fourier Series.

A minor defect is the unnecessary choice of yet another distinct notation for vector products, which makes the otherwise compact treatment of Differential Geometry rather heavy reading until the new notation is mastered.

It is a pity that the cost of the book is rather high, as more text-books of its type are badly needed in this country.

H. S. W. M.

Applied Bessel Functions. By F. E. RELTON, M.A., D.Sc. [Pp. viii + 191, with 10 figures.] (London and Glasgow: Blackie & Son, Ltd., 1946. 17s. 6d. net.)

Of late there has been an increasing number of text-books directed at supplying the mathematical equipment requisite for theoretical investigations likely to be undertaken by the technical physicist or engineer. The aim of these texts is a laudable one, and in time one may hope that every engineering undergraduate course will contain an introduction to the methods of complex

analysis, with the consequent operational calculus, so useful particularly in the treatment of vibration and wave problems. For the present, however, there must remain a large technical public, unfamiliar with these more advanced methods, and perhaps not requiring their full scope, who nevertheless are anxious to extend their knowledge of the special functions of practical application. It is to such a public that Dr. Relton's book is addressed, growing, as the author explains in his Preface, from a course of lectures given to an audience of physicists and engineering research workers "impeded in the progress of their researches by their inability to handle Bessel Functions."

The previous knowledge assumed by the author lies well within the traditional scope of an engineering or physics degree course—the exponential and trigonometric functions, and the elements of differential and integral calculus. Complex variable theory is not used. After an introductory chapter on the Gamma- and related functions, Chapter II is devoted to a brief exposition of the Sturmian theory for second-order linear equations. The Cylinder functions are then introduced by their recurrence formulæ, leading to Bessel's equation, from which, without explicit series solution, the form of the functions is discussed. The series solution for $J_n(x)$ is deferred to Chapter IV. This approach is a novel one, and seems to offer some pedagogic advantage in an elementary course. The remaining theoretical ground covered consists simply in the introduction of the second solution $Y_n(x)$, and the modified functions, whilst the last two chapters are devoted to the derivation of some integral formulæ, the asymptotic expansions, and brief mention of some of the allied functions.

In keeping with the aim of the book, a large number of problems are discussed—the solution to twenty being given in full, whilst others are provided as exercises. The emphasis is mainly on mechanical problems—stability of struts, and vibrations of bars, discs and membranes—though some others are treated. Like the rest of the book the problems are all of an elementary nature.

Throughout the author's exposition is pleasant in style, and clear on points of principle. The production of the book is up to the usual high standard one expects from the publishers.

J. C. G.

Guide to the Literature of Mathematics and Physics including Related Works on Engineering Science. By NATHAN GRIER PABKE III. [Pp. xv + 205, with 8 plates.] (New York and London : McGraw-Hill Book Company, Inc., 1947. 25s. net.)

THE first part of this very painstaking guide is in four chapters, namely : Principles of Reading and Study, Self-directed Education, Literature Search, and Periodicals. This part offers helpful information on the use of large libraries, describing the varieties of catalogues and their arrangement ; classification, with the Library of Congress system as an example, is also discussed. The nature and merits of dictionaries, encyclopædias, handbooks, etc., are treated in both the first and third chapters, which causes some repetition. The chapter on periodicals gives details of the dozen or so leading abstracting journals and reviews in the fields covered. The author's preference for things American is natural, especially where German *Handbücher* are concerned. But it is not clear why *Science Abstracts* are decried for "the bad policy

of translating all foreign titles into English." That policy is followed for all subject entries in the great American *Quarterly Cumulative Index Medicus*.

The selective bibliography which follows contains about 2300 classified entries, partly annotated, with author and subject indexes. The bibliography is restricted to books, with the exception of the articles in the *Reviews of Modern Physics* and some of the *Ergebnisse der Mathematik*. The number of references to a given subject can be briefly indicated. Under "Electric Engineering" the three subdivisions "General," "Communications," and "Power" yield 28, 9, and 20 titles respectively, none being of later date than 1945. "Nuclear Physics" gives 33 references, also limited to 1945. One reference to a 1946 publication occurs in 36 titles under "Atomic Physics" and two in 33 titles under "Electronics." This does not alter the fact that no important omissions were found in a fairly broad check; it demonstrates, however, the need for periodic revision which this work merits. The bibliographic details reveal few faults and will be particularly welcome to librarians.

C. F. A. MARMON.

PHYSICS

A University Text Book of Physics. Vol. I: Properties of Matter.

By J. H. POYNTING, Sc.D., F.R.S., and SIR J. J. THOMSON, O.M., M.A., F.R.S. Revised by G. W. TODD, M.A., D.Sc., F.Inst.P. Fourteenth edition. [Pp. viii + 278, with 174 figures.] (London: Charles Griffin & Co., Ltd., 1947. 20s. net.)

ACCORDING to the publishers' announcement, the companion volumes to *Properties of Matter* in this *University Text Book of Physics* are to be new works by present-day authors rather than revised versions of the originals by Poynting and Thomson. It is to be hoped that the decision to revise but not to replace the first volume was not taken in the belief that in the last thirty years or so only insignificant advances have been made in the subjects dealt with.

While it is true that Poynting and Thomson's original exposition of classical Properties of Matter can scarcely be improved, yet the student may possibly gain from this revised edition the false impression that many of the topics which it treats have long since ceased to engage the serious attention of physicists. Thus the chapters on the compressibility of liquids and the relation between the pressure and volume of gases might surely have been extended to include some mention of the high-pressure work which has been done since the time of Amagat. Bridgman's name does not appear in the index, although there is a reference to his work on viscosity. Again, in connection with the drop-weight method of surface-tension determination, Rayleigh's statement that it is sufficiently accurate for many purposes to assume that the correcting factor has the value 3.8 cannot now be regarded as the last word on the subject—the Harkins and Brown corrections are nearly thirty years old—and Poynting and Thomson's remark that most observers who have used the method seem to have adopted the value 2.7 should now be deleted, true though it was when first written.

These are not the only occasions on which the usefulness of the book is somewhat diminished by the omission of short references to recent developments, although this criticism does not apply to the chapters on g and G , where mention is now made of the Eötvös torsion balance and of Heyl's work.

R. C. B.

Time and Thermodynamics. By A. R. UBBELOHDE. [Pp. viii + 110.]
(London: Oxford University Press, 1947. 6s. net.)

THE title of this little work does not seem to be very appropriate. In the early pages the opinion is expressed that the methods of thermodynamics promise "fresh insight into the nature of time." On reading this one naturally asks what kind of statement could be regarded as giving "insight into the nature of time"? The theory of relativity, for example, can hardly be said to tell us anything about the *nature* of time, though it may tell us something about the relationship between temporal and spatial co-ordinates. All that one can gather about it from this book amounts to no more than is contained in the statement that entropy "gives some sort of measure of change in spontaneous processes," which is not very helpful.

It is not at all easy to understand the author's attitude to thermodynamics. The trend towards increasing entropy, we read, "arises from the nature of vision," and there is the even more puzzling pronouncement that the "Primacy of Vision is summed up by the Uncertainty Principle." One gathers that "vision" includes observation by means of radiation. Apparently the author's physical world is limited to what is observed. "Objects that cannot be apprehended do not exist so far as the science of mensuration is concerned." This seems to be a modern variant of Berkeley's *esse est percipi*. Some metaphysical assumptions seem to be needed by physicists and chemists—for example the assumption that there is a physical world which is independent of the observer or of being observed. Planck approved of this. Perhaps the author does not accept this postulate of an independently existing real physical world. Certainly his view of the dependence or partial dependence of the trend towards increasing entropy on "vision" is difficult to reconcile with it.

The reference to Carnot's work will mislead any reader not previously acquainted with it. It is perhaps worth while to quote, in this connection, a passage from pp. 36 and 37, since it gives some idea of the style in which the book is written, as well as of the author's views of thermodynamic origins. Speaking of the familiar reversed engine (refrigerator) he says:

"But Carnot's speculations did not refer to the practical convenience of keeping Friday's fish for Sunday's supper. His aim was a much more profitable one, if only it could have been realized. Would it be possible to find two engines with such efficiencies that, when one is made to drive the other in reverse, the refrigerator could be made to abstract enough heat from the surroundings at the lower temperature, and supply enough heat at the higher temperature to work the driving engine *without the need for any outside supply of fuel?*"

Carnot, of course, had no thought at all of getting work done at the expense of heat; but by the passage (or descent) of heat (a strictly conserved thing as he believed at that time) from a higher to a lower temperature. His fundamental *premisses* were (a) conservation of caloric and (b) the impossibility of the *perpetuum mobile*. "Par nos premières opérations" (*vide Réflexions sur la Puissance Motrice du Feu*) "il y avait eu à la fois production de puissance motrice et transport du calorique du corps A au corps B; par les opérations inverses, il y a à la fois dépense de puissance motrice et retour du calorique du corps B au corps A." Any gain in *puissance motrice* would be "tout à fait contraire . . . aux lois de la mécanique et de la saine physique; . . ."

There is a chapter on Thermodynamics and Life, in which the question of the possibility of "disentropic" behaviour of living organisms is discussed, and it may be added there are some errors which are probably slips or misprints: e.g. $\frac{1}{2}mc^2$ for the energy of the mass m ; a wrong description of the bending of red and blue light by a prism and of the Lorentz time distortion.

W. W.

Methods of Measuring Temperature. By EZER GRIFFITHS, D.Sc., F.R.S. Third edition. [Pp. x + 223, with 91 figures.] (London: Charles Griffin & Co., Ltd., 1947. 20s. net.)

It is twenty-two years since the second edition of this well-known book appeared, and for the third the type has been re-set and some modifications made, all of which changes are helpful. There are in all about twenty extra pages.

The main omission is the description of the Smith Difference Bridge used with a resistance thermometer. The additions include brief discussions of the scale of temperature below -200°C ., mercury-in-steel thermometers, Seger cones, melting-points of metals of the calcium family, and more information on thermocouples. There is a new table of uncertainties in melting-points, and three valuable additional appendices: I, on the International Temperature Scale (based on the revised specification issued by the Bureau International des Poids et Mesures in 1934); III, on Temperature Indicating Paints, and IV, a collected table of melting-points. A number of recent references have been added.

The book remains a sound and practical text. Since it is designed to extend the treatment given in standard text-books on heat, it might be worth while in a future edition to omit some of the material of purely historical interest and use the space to give more information on the measurement of very low temperatures. It would help to include more dates of the measurements listed in the various tables, and also to convert from Hefner candles to the new International Candle (e.g. on p. 161).

F. A. V.

Fundamental Principles and Applications of Induction Heating.

By "HEAT-TREATER." [Pp. 147, with 104 figures.] (London: Chapman & Hall, Ltd., 1947. 10s. 6d. net.)

AN author whose *nom de plume* implies that he is actively engaged in the practical business of heat treatment of materials can be expected to write a book about practical matters, can be excused if its style is lacking in scientific precision and order, but cannot be forgiven for implying by a misleading title that the book is a scientific treatise on the subject.

The modest claim of the introduction to Induction Heating, "to indicate the very great possibilities of this method of heating and the wide range of processes to which it can be adapted," is adequately substantiated. Over a half of the text is devoted to description, with numerous illustrative photographs and diagrams, of applications of the principle of heating metals, both magnetic and non-magnetic, by subjecting them to alternating magnetic fields. The description tends to be discursive and uncritical, with many and frequent repetitions, but does cover the field of application fairly completely.

The fundamental principles of Induction Heating, mentioned in the title,

are described in sketchy outline. Quantitative theoretical treatment is not given, except for the quotation of several formulæ from original papers without any attempt at assimilating them into the body of the text or reducing them to a common nomenclature. The chapter on Principles of Induction is no doubt an attempt to explain them to the uninitiated, but the intention, however laudable, is unlikely to be here fulfilled; the reader initiated in electro-magnetism will find them no more satisfactory.

H. M.

Achievements in Optics. By A. BOUWERS. [Pp. viii + 135, with 64 figures.] (Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1946. 12s. net.)

It is remarkable that, in spite of the German occupation, a good deal of technological research was carried out in Holland, France, and elsewhere during the war; and some of it was, moreover, successfully kept secret from the invaders. Consequently it has only recently become possible for other workers to learn what was accomplished under such difficult conditions. This little book is the first of a projected series dealing with the Progress of Research in Holland during the war and was prepared, we learn, before the end of hostilities there.

Perhaps the most outstanding Dutch contribution to Optics, published just *before* the war, was the "Phase contrast" method of Prof. F. Zernicke of Gröningen; the development of the relevant theory was carried forward in the war period, and a useful summary is given at the end of this book.

It is interesting to learn that the idea of employing a thick meniscus lens to replace the aspheric plate in Schmidt cameras, as suggested by Maksutov in 1944 (and in a modified form also by Gabor), had been discussed in Holland by Bouwers in 1941 and 1942. References are given to the relevant patent applications. A useful theoretical account of the relative advantages of the Schmidt and meniscus systems is given, and a number of ingeniously designed reflecting telescopes and microscopes are described.

Van Albada, well known for his stereoscopic lenses before the war, has contributed some useful ideas concerning graphical methods in optical design, and a clear summary is given of these.

Later sections of the book deal with mathematical researches on aberrations by J. Korringa and W. G. Stephan, carried out under the direction of Prof. A. C. S. van Heel of the Technical University of Delft. These are extensions of the well-known fundamental work of T. Smith on the Hamiltonian functions. In particular, Stephan has shown how to use his formulæ for the numerical calculation of higher aberrations of typical lenses; his work may be compared with calculations published in the *Proceedings of the Physical Society* by Buchdahl.

Yet another discussion on aberrations is an extract from the thesis (1942) of B. R. A. Nijboer, sometime pupil of Zernicke, in which a neat mathematical device has been used to simplify considerably the expression of the aberrational displacements. A new scheme of classification of the aberrations is suggested.

There are other short articles dealing with such questions as the modifications in image characteristics and properties, which occur as the result of using large-aperture systems. Such questions have also attracted attention in Great Britain.

While the theoretical articles are probably too condensed to give more

than a general idea of the scope of the work, the whole book constitutes a very useful little work of reference, and will be read with much interest by all optical workers.

L. C. MARTIN.

Photography by Infra-red: Its Principles and Applications.

By WALTER CLARK, Ph.D., F.R.P.S., F.P.S.A. Second edition. [Pp. xviii + 472, with frontispiece and 93 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1946. 36s. net.)

THE author of a book on some special application of photography, such as photography by infra-red, must be faced with a difficult problem. So much of the principles and practice of infra-red photography is common to all branches of photography that the author has a wide choice between two greatly separated extremes. One extreme is a book on the principles and practice of general photography, with an appendix on infra-red photography and its applications, and the other is a book confined solely to infra-red photography, which could be no more than a supplement to a standard photographic text-book. Dr. Clark has made a most successful compromise. Of the sixteen chapters, not more than four deal with relevant general photographic principles and practice. This is probably the minimum introductory matter which is necessary to make the book self-sufficient to any reader with some acquaintance with ordinary photographic practice. The needs of the practitioner are served to the extent of including a chapter on the design and equipment of a darkroom, the preparation of processing solutions and defects on sensitive materials produced during processing; perhaps in some future edition Dr. Clark may provide the theorist with a brief chapter on the interesting mechanism of the photolytic process, and the chemistry of development and fixation.

These chapters on general photography are comprehensive and concise, that on dye-sensitising being especially meritorious. Chapter IV is an admirable summary of the sensitometric and other characteristics of photographic materials, in spite of the fact that this is the only chapter in which the reviewer found a few minor inaccuracies. On p. 65, one reads the surprising statement that graininess of a material is affected by the definition of the image given by the camera lens.

However, when Dr. Clark deals with the applications of infra-red photography, one can only sit at the foot of the master, filled with admiration of his knowledge of his subject, and with gratitude for the considerable labour of collecting the widely scattered material and presenting it so attractively. The whole range of infra-red photography has been covered in considerable detail, and each chapter contains a most comprehensive bibliography. The author has been sufficiently helpful to include chapters on sources of infra-red radiation, and the penetration of infra-red through fog and haze, and the appendix contains a list of infra-red materials available commercially. Several new sections have been added in the second edition, notably a chapter on camouflage detection, and a section on forest survey from the air. The illustrations are excellent, with the exception of Fig. 13, "Typical reciprocity curve." This is typical of those produced on drawing-boards for illustrating photographic literature, not of the curves found in practice. The references show that the author has spared no pains to bring the contents quite up to

date, and one finds continental as well as allied wartime literature included therein.

This is a book which can be recommended unreservedly as a most complete and authoritative treatise on the subject, and it is likely to be the standard text-book for many years to come.

H. B.

Light, Vision and Seeing. By M. LUCKIESH, D.Sc., D.E. [Pp. xvi + 323, with 16 plates and 83 figures.] (New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1944 (fourth printing, 1946). 25s. net.)

THE theme of this book is summarised in the words "Better Light—Better Sight," which is the slogan of an American "campaign" to which the author dedicates his work. Of the eighteen chapters in the book, the first five develop this theme in a general way, and include a sketch of the history of artificial lighting. The major part of the book is devoted to "a simplified presentation of the relationships of light, brightness, vision, lighting and seeing" for the benefit of anyone who has a practical interest in the ease of seeing.

It is always difficult to write for the "general" reader upon the detailed work undertaken by the scientist to determine the conditions most favourable for human well-being. The author's success in this endeavour is best indicated by the fact that this is the fourth printing of his book.

Four chapters are given to an account of the effects of varying the chief objective factors involved in every visual task. These variables are (1) the size of the objects of vision or their details; (2) the contrast by which they are distinguished; (3) their brightness; and (4) the time available for seeing. The discussion here, and, indeed, throughout the book, centres around the experimental work which the author and his colleagues have done during the past thirty years. In this sense, it is a parochial book. The author says, however, that from this work has developed "much of a new structure of knowledge properly known as the science of seeing." Unless Ruskin erred when, early in 1872, he told his Oxford students he would speak of "the science of seeing," this branch of knowledge has a wider scope and a longer history than the author supposes.

In later chapters, the author describes his method of measuring the visibility of external objects, as well as the results of his studies of certain physiological variables under different objective conditions. A critical review of this work cannot be given here: suffice it to say that some of the author's findings are not confirmed by those of other investigators. The measurement of supra-threshold visibility involves certain assumptions, which the author claims are axiomatic because they are necessary for this purpose. Other "axioms," stated in the chapter on Ease of Seeing, seem to beg the question at issue.

Subsequent chapters deal with Brightness Engineering, Light and Colour, Efficiency, Safety and Easy Seeing, and, finally, with the answers to 100 selected questions on the subjects of light and vision. The book is well produced, and there is much in it of interest and value.

H. C. W.

Elementary Applied Aerodynamics. By P. E. HEMKE. [Pp. viii + 231, with 130 figures.] (New York: Prentice-Hall, Inc.; London: Constable & Co., Ltd., 1946. 26s. net.)

THIS book is written as a text for a first course in applied aerodynamics. The treatment is based on a combination of the approach by theory and by observation; but the balance of the mixture cannot be commended with any enthusiasm.

The theoretical treatment consists throughout of a statement of first principles, followed by quotation of advanced formulæ, without proof. References are given to other works in which proofs can be found. If a student reading this text can make use of the references successfully he is likely to find this volume useful only as a guide to other reading; otherwise, he will find it useful only as a somewhat incomplete catalogue of formulæ. That part of the treatment in which reference is made to experiment is perhaps better, but it is, in places, out of date.

The volume contains a number of errors of omission and of fact. The omission of all but the scantiest of references to the more modern ideas on aerofoil design is rendered the more unacceptable because the reader is given the impression that such ideas are very undeveloped. The statement, often repeated, that there is no basis for the calculation of the drag of aerofoils neglects completely the successful work which has been done on this subject. The confusion between boundary layer transition and separation in Chapter 8 is inexcusable; had the chapter dealing with boundary layer been a better one, this could hardly have arisen.

The text contains a number of worked examples and exercises.

A. A. HALL.

Aircraft Strength of Materials. By H. D. CONWAY, M.A., Ph.D. [Pp. viii + 256, with 154 figures.] (London: Chapman & Hall, Ltd., 1947. 21s. net.)

THIS is a very useful contribution to the literature on the strength of materials, and it is to be recommended for aircraft draughtsmen and junior stressemen. Elementary principles are developed and, where a particular mathematical procedure is required, it is usually discussed in some detail. This remark applies to second moments of area, curvature and the funicular polygon. One exception is the treatment of Fourier series, which is rather obscure on pp. 119, 120. Other minor criticisms are (i) principal axes of a beam section are not defined, so that in some instances (e.g. § 19) the treatment is misleading; (ii) the term "elastic limit" is introduced (p. 17), but not defined: it cannot be assumed to be the same as the "limit of proportionality" for all materials.

A more serious criticism is that the reader is not warned that the results obtained by theoretical methods are frequently unreliable when applied to practical problems. This is particularly true of the theory of beams and struts and, although an adequate treatment of the practical complications is beyond the scope of a book of this nature, there should be clear indications of the kind of adjustments that have to be made to the theoretical results.

In spite of its title, this book will be found to be very useful to any mechanical or civil engineer. It does not, in point of fact, refer to some of the unique design criteria used by aircraft designers as distinct from other engineers.

B. A. E.

The Escalator Method in Engineering and Vibration Problems.

By JOSEPH MORRIS. [Pp. xvi + 270, with 64 figures.] (London: Chapman & Hall, Ltd., 1947. 21s. net.)

THE escalator method, to which the reader is introduced in Chapter 10 of this book, was devised by Morris and Head for the numerical solution of a Lagrangian system of vibration equations without the evaluation of large order determinants. The procedure in "forward" escalation is first to obtain the normal frequencies and modes of the second order system (say) derived by suppressing all but two of the generalised co-ordinates, then to obtain the frequency equation and hence the normal frequencies and modes of the third order system obtained by including another co-ordinate, and so on. The method owes its effectiveness to the fact that the frequency equation for the augmented system at any stage can be expressed in a simple form in terms of the normal frequencies and modal ratios of the preceding system, as is also the case for the modal ratios of the augmented system, when its frequency equation has been solved. The method can be applied equally well in reverse and possesses simple and effective numerical checks.

The first seven chapters of the book give an account of the theory and methods of analysis of statically loaded structures, and with this as foundation the next four chapters deal with the problem of vibrations. Methods of determining the normal frequencies and modes are described in Chapter 10 with various other applications of the escalator method, which is the main feature of this chapter. In the later chapters the methods developed are applied to problems arising in aircraft engineering, and there is a final chapter on the theory of vibration dampers.

Although the exposition is not uniformly clear throughout, and in some cases the correlation of a diagram and the relevant text is obscure, the book as a whole is of great interest, and the vigorous and effective attack on the complicated problems of the later chapters makes stimulating reading. The reader who is actively interested in vibration and allied problems occurring in aircraft engineering and other fields should find it of great value.

L. A. W.

A Manual of Vacuum Practice. By L. H. MARTIN, Ph.D., and R. D.

HILL, M.Sc. [Pp. x + 120, with 56 figures and 3 plates.] (Melbourne: The University Press; London: Oxford University Press, 1947. 10s. 6d. net.)

THE first impressions of this little book are pleasing. It is written in a clear and concise style, quite well printed for the times, with many good diagrams. The binding makes use of a helical wire, which certainly enables the opened page to keep flat, but how the book will stand up to continuous use in the laboratory remains to be seen. One point of criticism in the printing is the layout of some of the equations, where the use of the solidus without sufficient brackets leads to ambiguity.

The book is intended to assist post-graduate students in the first years of their research, and does contain much useful up-to-date information on the production and measurement of high vacua, and the construction of vacuum systems. The authors have selected with discrimination descriptions of "gadgets" and "dodges" from the many which continue to appear in the various journals, and have added some useful tips from their own experience. The book by itself would not, however, enable a student new to the subject

to construct and use vacuum systems without some guidance from a more experienced person. The authors give little or no information on such practical points as: the best way of greasing a tap; the annealing of glass; the preparation of conducting coatings on glass; the design and positioning of drying-tubes; the construction of electrode systems, including vacuum and hydrogen furnacing. The constant use of the book by the student would, however, save the supervisor much time, since the advice given is sound, and there are few errors and practically no misprints. The book can be recommended for use in the honours physics laboratory, as well as by those commencing research.

F. A. V.

Klystron Tubes. By A. E. HARRISON. [Pp. x + 271, with 142 figures and 13 charts.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 17s. 6d. net.)

THE literature of electromagnetic radiations of frequencies above 1000 Mc/s was until two years ago almost entirely in the form of a few published papers and many unpublished wartime reports. This volume is a valuable contribution to the appearance of comprehensive texts in book form, and is based on the Sperry Klystron Manual with considerable new material and theoretical analysis.

The Klystron (*i.e.* the electron valve incorporating velocity modulation, interchange of energy between electron beam and resonant cavities, and a drift space for electron bunching) has a versatility to which the author does ample justice. Its operation as amplifier, frequency multiplier, reflex and double resonator oscillator, is described in five of the fourteen chapters, and a further six chapters are devoted to the theory of cavity resonators, energy interchange, electron bunching, and modulation, and to the principles of construction. This section, the bulk of the book, is an admirable example of mathematical analysis accompanied by a parallel verbal description that is commendably clear.

Useful practical information on the operation of Klystrons adds another two chapters. The final chapter on Microwave Measurements attempts to cover far too large a field, and is of doubtful utility. Appendices give design charts on resonator cavities and on operating efficiencies, and also a long but unclassified bibliography.

The omission of the less tractable refinements of theory, without giving specific references to relevant papers, and the omission of references to recent British papers, are blemishes in an otherwise excellent and attractive work.

H. M.

Electronics and their Application in Industry and Research.

Edited by BERNARD LOVELL, O.B.E., B.Sc., Ph.D., F.Inst.P. [Pp. xvi + 660, with 404 figures.] (London: The Pilot Press, Ltd., 1947. 42s. net.)

THE aim of this book is stated by the Editor to be "to collect together examples of important advances in the science and use of electronics which have occurred during the past few years." An Introduction by the Editor, outlining the origin and growth of electronics—without fulfilling that long-felt want of defining what is meant by "electronics"—and giving some details of the influence of electronics during the recent war and a glimpse into the

future, is followed by a general chapter on Electron Physics by F. A. Vick. This is a very sound chapter and, besides giving an understandable outline of the basic principles involved, touches briefly on the phenomena occurring in the devices described in later chapters, in some of which it might have been referred to with profit.

Each of the succeeding chapters is devoted either to electronic devices—Photocells for the Visible and Ultra-violet by H. G. Lubszynski, Recent Advances in Photo-cells for the Infra-red by A. Elliott (a timely review of the development and properties of thallium sulphide, lead sulphide, lead selenide and lead telluride cells), an excellent chapter on The Electronic Generation of Television Signals by J. D. McGee, Thermionic Valves for very High Frequencies (including the Klystron, the Reflex Klystron and the Magnetron—some mention of the effect of secondary emission in the performance of the latter might have been mentioned) by F. C. Thompson, and The Betatron by J. D. Craggs—or to the application of such devices—Radar by R. A. Smith (another excellent chapter), Control Applications of Cold Cathode Valves by L. Atkinson, High Frequency Heating and A Moisture Content Control Equipment by H. Wood (the latter chapter departing rather from the general review nature of the rest of the book), Electronics Applied to Servo-mechanisms by F. H. Belsey, Electronics in Medicine by L. D. Grimmett, Electronics in Physiology by R. J. Pumphrey, and Electron Microscopy and Electron Diffraction by V. E. Coslett.

Each chapter is complete in itself and is followed in most cases by an excellent list of References (the one after the chapter on Valves for Very High Frequencies could perhaps have been wider in its scope). They are all of a very high standard, as would be expected on reading through the list of authors, and, while there is a certain amount of overlapping, the pitfall of contradiction appears to have been avoided.

The production of the book is excellent with one small, but very irritating, exception. Throughout the book the figure 1 appears to be from a different set of type from the rest; it is to be hoped that this error may be corrected in future editions of the book.

The aim of the book, as stated in the quotation given above, is very well achieved, as is also the "exclusion of subjects already dealt with adequately" elsewhere, although the chapter on Photocells for the Visible and Ultra-violet is perhaps the least successful in this direction.

The reader is amply assisted in finding his way about the book by excellent lists of contents, illustrations and tables, and by very complete subject and author indexes; one might doubt, however, the desirability of including a recipient of acknowledgments in the latter.

It is perhaps unfortunate that a book of this type should be the vehicle of, no doubt unintentional, advertising—particularly the chapter on A Moisture Control Equipment mentioned above—but the book can nevertheless be recommended as a competent survey of the field it sets out to cover.

N. L.

A Survey of the Principles and Practice of Wave Guides. By L. G. H. HUXLEY, M.A., D.Phil. [Pp. xii + 328, with 148 figures.] (Cambridge: at the University Press, 1947. 21s. net.)

This book is the first of a new series of Monographs dealing with the advances in radio technique made during the war. Although the propagation of electro-

magnetic waves through metal tubes has been studied theoretically for about half a century, it was the introduction of centimetre wave radar about 1940 that caused the development of wave-guide technique to become of the greatest practical importance. Much of this development took place at the Telecommunications Research Establishment of the Ministry of Supply, and Dr. Huxley's book is based on courses of lectures given in the Radar School of the Establishment.

The reader who is not mathematically inclined will find the first six chapters provide a very clear exposition of the phenomena associated with wave propagation in guides, and of many practical applications of wave-guide systems. The seventh and last chapter gives a formal mathematical treatment of various aspects of the subject.

Chapters 1-3 provide a clear treatment of the transverse fields in electromagnetic waves and of the general features of progressive electromagnetic waves in metal wave guides. The propagation of waves in different modes is described, and the appropriate formulae are derived for the field components and the attenuation of the waves. Chapter 4 deals with practical wave-guide techniques, and will prove most useful to the experimental worker in the subject. The fifth chapter, which occupies nearly one-third of the book, is entitled Wave-guide Impedance and Further Techniques, and contains a general treatment of transmission line circuit theory, from which are developed the corresponding formulae for transmission in wave guides. The important concepts of wave-guide impedance and normalised impedance are introduced, and their physical interpretation is illustrated by reference to many practical techniques used in the development of radar systems. Chapter 6 deals with cavity resonators and, while not intended to be a complete account of the subject, illustrates in a simple manner the relation of the theory of resonators to that of wave guides. This chapter also contains an indication of the manner in which resonant cavities may be used for a variety of purposes, including that of the measurement of the properties of dielectrics at very high radio frequencies.

The book is well produced and liberally illustrated, and it is very satisfactory to find that all this useful material, which previously existed only in confidential government reports, is now made available for all students and scientific workers in this and allied subjects.

R. L. S.-R.

Records and Research in Engineering and Industrial Science.

By J. EDWIN HOLMSTROM, B.Sc.(Eng.), Ph.D.(Econ.), A.C.G.I., A.M.I.C.E. Second edition. [Pp. xii + 366, with 9 figures.] (London: Chapman & Hall, Ltd., 1947. 21s. net.)

If one can define a central theme in this excellent book it is that engineering science is a co-operative affair. Each chapter illustrates a different aspect of this theme, and supplies just the sort of information that the scientist or engineer needs in his capacity as a collaborator.

The first five chapters are concerned with the fundamentals of scientific method, the pooling of knowledge, invention and patents, standardisation and the organisation of research in this country, the more important learned societies and their various activities, the work of international organisations. Chapters 6 and 7 deal with libraries and other sources of information, recording, abstracting, filing and indexing. Systems of classification are discussed.

and there are useful hints on cross referencing and automatic sorting systems. In the eighth chapter advice is given on the expression and transmission of facts and ideas, by both graphical and photographic methods, by mathematical symbols and by the spoken and written word. Sound as the advice is in general, the present reviewer cannot agree with Holmstrom when he quotes the following sentence as a model of good presentation :

"The Kanda river, 90 ft. wide, was underpassed after cofferdams had been formed to divert the flow through a steel flume 20 ft. wide, 60 ft. long, and 11 ft. high, to provide for barge traffic, which was lowered to rest on two lines of sheet-piling previously driven to bed level across the river."

Mr. Holmstrom likes this monstrosity because so many significant statements are compressed into 55 words, but a page or two of such overcrowded writing is most exhausting. One misses all but a small number of the significant statements when they are presented at a more rapid pace than the human mind can follow. It may be mentioned that Mr. Holstrom's practice is better than his precept, his style is easy to follow and admirably lucid, even down to the superficial, but no less important, matter of punctuation.

The ninth chapter is concerned with foreign languages and translation from them, and the tenth and final one is on the relation of the technician to his job. In this, questions of personality, education and salaries are discussed.

The book is to be recommended to every engineer or scientist who would learn to find his way about the world into which his profession takes him.

REGINALD O. KAPP.

Sub-station Practice. By T. H. CARR, M.Inst.C.E., M.I.Mech.E., M.I.E.E. [Pp. viii + 396, with 287 figures.] (London : Chapman & Hall, Ltd., 1947. 32s. net.)

THIS book contains a great deal of information of a purely practical kind, very little theory, very little discussion of principles, very little explanation. It is clearly intended as a book of reference. In each chapter some aspect of sub-station design is discussed, such as constructional work, layout of plant, switch gear, transformers, converting plant. There is a chapter on organisation and control and another on technical considerations and calculations. This opens with a numerical example of the calculation of short-circuit burdens. No explanation of the method is given, nor of the meaning of any of the quantities. To follow the calculation a student would have to know enough not to need what is said in the book. The example is unfortunately not an isolated one. Later in the chapter surges are discussed, and the only type of surge mentioned is that from an oscillatory discharge, one that occurs in the now obsolete systems where the neutral point is floating. The far more important lightning and switching surges are not referred to.

The practical engineer would find the book more valuable for reference purposes if, within chapters, the material were arranged to some logical and consistent system, alphabetic or other. As it is, he may have to read through a whole chapter in order to obtain enlightenment on some specific point. The choice and sequence of sub-headings seem to follow no plan. The opening of sub-sections and individual paragraphs give little, if any, indication of what is to follow ; and successive sentences introduce a perpetual change of theme. Instead of the reader's attention being taken along a straight course, he is forced to follow a zig-zag one. Thus, in the opening paragraph

to the chapter on Electrical Protective Equipment, the first sentence says that some of the principal systems will be dealt with, the second sentence refers to capital cost of a protective system, the third mentions a possible, and not very logical, classification, the fourth the function of protective gear, the fifth and sixth operation, the seventh and eighth some definitions. None of these sentences is so worded as to indicate that something more will be said on the subject or where it will be found, though, in fact, here and there some of the items mentioned thus quickly do receive further elaboration. A good deal of work must have gone to the collection of so much information. It is all the more regrettable that the labour will have been very largely wasted through so much bad packing.

REGINALD O. KAPP.

Manual for Process Engineering Calculations. By LOYAL CLARKE.
Chemical Engineering Series. [Pp. x + 438, with 145 figures.]
(New York and London : McGraw-Hill Book Co., Inc., 1947. 30s. net.)

THIS book is presented in the form of a manual for the use of engineers engaged in the design and operation of chemical and other forms of processing plant. The author, who is chemical engineer to the Celanese Corporation of America, has collected, in this single volume, data and knowledge from a wide field, and has presented it in a ready form for those practising in this rather specialised profession.

After a short opening chapter giving general references and nomenclature, four chapters are devoted to mathematical data, conversion tables, physical and mechanical properties and thermodynamic data respectively. These are very informative and are profusely illustrated by tables and graphs and by nomographs for rapid estimations where the data lends itself to this artifice.

The section which follows contains chapters on specialised aspects of the work, and in them is given information on piping, hydraulics and fluid flow; heat transfer; combustion; power; pumps; fans, blowers and compressors. These chapters include important data together with a résumé of fundamental facts, although in those parts of the text bordering on purely mechanical or electrical engineering the treatment is apt to be sketchy; for example, only four pages are devoted to electricity in the chapter on power.

The concluding portion of the book gives chapters on absorption, stripping and distillation; and on the treatment of water and its application for cooling. The text on absorption and distillation is rather specialised for an intelligent first reading by an ordinary engineer, but is supported by additional references. The average engineer, on the other hand, would consider the portion on the treatment of water to be rather brief, although the cooling tower theory given is precise without being under-informative.

Into the space at his disposal the author has crammed information which could only be obtained otherwise by reference to a small library. Compensation is given for any brevity by generous references to English and American publications. Worked numerical examples have been judiciously interposed through the text, and the book can be thoroughly recommended to engineers and other technicians working in the field it covers.

W. A. J. C.

CHEMISTRY

A Rational Approach to Chemical Principles. By J. A. CRANSTON, D.Sc., F.R.I.C. [Pp. xii + 211, with 37 figures.] (London and Glasgow: Blackie & Son, Ltd., 1947. 8s. 6d. net.)

WHILE appreciating that the historical approach to chemistry reflects the logical development of chemical principles, and affords an excellent exercise in scientific method, the author has here abandoned this in favour of a rational approach. It is felt that the growth of the subject is so great and so rapid that some material must be cut out of an introduction, and the rational treatment is therefore adopted as a short cut to a clear understanding of modern physico-chemical principles.

Such a treatment has much to commend it. In this age of wireless, the beginner is often familiar with some of our present-day scientific concepts and ideas, and, by an early development of these, he can be saved the task, often painful, of re-orientating in the light of modern theories the knowledge he would have acquired by the traditional approach. The first chapter of this new introduction deals, therefore, with the modern views concerning the structure of the atom, and from this are developed the fundamental laws of chemistry, the concepts of valency, the reactivities of the elements, and the ionic hypothesis. The book is divided into five main sections, dealing with the structure of matter, the states of matter, chemical combination, electrically-charged atoms, and chemical equilibrium. It is well written, and can be warmly recommended. It forms a valuable up-to-date introduction, one that will be most useful, not only to pupils in the higher forms of grammar schools, but also to students in their first year at the universities.

B. J.

Chemistry for our Times. By ELBERT COOK WEAVER, M.A., and LAURENCE STANDLEY FOSTER, Ph.D. [Pp. xii + 738, with frontispiece and 412 figures.] (New York and London: McGraw-Hill Book Co. Inc., 1947. 12s. 6d. net.)

"AN atomic age calls for a new approach to chemistry. Hence we are presenting an entirely new chemistry textbook. . . . We have eliminated many errors that cling to chemistry textbooks by tradition. We have also tried to strike a balance between chemistry principles and applications of chemistry." "This book centers about the individual pupil and his activities. It starts with his immediate environment . . . and then swings through the gamut of chemical actions. . . ." It is, in fact, an excellent example of that sort of thing, and will appeal to those who like it. Its forty chapters are each provided with a Summary, Questions ("Of what can we be sure when undissolved solute remains in a solution after shaking?") and often More Challenging Questions ("What is the difference between an 80-ampere-hour storage battery and a 100-ampere-hour battery?"), with proper appreciation of human frailty: "Balance the following equation (*do not write in this book*)."

It will provide lower-forms science masters with much background, not a few illustrative ideas, and a recipe for cold cream. A very large amount of information concerning elementary science, neutron bombardment, and personal anecdote is collected in its 700 pages, which at five a penny are splendid value for money.

A. A. E.

The Rare Earth Elements and their Compounds. By D. M. YOST, H. RUSSELL, Jr., and C. S. GARNER. [Pp. x + 92, with 9 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 15s. net.)

THIS volume is particularly good in describing the properties of the rare earth elements which are of special interest to modern chemists and physicists. It opens with an account of recent work on emission spectra and energy levels, which leads to a discussion of the states of oxidation which have been observed for these elements. The next chapter deals with the magnetic properties and is followed by a discussion of absorption spectra; this is expressed in terms of modern theories and refers to a number of interesting but, as yet, unsettled problems. There follows a brief account of the evidence for and against the existence of element of atomic number 61.

The chapter on methods of separation is mainly devoted to the methods used by Prandtl. It omits a number of important points; only one of them need be mentioned. The fractional crystallisation of the bromates is suggested for the separation of dysprosium and holmium, yet James and subsequent workers have found that this method gives an exceedingly slow separation and is of little practical value. In general, the scheme given can hardly be regarded as a working guide for laboratory operations.

The final chapter is concerned with the chemical and physical properties of the rare earth metals and some of their compounds. Here a number of useful and significant properties are recorded, e.g. atomic radii, oxidation-reduction potentials, etc. An appendix is devoted to the nuclear properties of these elements. Since some of them occur as fission products of uranium and some show remarkable interactions with neutrons, this appendix presents many points of interest. The intense artificial radioactivity of dysprosium and the high absorptive capacity of samarium and gadolinium for thermal neutrons have already been used in nuclear investigations. The appearance of a volume which gives data on these topics in a convenient form is to be welcomed at the present time. It will appeal to a much wider group of readers than those who are interested only in this group of elements.

S. SUGDEN.

Lecture Experiments in Chemistry. By G. FOWLES, M.Sc., A.R.I.C. Third edition with Supplement. [Pp. xvi + 612, with 158 figures.] (London: G. Bell & Sons, Ltd., 1947. 25s. net.)

THERE is a lot of difference between a recipe book for class demonstrations and a manual on the teaching of chemistry with ample experimental illustration. Teachers of chemistry, whether in school, technical college, or university, know Mr. Fowles's book well, and value it not only for the abundant technical information that it imparts but also for its penetration into the mind of the pupil. The experience of a lifetime has gone into its making, and a sound judgment into the selection of its content. In particular, the younger teacher will welcome its guidance, not only in taking precautions which will avoid physical accident, but also in taking precautions which will prevent misconception or confusion in the minds of his pupils. Stilted phraseology is avoided, and simple terms are used to express simple ideas; neither does the author hesitate to point a moral when occasion invites ("The result is spectacular and of decided hygienic significance," says he of an experiment entitled "Bacteria on Unwashed Hands"). The third edition

of this classic is enlarged by a supplement in two parts: the first comprises additional experiments on topics previously treated, as well as some experiments on subjects—osmotic pressure and the Marsh test for arsenic are examples—not already considered; the second is a sequence of simple experiments designed to illustrate the study of a chemical reaction. In the latter section simple experiments illustrating the Landolt reaction, the effect of temperature on reaction velocity, the effect of pressure on the position of equilibrium, the common ion effect, etc., are presented. Before placing a copy of this book in a particularly accessible position on the laboratory bookshelf, most teachers will turn again to the first Appendix, wherein reside many sound observations concerning their aims and methods. Then, if they have reason to think that they have already interested their pupils, they will take the precaution of placing another on their own desk.

A. A. E.

The Elements of Physical Chemistry. By F. W. GODDARD, M.A., F.C.S., and E. J. F. JAMES, M.A., D.Phil. Second edition. [Pp. viii + 251, with 67 figures.] (London, New York, Toronto: Longmans, Green & Co., 1946. 6s.)

In the preface the authors state that it was their intention to write a concise account in logical form of the principles of physical chemistry that would be suitable for students working for the Higher School Certificate Examination. Had they succeeded, their book would have served as an invaluable introduction to the subject. Unfortunately it is in the logical presentation that the book is defective.

In discussing the theory of solutions they make no attempt to explain that the lowering of the vapour pressure, the depression of the freezing-point and the elevation of the boiling-point are all manifestations of osmotic pressure and that these properties can be shown on theoretical grounds to be inter-related. Curiously enough, they account for the enhanced values of these properties possessed by solutions of electrolytes on the basis of incomplete ionisation and give worked examples to show how the degree of ionisation may be calculated; yet no reference is made to the Arrhenius Theory until much later on in the book. It was by this type of calculation that Arrhenius attempted to justify the degree of ionisation as calculated from the conductance-ratio, so that the view that ions are osmotically active is an essential part of the Arrhenius Theory. It is not clear why the authors devote any space to the Hittorf "Transport numbers" since they make no use of them. They might have indicated how the mobilities of individual ions may be computed, and how the all-important Equivalent Conductivity of any electrolyte at Infinite Dilution can be obtained.

In dealing with Valency and Atomic Structure, the authors do not emphasise the experimental evidence, including the periodicity of the elements in the Periodic Classification, on which the Electronic Theory is actually based. Instead, the theory is expounded as if it were an established fact and such statements as "The most important aspect of Atomic Structure lies in the arrangement of the planetary electrons around the central nucleus, for it is now known that the chemical properties of an element are entirely determined by this arrangement" (p. 34) convey a false impression. Regarding the structure of the atomic nucleus, it is stated that it consists of protons and electrons! No mention of neutrons appears anywhere in the book.

In describing the structure of Isotopes, they state that, "If the nucleus is increased by one proton and one electron chemical properties of the element remain absolutely unchanged." This is too dogmatic and is probably not correct. Incidentally, it is unlikely that the student will gain much knowledge of Isotopes from this book.

Chapter IV, headed "Solution," first deals with the solubility of solids, liquids and gases in liquids with typical curves of the Binary Systems involved. Towards the end of the chapter, however, the Phase Rule is introduced and it is explained by referring to one-component systems and the system Ferric chloride-Water, yet no reference is made to the previously described Binary Systems.

Although Chapter VI is headed "Law of Mass Action," it deals mainly with the kinetics of reactions, whilst the more important chemical principle, *viz.* "equilibrium," is almost entirely ignored. This is a serious defect. The chapter ends with a brief reference to the "Activity Coefficient of Lewis" in which it is stated erroneously that the Activity Coefficient is *always less than unity*.

The apparatus illustrated on p. 203 for the measurement of hydrogen-ion concentration with the hydrogen electrode is faulty in that it lacks any arrangement whereby (a) the platinum electrode is kept directly in contact with the hydrogen gas, and (b) the Liquid Junction Potential between the two electrodes is probably eliminated.

The many questions, taken from the various Higher School Certificate examination papers, greatly add to the usefulness of the volume. As it is of the nature of a "cram" book, it is felt that it will continue to find favour with students who are receiving instruction in physical chemistry.

H. T. S. B.

X **The Chemical Kinetics of the Bacterial Cell.** By C. N. HINSHELWOOD, F.R.S. [Pp. x + 284, with 1 plate and 79 figures.] (Oxford: at the Clarendon Press, 1946. 20s. net.)

FROM time to time a review appears in which the reviewer states that, having once commenced, he was unable to lay the book down before reading it from cover to cover. This essay, as the author modestly calls it, is too full of meat for this to be possible, but the reader is held fascinated from page to page.

In building up a picture of the mechanism of operation of the cell several alternative premises are advanced as scaffolding for the development of the suitable kinetic equations. It is, of course, arguable whether mechanisms other than pure chemical kinetics exert control on growth or no, and whether structure can be interpreted on analogy with crystallisation. Thus one of the basic assumptions involves the reaction enzyme + substrate = more enzyme + product. If the nucleotides are the template on which the simple constituent compounds are adsorbed and there joined up to form the complete protein enzyme, which on this view is not regarded as a virus, we must envisage some process by which this complex product leaves the nucleotide and finds its proper place in the cell. That certain proper places exist at least is shown by the fact that all bacteria are not spherical and that flagella appear at definite positions on the cell surface.

Again, it may well be that diffusive processes, and thus reaction rates, can be controlled by suitable alteration in the permeability of the diffusion medium; *e.g.* by the alteration of the pH in a Donnan or Teorell dynamic

distribution, or again by the access or withdrawal of lipid by a protein skeleton. It is also known that both energies of activation and entropies of reaction can be affected by altering the orientation of a reactant in systems possessing a macro-structure.

The kinetics of the cell growth are envisaged as a sequence of linked enzyme reactions and the novel and valuable concept of a cyclically linked enzyme model is introduced. Here an enzyme produces a product which serves as a substrate for a second enzyme, the product of which reacts in turn with the first enzyme.

The linkage of several enzyme systems permits of complicated structures being either utilised or synthesised by a series of relatively simple reactions. In addition, the combination of enzyme and substrate is regarded as a free radical rather than as an ion or even salt. The advantage of this concept lies in the ready explanation it offers of high reactivity and their preservation from decay during resting.

Several possibilities are pointed out to account for variation. Thus it is possible, by destroying a link in the enzyme system, to raise a growth in which the enzymes which operate after the broken link are inoperative or are not formed, provided, of course, that growth is possible under these circumstances. Again, one enzyme which is to be regarded as a protein in a particular configuration may have in it a small quantity of another enzyme which under suitable conditions, *e.g.* the right substrate, may increase in quantity. Lastly, the protein pattern may be altered spontaneously. Such a process usually requires a high energy of activation, or a large change in entropy. High temperatures are precluded owing to the increase in speed of the process of denaturation; we are thus driven to the view that spontaneous mutation is possible if we are prepared to wait for it.

The essay, as good essays should be, is full of the most interesting speculations and ideas, and should be read especially by those who think that there are not many new wide horizons for the human mind to overlook.

The format, printing and binding of the volume maintain the high standard expected in a product of the Oxford Clarendon Press, and in these inflationary days it is a bargain at 20s.

ERIC K. RIDEAL.

- * **Quantitative Organische Mikroanalyse.** By F. PREGL. Fifth edition by Dr. H. ROTH. [Pp. xii + 317, with 80 figures.] (Vienna: Springer-Verlag, 1947. Swiss Frs. 32.-.)

THIS new Austrian edition of Pregl's *Micro-Analytical Methods* follows on from Dr. Grant's English edition published a year or so ago. The book is "utility" in appearance and the binding is poor, but inside is a wealth of excellent material. The style has considerably deviated from the original Pregl, although there is still retained a large proportion of the technique put forward by the pioneer of micro-analysis. To the English reader it is of special interest in that it gives some details of the advances made in micro-chemical techniques in the German speaking countries during the war years. It is, however, in striking contrast that some of the many rich additions made to the subject by English and American workers during the same period are not mentioned.

Of the recent Continental work Unterzaucher and Zimmermann have come in for special mention. The method for oxygen proposed by the former is

described in detail. This consists of combustion to carbon monoxide, absorption in iodine pentoxide and subsequent titration of liberated iodine. Zimmermann is specially mentioned in connection with the fully automatic electric combustion apparatus, the automatic pipette and an ingenious method of sulphur estimation. This latter consists of fixation as sulphide with metallic potassium, absorption of liberated hydrogen sulphide in cadmium acetate and iodometric determination. Bürger's argentometric methoxyl technique is described, and also an alternative nitrogen estimation by Lacourt. In this procedure, nitrogenous matter is reduced by hydrogen in the presence of nickel-thorium dioxide catalyst to form ammonia.

Rather superficial information is given regarding the estimation of metals, and a tardy inclusion is Roth's own modification of phosphorus estimation by the ceruleo-molybdate method.

Certain aspects of the book are disappointing. One would have liked to see included alternatives to the Van Slyke technique for amino nitrogen and for Barger's method for molecular weights. The section on electrochemical methods is grossly inadequate. The details given are insufficient for accurately performing the methods and leading references to the subject are omitted. The newer techniques in this field (*e.g.* oscillographic and amperometric) are without mention, and one feels that treated in this manner the subject would be better avoided.

To the micro-chemist the book is still the authority. One feels, however, that the time is now ripe for a text-book of wider scope, which will retain the original Pregl methods only where they have not been superseded by more advanced technique, and which will also include selected material from the vast and ever-growing literature on the subject in languages other than German.

R. F. M.

Fatty Acids: Their Chemistry and Physical Properties. By KLARE S. MARKLEY. [Pp. x + 668, with 81 figures.] (New York and London: Interscience Publishers, Inc., 1947. 60s. net.)

THE author of this most welcome volume has earned the warmest thanks of chemists and technologists for his task of bringing together for the first time the extensive and widely scattered information on fatty acids.

It may seem surprising that a book on so important a subject has not appeared earlier, but our knowledge in this field has been of somewhat unsteady growth, and, although the nature of fats was understood in 1815 (Chevreul), the conversion of oleic into elaidic acid in 1819 (Poutet), and the lead salt separation of saturated and unsaturated acids in 1828 (Gusserow), very few further advances of importance were made until well into the present century.

This renaissance was initiated mainly by improvements in the technique of vacuum fractionation and low-temperature crystallisation, which enabled pure acids to be prepared, many for the first time, and which thus opened the way for a systematic attack on the detailed structure of fats (Hilditch). Moreover, at about the same time, the surface film work of Langmuir, Adam, and Rideal, and the X-ray investigations of Piper and Müller, focused a much wider attention on the subject than hitherto, and greatly increased the tempo of research.

The author has now garnered and winnowed the resultant expanded and

diversified literature with the greatest industry and skill. No important work up to early 1946 appears to have been overlooked, and the book is packed with data and references. Yet it is no mere work of reference, for it is eminently readable, and the many aspects of the subject are treated with commendable insight and understanding.

Briefly, the book is divided into sections on the History of Fats and Waxes, The Classification and Structure, The Physical Properties, The Chemical Reactions, The Synthesis (including Biosynthesis), and The Isolation and Identification of Fatty Acids. Of the many excellent chapters, those on Crystal Properties, Oxidation, Synthesis, and the Separation of Fatty Acids are outstanding.

The printing, paper and binding are of the high standard one associates with Interscience publications.

T. MALKIN.

* **Practical Emulsions.** By H. BENNETT. Second edition. [Pp. xvi + 568.] (Brooklyn, N.Y.: Chemical Publishing Co., Inc.; London: Chapman & Hall Ltd., 1947. 48s. net.)

At first glance—and being duly impressed by the mass of references—this book, within a modest compass, seems to be almost encyclopaedic in character.

The contents pages are impressive, for there are few industries which do not have some interest in emulsions, and the Formulary, i.e. the third part of the book, is extensive, running into 22 chapters, of which the longest by far are concerned with Cosmotic, Polish and Food emulsions. The book opens with a series of ten chapters, which cover the theoretical aspects and give information in general terms about technical problems and the agents promoting emulsification, wetting and dispersion, etc. The middle section of the book is a useful collection of short but interesting papers by different authors summarising their specialist scientific background under the two headings "Emulsifying Agents" or "Emulsions."

Naturally one first turns to the subject of one's own special interest, but it looked as if it might be difficult for a novice on emulsion paints to graduate in this field. Admittedly a chemist already familiar with the techniques is provided with a large mass of information, much of it being patent detail, and a great number of formulations. Unfortunately, there seems to be no real connecting thread which would help to that end, which the author declares to be his aim (or rather one of his aims), namely a proper understanding of what is necessary to produce a good emulsion. His other aim is to give sufficiently diverse examples to build a foundation for practical work, and on the practical issue the book must be judged.

In matters of this kind so much more is required than can easily be written down, and the author disarms criticism by saying in his introduction that to produce a stable and good emulsion is not a mechanical process but an art. He also points out, after recording the desirable features of emulsions, that they would be used much more than they are if their manufacture were less difficult; in short, he says that they are unstable preparations, very susceptible to trifling impurities and variations in technique of manufacture, sensitive to heat and cold, and likely to deteriorate on ageing. It is the essence of the art to make good emulsions in spite of these handicaps. A comment must be made to the many references, over 600, to another book, *vis.*, the Bennett Chemical Formulary, which presumably gives information

not in this book. Also if trade names for materials must be used (and they are used quite extensively) it would be nice to know what they stand for chemically, and some people would like to know whence they may be obtained.

L. A. J.

 **The Technology of Adhesives.** By JOHN DELMONTE. [Pp. viii + 516, with 79 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1947. 48s. net.)

RECENT developments in the field of high polymers have provided new materials of interest as plastics, fibres, rubbers and not least as adhesives. The synthetic adhesives possess many advantages, particularly durability in service, but they are not without limitations. For some applications, the higher cost and more critical nature of the synthetics are not justified and a balanced appraisal of adhesives of all types would be of great value. Mr. Delmonte has the twofold objective of relating developments in plastics with those in adhesives and of comparing the properties and performance of synthetic adhesives with those of established glues.

The book falls into two main sections, the former dealing with individual types of adhesives and the latter with practical gluing problems. Sandwiched between these two sections is a gallant attempt to outline theories of adhesive action.

A good wine needs no bush and, in describing the synthetic adhesives, there is no need to ignore their defects. This book contains no discussion of relative costs or of dermatitis troubles. Again, the limitations of the poly-vinyl adhesives, with respect to temperature and humidity, are barely mentioned. The most useful chapters are those describing adhesives for specific materials, *e.g.* wood, plastics, rubber, metals and paper. An up-to-date survey of existing knowledge is given and, naturally, wood receives the fullest treatment; here the author relies largely on the excellent work of the U.S. Forest Products Laboratory. The concluding chapter on methods of test and specifications is less thorough; significant German work is omitted and the British Specifications mentioned have now been superseded.

Summing up, the book is comprehensive, but somewhat uncritical; it will be useful to the expert for reference, but possibly confusing for the novice, since much good material is obscured by excessive reference to the patent literature and by irrelevancies.

K. W. PEPPER.

GEOLOGY

An Introduction to Palaeontology. By A. MORLEY DAVIES, D.Sc., A.R.C.S., F.G.S. Second edition. [Pp. xi + 372, with frontispiece and 118 figures.] (London: Thomas Murby & Co., 1947. 18s. net.)

In the years following the First World War, the impetus given to the study of Physics and Chemistry affected the curricula of our secondary schools to the detriment of the biological sciences. As a consequence, Palaeontology had to be taught to students of Geology who rarely had any previous biological training. In his book Dr. Morley Davies meets this difficulty by describing minutely certain selected members of a group of fossils, and then, having shown the student methods of observation, description, and classification, passes on to the systematic study of the group in detail. The idea is a sound one, the best possible in the circumstances.

Presumably the author set out to write an elementary text-book suitable for non-biological students of Geology and "amateur fossil collectors." The result will hardly appeal to such readers, but rather to students of a more advanced type who should have the requisite biological background for its proper appreciation. In the desire for completeness both the animal and vegetable kingdoms are included, with the result that, while certain groups receive adequate description, many sub-groups are treated so briefly as to render their inclusion of little value to the student. The chapter on plants might well have been left to the palaeobotanist, requiring as it does a degree of botanical knowledge hardly to be expected of those for whom the book was written. A short account of the rise and progress of a land flora, and, in particular, of a Carboniferous swamp, would have conveyed more to the non-biological student.

The arbitrary manner in which the various fossil groups are arranged calls for criticism. It is disconcerting to pass from Mammalia to Graptolites. One wonders if the time-honoured zoological arrangement from the simple to the complex, or even the order of appearance in the geological record might not have been of more advantage to the student, emphasising as it does the story of evolution thereby revealed.

Still, the book has much to recommend it. The advanced student working through the various fossil groups will find it a mine of information; such students will also find the concluding chapter and the appendices of zonal fossils exceedingly useful for reference. Illustrations are numerous and adequate, and the book is thoroughly up to date, both as regards recent research, nomenclature, and the bibliographical changes since the appearance of the first edition.

T. M. F.

The Geology of Oxford. By W. J. ARKELL, M.A., D.Sc., F.R.S. [Pp. vi + 267, with 49 figures, 6 photographic plates and 2 folding-plates.] (Oxford: at the Clarendon Press, 1947. 20s. net.)

THE title Dr. Arkell gives his book is too modest—or it expresses an Oxonian presumption. Professedly limiting himself to an area of some 30 miles radius about Oxford, he in fact does not hesitate to wander indefinitely when occasion calls for it. The book's contents range over the geology of much of southern England, and are only by slight emphasis centred on Oxford itself. Thus, in dealing with the "solid" rocks, Dr. Arkell conceives Oxford to extend over all formations from the Lower Lias to the Pliocene. The result is to increase greatly the value of the book, which should appeal to a wide company of geologists.

Rather less than half the book is an account of Jurassic stratigraphy. The method is chronological, each series being accorded a chapter in which details of succession and outcrop are usually prefaced by brief comments on the general environment of deposition. This is perhaps the most substantial part of the book, almost all of it being the sifted product of the author's own researches.

The continuity of the stratigraphical account is unbroken to the end of the Wealden. The succeeding chapter, however, describes structures in the Jurassic-Wealden strata; while some of these are of early Cretaceous age and then follow in chronological sequence, others are purely superficial, of

Pleistocene age or later, and are out of place. The last includes camber and esculator structures, landslips, and valley bulges.

The Lower Greensand and the Upper Cretaceous rocks (including the Chalk of the Berkshire Downs) occupy three short chapters. The succeeding chapter is briefer still: it deals with Eocene stratigraphy (Reading Beds to Upper Bagshot Sands), Alpine folding, Miocene erosion, Pliocene deposition and erosion, in nine pages—a perfunctory brevity utterly disproportionate to the remainder of the book.

The elaborate and lucid account of the deposits and environments of Pleistocene times occupies some fifty pages. In it Dr. Arkell with great advantage extends his remarks to include the southern Midlands, and offers a partial correlation of the events in the upper Thames basin with those north of the Cotswold escarpment. There are some gaps in his explanation of the establishment of the primary drainage pattern, but his descriptions of the modifications of valley form and of the successive terraces are comprehensive. He adds a chapter on tæle gravels, clay-with-flints, solifluxion deposits, brickearths and other periglacial features.

In a final chapter on post-Glacial times he departs from strictly geological matters to comment on the close dependence of present and past cultural expression on the nature of the geological foundation.

The book is written with the felicity of style now expected from Dr. Arkell. It is for the greater part a thorough and penetrating account of stratigraphy and landscape expressed in language at once exact and yet not over-technical: it should appeal to the young student (and indeed the intelligent amateur) no less than to the hardy professional. It is illustrated with six photographic plates and a number of beautifully drawn maps, sections, and fossils (gastropods, lamellibranchs, brachiopods, corals, sponges—but not a single ammonite). Its format is worthy of the Clarendon Press. Within the explicit limits of its nature it is a highly satisfactory compendium, very reasonably priced.

T. N. G.

MICROSCOPY

Petrographic Micro-Technique. By A. V. WEATHERHEAD, F.G.S., F.R.M.S., A.R.P.S. [Pp. x + 102, with 94 figures.] (London: Arthur Barron, Ltd., 1947. 12s. 6d. net.)

THE sub-title of this book is *A Practical Handbook for the Preparation of Thin Sections of Rocks for Use with the Petrographical Microscope*, and the author has undoubtedly succeeded in his endeavour, as he says, "to fill a gap in geological and microscopical literature." An introductory chapter contains some elementary information for microscopists who are not geologists, on the petrological microscope, and on the nature of polarised light. Thereafter come chapters on Preliminary Treatment (collections, storage, labelling, trimming, etc.); Preparation of Thin Sections (general methods, improved methods, thickness of section, etc.)—this is the longest and most important chapter; Thin Sections (Igneous and Sedimentary Rocks); Special Methods (serial sections, pyroxylin films, clays, streak smears, etc.); Heavy Residue mounts; and, finally, Photomicrography with the Petrological Microscope.

Mr. Weatherhead writes with an authority based on almost lifelong experience and, as Dr. A. K. Wells testifies in a short foreword, his methods have proved very successful. But the reviewer thinks the book would have been

better, as well as more complete, if the author had included some account of the techniques developed by other workers, notably P. J. Robinson (*Trans. Geol. Soc. Glasgow*, XIX, 1932, Pt. 1, 141-51), who, by a method of embedding in a shellac cement and remounting with the use of plaster blocks, has produced the finest and largest thin sections the reviewer has been privileged to use.

The book is well written, and produced on good paper, with a large number of excellent illustrations. It is certain to become an indispensable adjunct to the petrological workshop and aid to the amateur worker.

G. W. T.

The Microscope : Its Theory and Applications. By J. H. WREDDEN, F.R.M.S. [Pp. xxiv + 296, with 298 figures, including 23 plates.] (London : J. & A. Churchill, Ltd., 1947. 21s. net.)

IN outline this book follows much the same course as the usual text-books, but the author draws on his experience in the industrial field, and emphasis is given to new and unusual processes in this direction.

Mr. W. E. Watson-Baker contributes an historical introduction, which many readers would wish to see greatly expanded, while in some chapters, as for example those on objectives and substage condensers, the author deals with the historical aspects as well as with the modern ones and adds much to the interest of the subject by so doing.

The chapters which deal with elementary optics are clear and concise and are followed by chapters on objectives, eyepieces and other necessary equipment. A good deal of attention is given to various types of stands, including an account of modern universal microscopes, while there is a thoroughly practical chapter on the use of the microscope and a very lucid exposition of the theory of polarised light and the application of this form of light to microscopy. Micrometry, photomicrography, light filters and details of mounting techniques are also dealt with, emphasis being given in the last to special processes.

Some comment may be made on the figures, of which many take the form of plates. It is annoying to discover that none of these figures bears a legend and that search must be made through the text to discover what they are. This, however, is a minor criticism and one which can be met should a second edition be called for. Mr. Wredden has produced a useful and attractive book on microscopy, which should be of use to a wide circle of microscopists and especially to those who use the instrument in industry.

F. W. J.

The Intelligent Use of the Microscope. By C. W. OLLIVER, A.M.I.E.E., F.R.M.S. [Pp. viii + 182, with 53 figures.] (London : Chapman & Hall, Ltd., 1947. 12s. 6d. net.)

THIS is essentially a practical book. Optical theory first receives brief treatment, followed by an account of numerical aperture and resolution. A chapter on the microscope and its components and accessories contains some very sound advice on the selection of both new and second-hand equipment, while the section on the microscope in use is another thoroughly practical piece of work. Lamps, condensers and vertical illuminators, as well as light filters, micrometry and photomicrography all receive attention, and various types of microscope are discussed in the section on special instruments.

The aim has been to give "just what was wanted, all that was wanted and nothing but what was wanted." How far this aim has been achieved depends, as the author writes, "on the reader's own way of viewing things." The beginner may well feel that in his desire to omit non-essentials the author has sacrificed clarity for brevity; the size of the book might have been doubled without fear of "padding." Nevertheless, *The Intelligent Use of the Microscope* is not just another book on microscopy: it fills a niche of its own. It is not a book that a beginner can afford to ignore, but it should prove of value to more advanced microscopists, while research workers—and there are many who use the microscope although they are not microscopists—might use the book with advantage as a practical guide, and learn the possibilities of the wonderful instrument which they employ.

F. W. J.

ZOOLOGY

La Vie des Requins. By P. BUDKER. [Pp. 277 with 22 plates and 40 figures.] (Paris: Gallimard, 1947. Frs. 325.-)

THIS is another volume in the series of *Histoires Naturelles* edited by Jean Rostand. It is very clear that the author has had an abundant experience of the sharks and dogfish, not only in their natural haunts, but also in the laboratory and as recorded in the literature from classical times to the present day. The result is a brilliant, lively account of the group, rivalling Norman's *History of Fishes*, and having the advantage over this well-known work of its more recent publication and of its specialisation. The reader is constantly interested and amused by anecdote and quotation, as well as instructed by an abundance of useful information. There is a chapter devoted to the problem of the reality of the man-eating habits of the sharks, and others which survey the history and behaviour of the Remoras and the commercial importance of shark products.

On matters of fact the author is admirably up-to-date (he will no doubt alter his views on the function of the ampullæ of Lorenzini when he has access to the English literature of the war period). Problems as diverse as those concerned in the manufacture of shark-repellents and those of the osmotic balance of fresh-water elasmobranchs are introduced in their proper context and discussed in an informed manner.

This book has no equivalent in the English language and should have a wide public in this country. But why do French authors so often diminish the value of their references to the literature by giving the context only when they feel so inclined?

W. H.

Mankind So Far. By WILLIAM HOWELLS. [Pp. xii + 319, with 7 plates and 37 figures.] (London: Sigma Books, Ltd., 1947. 16s. net.)

IN the last few years physical anthropologists in America have produced a number of text-books which serve admirably as introductions to the study of human biology. Of these, that by Prof. Hooton, entitled *Up from the Ape* is now a well-established work; that by Prof. Ashley-Montagu, *Introduction to Physical Anthropology*, is particularly useful as a student's text. The latest, by Prof. William Howells, *Mankind So Far*, maintains the high standard of exposition of its predecessors. It differs in that it is concerned mainly with one aspect of physical anthropology, that is with the story of

man's emergence and evolutionary differentiation. Prof. Howells's telling of the story is accurate and up-to-date and in these respects will fully satisfy the need of the student approaching the study of physical anthropology. The telling, moreover, is so interesting that the book can be unreservedly recommended to the general reader who is intent on gaining, even in outline, a reliable view of our present understanding of human origins.

The contents of the book are arranged in three sections to cover consecutively the primate origins and relationships of man, the varieties of fossil Man, and lastly the varieties and races of present-day Man. Chronologically, these sections deal with the Tertiary, the Pleistocene and the post-Pleistocene. A few points invite comment. Prof. Howells presents in his first section an introductory outline of evolution in general. While this will serve the purpose of the book well enough, the treatment is too simplified and compressed for the new student of human biology, who would not be made sufficiently aware of what modern evolutionary biology can offer in explanation and understanding of human evolution. The significance of the South African man apes as the most direct and crucial "proof" so far available of man's emergence from an ape-like stock is not stressed to the extent it might be, though the book was written perhaps too early for the more recent finds of Dr. Broom to be taken into account. Prof. Howells deals judiciously with the "arboreal" theory of the evolution, in the primate order, of those characteristics which reach their highest expression in man. One misses, however, here, an account of Zuckerman's findings and suggestions on the biological basis of social behaviour amongst the primates—a topic of undoubted importance to the anthropological student of human biology.

A few further comments in the same strain could be made, but even when all such points are taken together they do not detract at all from the general excellence of this very readable and informative work of Prof. Howells.

J. S. W.

Experimental Embryology in the Netherlands, 1940-1945. By M. W. WOERDEMAN and CHR. P. RAVEN. [Pp. xii + 132, with 40 figures.] (Amsterdam: Elsevier Publishing Co., Ltd.; London: Cleaver-Hume Press, Ltd., 1946. 13s. net.)

THIS admirable little book, one of a series of *Monographs on the Progress of Research in Holland during the War*, partially fills the gap made by the war in our knowledge of work in progress on the Continent. It lays before workers in this country a concise yet detailed summary of the work carried out by Prof. Woerdeman and Prof. Raven, together with their colleagues, at Amsterdam and Utrecht. That it should have been possible to produce such a wealth of results during the years of occupation and oppression in Holland must be a source of admiration to scientists in this country.

The first half of the book is devoted to the Amsterdam work, chiefly carried out on amphibian and avian material, the latter much limited owing to shortage of supply. As the result of a most interesting hypothesis, proposed by Ten Cate, that respiration (proportional to temperature) is coupled with the "intrinsic" rather than the morphogenetic development of an embryo, it seems probable that the different results of Spemann and of Woerdeman on the ability of the esculent frog to self-differentiate a lens primordium without the presence of eyecup, may be brought into line. In

Spemann's laboratory eggs were kept "fresh" at 10°-15° C., whereas at Amsterdam they were kept at 25° C., and the difference in development, hitherto explained by racial differences in the species, is probably due to physiological causes.

The second half of the book deals with the work of the Utrecht laboratories and it is devoted in the main to a comprehensive investigation of the development of *Limnea stagnalis*, an animal with a mosaic egg, from the cyto-physical and chemical point of view. A thorough study of oogenesis and oviposition such as undertaken here is a side of causal embryology often neglected. Some experiments on classical urodele material (and the egg of the clawed toad *Xenopus laevis*) was also carried out at Utrecht, and special note may be given to the ingenious technique, devised by Nieuwkoop, for rearing amphibian embryos on collodion films.

If criticism should be applied to such a praiseworthy publication two—possibly unavoidable—faults, namely those of high price and the unchecked erratic use of the English idiom, are unfortunate.

PETER FORD.

The Louse: An Account of the Lice which Infest Man, their Medical Importance and Control. By PATRICK A. BUXTON, C.M.G., F.R.S. [Pp. viii + 164, with 47 figures.] (London: Edward Arnold & Co., 1947. 10s. 6d. net.)

If we consider what insects have most affected man and determined his progress in the world, it would probably be agreed that, in the tropics, he has been more hampered by the mosquito and the tsetse than by other insects, but, if we include the temperate zones, the flea and the louse have been man's worst enemies, and, of the two, the louse has changed history to a greater extent than has the flea. History records that, during Napoleon's retreat from Russia, his once magnificent army of 612,000 men was reduced to a rabble of some 112,000 refugees within a few months, these huge casualties being mainly due to epidemic typhus, a disease which can only pass from man to man through the agency of the louse. In the Napoleonic campaign of 1813, the association between the louse and disease was not recognised; 100 years later the importance of the louse as a vector of epidemic typhus and relapsing fever had been proved, but, unfortunately, the means adopted for its control were still so sadly inefficient that to be a front-line soldier in the 1914-18 war, and not to be lousy, was regarded, in those days, as unusual and suggestive of an unwarrantable exclusiveness. The consequence of this failure of control was that, although epidemic typhus did not appear in the British Army during the "Great War," another louse-borne disease, trench fever, was widespread and caused more temporary loss of man-power on the Western Front than did any other disease, while during the post-war period of 1917-23, Zinsser records that more than 3,000,000 persons, in Russia alone, died from louse-borne typhus. These warnings from history were not entirely disregarded, and, during the twenty years separating World War I from World War II, our knowledge of the bionomics of the louse was somewhat augmented, but the knowledge of its control still lagged far behind the knowledge of its habits. It was at this stage, immediately after the declaration of war in 1939, that Buxton, impressed with the need for protecting our fighting and civilian forces from louse-borne epidemics, published the first edition of his book on the louse, in which he had collected together all the worthwhile

knowledge at his disposal, and in which he emphasised the insufficiency of the existing knowledge concerning its control, and prophetically urged the need of discovering a type of disinfection which would de-louse the individual without the necessity of bathing. We have seen that each successive war compelled the scientists to a further study of the louse, and added a fresh store to our knowledge of its habits and, although to a much less extent, its control. The 1939-45 war proved no exception, and the summarising of the knowledge acquired during the eight years separating the publication of the first and second editions of Prof. Buxton's book has increased its pages by about one-third. This new edition contains much fresh information on the morphology and biology of the louse and, above all, on the new methods of control, which were so successfully established during the critical years of warfare, and which followed closely on the lines suggested by Buxton in the first edition of his book, so that he is now able to state "the efficiency of DDT in controlling body and head lice, even under extremely bad general conditions, is so great that perhaps we have now seen the last great typhus epidemic." This achievement in the successful control of the most dreaded of all epidemic diseases ranks second to none amongst the great advances of medicine recorded during the last ten years, and forms a fitting testimony to the value of research in medical entomology.

R. M. GORDON.

PHYSIOLOGY AND MEDICINE

Chemistry of Muscular Contraction. By A. SZENT-GYÖRGYI. [Pp. viii + 150, with 49 figures.] (New York: Academic Press, Inc., 1947. \$4.50.)

THIS very interesting book brings much new information since the last review written by the Hungarian physiologist in *Acta physiol. scandin.*, 1945. The text is mainly oriented around the properties of actomyosin, which is a combination of two muscle-proteins, myosin and actin, of which the muscle myofibrils seem to be mostly built. The main point is the very remarkable observation that actomyosin solutions are superprecipitated by ATP, KCl and MgCl₂, and that the threads of actomyosin, prepared by Weber's method, become thinner and shorter under the influence of those substances. This is so impressive a phenomenon that a romantic type of scientist, such as Szent-Györgyi, cannot resist speculation. Thus the book is a mixture of remarkable facts and interpretations which are sometimes difficult to disentangle.

But superprecipitation of actomyosin and muscular contraction may be quite different phenomena; for instance, the shortening of the threads by ATP is a function of temperature and is nil at 0° C. However, physiologists know that the magnitude of shortening of a muscle is not a function of temperature and that a muscle shortens even at 0° C. For these reasons, calculations based on Van't Hoff's law, in order to evaluate heat of reactions, cannot have any physiological meaning, and their chemical significance remains doubtful, the reversibility of the phenomenon not being proved. It is, therefore, not surprising that the heat of the elementary process of superprecipitation appears negative, though there is no endothermic phase in the muscle thermograms of Hill's School, even at the very beginning of the shortening phase.

This is only one of the numerous points it would be possible to criticise in the light of what was known about muscular physiology before Szent-Györgyi became excited about it. But literature is practically omitted from his book, so that a reader who is not familiar with muscular contraction may get the impression that scarcely anything was done in the field before 1940. In fact, this is really a book about the physical chemistry of muscle proteins, and the relations between those properties and muscular contraction are still very uncertain.

M. DUBUISSON.

Patterns of Mammalian Reproduction. By S. A. ASDELL, M.A., Ph.D. [Pp. xiv + 437, with 12 plates.] (Ithaca, N.Y.: Comstock Publishing Co., Inc.; London: Constable & Co., Ltd., 1946. 27s. 6d. net.)

THE physiology of reproduction owes much to those who have described the habits, breeding cycles and reproductive behaviour, not only of the common but also of the more unusual species. Their reports are, however, widely scattered through the literature. In his new book Prof. Asdell has brought together an extensive collection of data, which deal with about 200 mammalian species, and has summarised the existing knowledge about many others. It would clearly have been impossible to include everything that has been published, [and Prof. Asdell has deliberately and rightly restricted himself to the most reliable and the most quantitative information. Wherever the data warrant treatment, he has added statistical analysis of his own.

The book is introduced by a short orientating chapter which deals with the general problems of sex determination, sexual maturity, the oestrous cycle, pregnancy and the male reproductive system. The discussion provides an excellent, if not specialised, treatment of these various problems.

The form and function of the reproductive organs is then described for the different mammals in turn. When information is scarce or unreliable, the amount of space allotted to a species may be as little as a single entry in a table giving its breeding season or gestation period. On the other hand, some of the more common mammals used in the laboratory receive detailed treatment extending over many pages. Similarly, large sections are allotted to other animals of economic importance.

One of the functions of a book such as this is to make clear where there are gaps in our knowledge, and Prof. Asdell himself often suggests where new investigations are needed. It is noticeable throughout that there is much more information about the reproductive processes in the female than in the male. Perhaps this is one of the larger gaps that are revealed.

The book has already established itself in our laboratories as an essential reference-book for specific information about a wide variety of questions. It is not to be expected that its subject-matter will make easy bedtime reading or force one to go on reading until the end is reached. But this is not its purpose, and there is no doubt that all who are actively engaged in the study of reproductive physiology will need it for constant reference.

P. L. K.

Drugs from Plants. By TREVOR ILTYD WILLIAMS, B.A., B.Sc., D.Phil. [Pp. 119, with 12 plates.] (London: Sigma Books, Ltd., 1947. 6s. net.)

EVERYBODY is interested in drugs, and drugs derived from plants have perhaps more fascination to the layman than the synthetic products, with their uncouth names, of pharmaceutical industry. Dr. Williams has chosen his examples well—quinine, hashish, digitalis, penicillin, ergot, opium and cocaine—and he has earned the gratitude of all who are professionally interested in drugs by emphasising, in a book for the general reader, the appalling dangers of drug addiction. Most readers will wonder why nicotine was omitted, and it is curious that in his chapter on ergot the author has not told the dramatic story of the discovery of ergometrine in "watery extract of ergot"; indeed, no mention is made of this traditional weapon in the armoury of the midwife.

The art of presenting scientific knowledge to the general reader is a difficult one and the author has not always avoided one of its commonest pitfalls, the mixing of elementary and difficult ideas. One wonders whether the reader who needs to be told that quinine sulphate is a salt of sulphuric acid will appreciate the brief discussion of stereoisomerism in relation to the cinchona alkaloids; moreover, the molecules of quinine and quinidine are not related as object and mirror-image (p. 42). It seems a pity too that the author should poke fun at chemical terminology, the more so because both the "rather frightening name" of nicotine (p. 31) and the name in which procaine "rejoices" (p. 105) are inaccurately given. These are, however, minor blemishes in a book which many general readers will find of fascinating interest.

H. R. I.

Curare: its History, Nature and Clinical Use. By A. B. McINTYRE, Ph.D., M.D. [Pp. viii + 240, with frontispiece and 24 figures.] (Chicago: The University of Chicago Press; London: Cambridge University Press, 1947. 27s. 6d. net.)

THE author, who played a considerable part in the initiation of the recent great increase in the clinical use of curare and its alkaloids, presents in this book a very compact summary of the most important publications on curare and its alkaloids from the year 1916 to 1946, and includes a "stop-press" list of papers dating well into 1947. Some previously unpublished observations are also recorded from the author's own laboratory, where much valuable work has been done of recent years. Over 1300 references are discussed in the text, which results sometimes in over-compression, but the author expands the more important points to a degree which gives an adequate résumé of the position. It is a little surprising to find, however, no reference to the classical work of Crum Brown and Fraser on alkyl ammonium compounds, and Ing's valuable discussion on the mode of action of opium salts is referred to only very briefly. Two minor errors have been noticed. On p. 61 the formula for tubocurarine should have two and not one methyl group attached to each nitrogen atom. On p. 180 a reference to a paper by West is given as from *Proc. Roy. Soc. B.*, London, instead of *Proc. Roy. Soc. Med.* Particularly in the section on history and botany the original authors' words are quoted, which enlivens the text and gives a more accurate view of what

in fact they established. The section on history is particularly well done, and makes fascinating reading. The summary of the chemistry of the alkaloids is extremely useful. In the discussion of theories of curarisation, while dealing sympathetically with Lapicque's views, the author decides for the theory of the inhibition by curare alkaloids of the role of acetyl choline at its point of action in the muscle. A very full and up-to-date account is given of the general pharmacology and clinical uses of curare. No one who works in the field can fail to be grateful to the author for this well-written and scholarly work.

J. W. TREVAN.

Malaria, with Special Reference to the African Forms. By W. K. BLACKIE, M.D., Ph.D., F.R.C.P.(Ed.), D.T.M. & H. [Pp. viii + 104, with 1 coloured plate.] (Cape Town: The African Bookman, for the Post-Graduate Press, 1947. 10s. 6d. net.)

THE subject of malaria, a disease justly designated by the late Sir William Osler as the greatest single destroyer of the human race, is so extensive that it would require a massive text-book of many hundreds of pages to cover all its aspects. The book under review makes no attempt to do so, being limited to an historical review, followed by chapters on parasitology, pathology and morbid anatomy, clinical features, diagnosis, treatment and drug prophylaxis. There is no mention of epidemiology, methods of investigation in the field or of the prevention and control of the disease by antimosquito measures, for which indeed there is no place in a work of this dimension. The book is presumably intended as a concise handbook for the clinician practising in tropical Africa, and as such it fulfils its purpose admirably.

In the chapter on parasitology, the four species of plasmodium concerned in the production of human malaria are carefully and systematically described, though the explanatory plate, which has no doubt suffered in reproduction, leaves much to be desired, particularly in respect of the figures representing the gametocytes.

It is not always realised that a primary attack of malaria in a non-immune subject differs markedly from a relapse, or from a superinfection in one who has been previously infected with the same species of parasite, particularly in regard to the severity of the attack and the character of the pyrexia. It is most important that all medical men who are likely to encounter cases of malarial infection should be made aware of this fact. This is fully recognised in the book under review, so much so that separate accounts are given of the clinical features and treatment of malaria in the European and in the African native.

In the chapter on treatment an account is given of the value and limitations of the cinchona alkaloids and of the various synthetic antimalarial drugs which are now in use or under trial in the chemotherapy of malaria. It is rightly stressed that further research in this ever-widening field is urgently required.

The book is well written and clearly printed and should prove particularly valuable to clinicians embarking on a career in the malarious zones of the African continent.

G. C.

HISTORY OF SCIENCE

The Chemical Society 1841-1941. By T. S. MOORE and J. C. PHILIP.
[Pp. 236, with 10 plates.] (London: The Chemical Society, 1947.
12s. 6d. net.)

THIS is a volume of remarkable interest, beautifully produced having regard to the exigencies of the times, fittingly commemorating the Centenary belatedly celebrated in 1947.

Extracts from the records of the Society including its principal officers recall many of the most honoured names in chemistry. The major and early part of the book consists of a narrative account of the Society's activities over the century of its existence. Not only is the story told delightfully, but it is liberally embellished with brief, but always charming, biographical sketches of almost all the leading British chemists of that period. Probably no active chemist of today could read this book without feelings of real enjoyment and satisfaction, and of amazement at the short span which separates him from those chemists such as Hofmann, Roscoe, Perkin, Ramsay and many others whose names are indolibly inscribed in the history of science. Equally stimulating will every chemist find the recounting of the circumstances surrounding so many features of the Society's activities--its publications and procedure, even its rooms and their furnishings. He will certainly reflect with personal interest and pleasure on the strong links associating so many of these features with personalities which younger chemists at least already regard as almost legendary figures of the past.

In short, this is a volume to excite the imagination of all who have British chemistry at heart; without doubt, it will increase in interest and historical value as the years pass.

A. H. COOK.

A Century of British Chemistry. By F. SHERWOOD TAYLOR, M.A., B.Sc., Ph.D. [Pp. viii + 40, with 12 plates.] (London: Longmans, Green & Co., Ltd., for the British Council, 1947. 1s. 6d. net.)

THIS booklet, marking the Centenary of the Chemical Society, relates the outlines of the achievements of chemistry in Great Britain over the past century. The story is told superbly, in simple terms, making it clear and enjoyable for chemist and non-chemist alike, it is very fittingly illustrated, and it is free from serious blemishes of production. The remarkably weighty contribution of British chemists to their science is reflected in the author's contriving in a very limited compass to refer to the more outstanding work of some 200 individuals down to the present day, thus including many of the most famous names in their field. This account could hardly have been more timely even had the Centenary not suggested it; every chemist who reads it will do so with pleasure, and will find his view of the subject agreeably brightened thereby.

A. H. COOK.

English Naturalists from Neckham to Ray. By C. E. RAVEN, D.D. [Pp. x + 379.] (Cambridge: at the University Press, 1947. 30s. net.)

PROF. RAVEN'S account of the English naturalists from Neckham to Ray takes us through a period of nearly five centuries, from the dominance of the

medieval outlook to the inception of the modern approach to science. It is no mere treatment of their contributions to Natural History, but an account—needless to say, a scholarly one—of the men themselves and of their achievements against the background of contemporary environment. The author is so full of his subject that his style is at times involved to the point of obscurity, where interpolations render it occasionally a matter of speculation rather than certainty when he has reverted to his main theme. But the text is a mine of information and, even where the weight of erudition does not sit lightly, the author is never tedious. The work begins with a consideration of the medieval attitude towards nature, which was in the author's phraseology "to accept legend in preference to actual record so that the world of tradition and of actuality lived side by side." In Prof. Raven's view it is to the artists rather than to the scientists that we owe the beginning of modern zoological and botanical science, but it must not be forgotten that the attitude, which in later times became known as the doctrine of signatures, and which was early and sincerely held, did more than a little to foster that accurate observation which Dr. Raven fully recognises as the foundation of progress.

William Turner is dealt with more fully than any of the other naturalists, and indeed his years of training, the period of his exile, the years of maturity and his scientific work, to each of which a chapter is devoted, together occupy more than a quarter of the entire volume. The correctness of Dr. Raven's dictum that the same motives which made Turner a gossamer made him a herbalist is at least arguable, but doubtless it was common motives that determined his method of approach to both spheres of activity. But we can certainly agree with Dr. Raven's happy tribute that Turner "in his own day stood alone as a scientific botanist in England; it is largely due to him that no one has ever been in that position again." John Caius the author of *Rariorum animalum*, which is the subject of a brief chapter, shares with Turner the position of being a pioneer in the study of zoology in England. Thomas Penny's work is considered at length as being the only important contribution of Englishmen to zoological studies until the latter half of the seventeenth century, but of Mouffet's editing of the *Theatrum insectorum* Dr. Raven writes: "Penny's arrangement may have been defective, it could hardly be worse than Mouffet's," who was emphatically not a great naturalist or a first-hand student. Of Gerard it may be said that his popular reputation is largely due to the value attached to his Herbal by bibliophiles, just as the philatelist prizes the faulty engraved stamp because of its rarity. Dr. Raven's estimate of Gerard is worth quoting: "It is hard to acquit him of almost all the sins of which a man of letters or of science can be guilty, but where we have ceased to respect him as a botanist, or esteem him as a man of honour, we cannot fail to enjoy him." But Gerard, "not doubting but some of greater learning will perfect that which I have begun according to my small skill," had his prophetic vision largely fulfilled by the admirable edition of Thomas Johnson, to whom and to Parkinson our author pays well-merited tribute.

From Johnson the author passes to How, Merret and ends with Browne and the *Religio Medici*. Sufficient has been written to indicate the scope of a work that should appeal to all students of history, whether of the natural variety or the human artifact.

E. J. S.

MISCELLANEOUS

Science in Transition. By A. W. HASLETT. [Pp. iv + 244, with 5 figures and 4 plates.] (London: Christopher Johnson, Publishers, Ltd., 1947. 10s. 6d. net.)

THAT Mr. Haslett is a journalist (he is Editor of *Science Today*) is evident from the style in which parts of this book are written. *E.g.*, "Alternatively, and this is merely another way of saying the same thing . . ." (p. 2). "Probably there has never in all history been so much concentrated and co-ordinated effort put into anything before" (p. 7). "As one of the greatest of British scientists once remarked . . ." (p. 26). There are also signs of hasty writing: some easily verifiable facts are incorrectly stated, some names are wrongly spelt and some statements are misleadingly dogmatic.

The book deals with a number of very interesting and topical subjects covering many branches of science. The first chapter, "Science Looks Ahead," discusses possible reasons for the success of scientists in tackling problems during the war right outside their normal specialised fields, the necessity for freedom of publication, and the need for more scientists and for more science teaching in schools. The next four chapters are concerned with the development of atomic and nuclear research, leading to the atomic bomb and artificial radioactivity and indicating how the general outlook has changed during the last fifty years. The story has been told many times and the author has little new to add in material or approach. Chapters 6 and 7 give a straightforward account of the development of radar and its possible applications, an account which has benefited by Mr. Haslett's own wartime experience. The next two chapters discuss scientific aspects of food production; Chapter 10 is on "The Chemist and Disease," and then the subject changes completely to "Jet Propulsion" in Chapter 11. The last two chapters are more general, "The Way of Science" and "Using our Scientist," and some of Mr. Haslett's remarks are full of common sense. There is a helpful list of suggestions for further reading and a good index, and the book is attractively bound.

The chief merits of the book are the breadth of the scope and the inclusion of general questions which demand the consideration of all thinking people.

F. A. V.

English-Spanish Chemical and Medical Dictionary. By MORRIS GOLDBERG. [Pp. x + 692.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 50s. net.)

ALTHOUGH this is rather a Medical than a Chemical Dictionary, the book is on the whole a good one within the scope chosen by the author himself. Its aim is to be an "aid to translators, export managers, chemists, physicians, nurses and others interested in English and Spanish scientific literature."

The dictionary contains "more than 40,000 of the most important English terms pertaining to Medicine, Surgery, Pharmacy, Chemistry, Dentistry, Veterinary, Biochemistry, Bacteriology and related sciences."

It would have been better, perhaps, if instead of covering such a wide field the terms collected had been more representative of fewer subjects, leaving the remaining material for a companion volume.

From the point of view of a translator, the ideal technical dictionary is that which includes that unusual word or rare definition which is just outside the range of the ordinary dictionary or beyond his fairly large encyclopædic

knowledge. It should contain, in short, that element which helps the translator to solve the difficult problems of phraseology which he sometimes encounters in his translations.

Failing that, a dictionary of the specialised technical type should avoid, as far as possible, the inclusion of ordinary terms already registered by dictionaries of a more general character and concentrate, not exactly on oddities—although their usefulness should not be ignored—but on the incorporation of hard, knotty and out-of-the-ordinary terms.

Mr. M. Goldberg's Spanish-English Dictionary is a good book, which, although not supplying all the needs of the general translator, contains a sufficiently comprehensive vocabulary to be of great help to him.

This work is, as far as we know, the first English-Spanish dictionary of medicine available. The usefulness of giving Spanish equivalents and Spanish definitions of the English terms is real and affords the possibility of turning into good technical Spanish the scientific phraseology of medical and allied texts. Further, it brings in concentrated and handy form the materials contained in the large and comprehensive bibliography printed at the end of the book.

This dictionary is a very useful work of reference which we would recommend to all those for whom it was intended when its compilation was designed.

R. P.

Mother Earth : A popular account of the Earth in the Light of Recent Knowledge. By T. A. RYDER, B.Sc.(Hons.), Ph.D., F.G.S. [Pp. 184, with 25 figures.] (London: Hutchinson's Scientific and Technical Publications, 1947. 15s. net.)

Mother Earth has been written for ordinary persons with no special qualifications. It ranges over a wide field and is mainly concerned with geology and astronomy, but also has chapters on meteorology, oceanography, soil, life in general and man in particular, in so far as they affect "Mother Earth." In geology it touches lightly on the structure, the changing face and the age of the earth, and also on volcanoes and earthquakes. It is a bold attempt to give in popular form many interesting facts and some of the theories of geology. Geological theories are, as is well known, in a state of flux and the theories selected are only sketchily outlined. In the case of the Ice Age, the author says (p. 76): "Many theories have been put forward to explain the Ice Age but since none of them is entirely satisfactory it is unnecessary to give them in this book"; but in the case of the contraction theory and of Wegener's theory he gives some account, although geologists are by no means satisfied with them. To the folded mountains he applies both the latter theories, as (p. 55) he says: "Although mountain-building has here been explained along conventional lines, that is as being due to the contraction of the earth, it will be seen that some geologists invoke the action of other forces in mountain-building"; while later on (p. 126) he says: "The Andes and the Rocky Mountains . . . were probably formed as the result of the crumpling movement consequent on the westward drift of the Americas." Incidentally he is in error in saying (p. 65): "Wegener graphically described these movements (e.g. Antarctica to the south) as the *Poßflucht*, or flight to the poles." Wegener made no such statement, he regarded the *Poßflucht* as a drift of the floating continents from the poles to the equator (Wegener, p. 196).

G. F. S. H.

• BOOKS RECEIVED

(Publishers are requested to notify prices.)

- Methods of Algebraic Geometry.** By W. V. D. Hodge, M.A., F.R.S., Lown-
dean Professor of Astronomy and Geometry, and Fellow of Pembroke
College, Cambridge, and D. Pedoe, B.A., Ph.D., sometime Charles
Kingsley Bye-Fellow of Magdalene College, Cambridge. Vol. I, Book
I: Algebraic Preliminaries; Book II: Projective Space. Cambridge:
at the University Press, 1947. (Pp. viii + 440, with 46 figures.) 30s.
net.
- Theory of Functions.** By J. F. Ritt, Davies Professor of Mathematics,
Columbia University. Revised edition. New York: King's Crown
Press; London: Oxford University Press, 1947. (Pp. 181.) 16s.
net.
- A Text-Book of Mathematical Analysis. The Uniform Calculus and its
Applications.** By R. L. Goodstein. Oxford: at the Clarendon Press,
1948. (Pp. xii + 475.) 30s. net.
- Sequential Analysis.** By Abraham Wald, Professor of Mathematical Statis-
tics, Columbia University. New York: John Wiley & Sons, Inc.;
London: Chapman & Hall, Ltd., 1947. (Pp. xii + 212, with 16 figures
and 7 tables.) 24s. net.
- The Differential Analyser.** By J. Crank, M.Sc., Courtaulds Ltd. Research
Laboratory, Maidenhead. London, New York, Toronto: Longmans,
Green & Co., 1947. (Pp. viii + 137, with 4 plates and 31 figures.)
10s. 6d. net.
- Five-Figure Tables of Natural Trigonometrical Functions.** Prepared by H. M.
Nautical Almanac Office. London: H.M. Stationery Office, 1947.
(Pp. iv. + 123.) 15s. net.
- Selected Techniques of Statistical Analysis for Scientific and Industrial
Research and Production and Management Engineering.** By the
Statistical Research Group, Columbia University, Applied Mathematics
Panel, Office of Scientific Research and Development. Edited by
Churchill Eisenhart, National Bureau of Standards, Millard W. Hastay,
National Bureau of Economic Research, W. Allen Wallis, University of
Chicago. New York and London: McGraw-Hill Book Co., Inc., 1947.
(Pp. xiv + 473, with 45 figures and 48 tables.) 36s. net.
- Mathematical Theory of Rocket Flight.** By J. Barkley Rosser, Ph.D.,
Professor of Mathematics at Cornell University, Robert R. Newton,
Ph.D., Member of Technical Staff, Bell Telephone Laboratories, Inc.,
Murray Hill, N.J., and George L. Gross, Ph.D., Research Engineer in
Applied Mathematics, Grumman Aircraft Engineering Corporation.
New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. viii
+ 276, with 41 figures.) 27s. net.

- An Introduction to the Theory of Seismology.** By K. E. Bullen, M.A., B.Sc.(N.Z.), M.A.(Melb.), Sc.D.(Cantab.), Professor of Applied Mathematics in the University of Sydney. Cambridge: at the University Press, 1947. (Pp. xiv + 276, with 43 figures.) 18s. net.
- One, two, three . . . infinity. Facts and Speculations of Science.** By George Gamow, Professor of Theoretical Physics, George Washington University. London: Macmillan & Co., Ltd., 1947. (Pp. xii + 340, with 8 plates and 128 figures.) 24s. net.
- Introduction to Modern Physics.** By F. K. Richtmyer, late Professor of Physics at Cornell University, and E. H. Kennard, formerly Professor of Physics at Cornell University. Fourth edition. International Series in Pure and Applied Physics. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. xviii + 759, with 241 figures.) 36s. net.
- L'Évolution de la Notion de Phénomène physique des Primitifs à Bohr et Louis de Broglie.** Par Jean Pelseneer, Chargé de cours à l'Université libre de Bruxelles. Brussels: Office des Cours du Cercle des Sciences, 1947. (Pp. 177.) Frs. 110.-.
- Paramagnetic Relaxation.** By G. J. Gorter, Ph.D., Professor of Experimental Physics in the University of Leyden. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1947. (Pp. viii + 127, with 25 figures and 26 tables.) 12s. net.
- The Atom.** By Sir George Thomson, F.R.S., Hon. D.Sc. Lisbon, Professor of Physics at Imperial College. Third edition. The Home University Library of Modern Knowledge, No. 145. Oxford: at the University Press, 1947. (Pp. viii + 196, with 15 figures and 1 folding plate.) 5s. net.
- Introduction to Atomic Physics.** By Henry Semat, Ph.D., Associate Professor of Physics, The City College, College of the City of New York. New York: Rinehart & Co., Inc., 1946; London: Chapman & Hall, Ltd., 1947. (Pp. xii + 412, with frontispiece, 169 figures and 23 tables.) 24s. net.
- Electronic Circuits and Tubes.** By the Electronics Training Staff of the Cruft Laboratory, Harvard University. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. xxiv + 948, with 754 figures.) 45s. net.
- The Electron Microscope.** By V. E. Coslett, Ph.D., M.Sc., M.A., F.Inst.P. Sigma Introduction to Science 8. London: Sigma Books, Ltd., 1947. (Pp. viii + 128, with 12 plates and 40 figures.) 7s. 6d. net.
- Heat Pumps.** By Philip Sporn, President, E. R. Ambrose, Air Conditioning Engineer, American Gas and Electric Service Corporation, and Theodore Baumeister, Professor of Mechanical Engineering, Columbia University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. (Pp. viii + 188, with 111 figures and 16 tables.) 22s. 6d. net.
- Plasticity in Engineering.** By F. K. Th. van Iterson, former Professor of Applied Mechanics, Delft; former Director of the Dutch State Mines. London and Glasgow: Blackie & Son, Ltd., 1947. (Pp. x + 174, with 136 figures.) 8s. 6d. net.
- Radio Engineering. Vol. I.** By E. K. Sandeman, Ph.D., B.Sc., A.C.G.I., M.I.E.E., Engineering Division, The British Broadcasting Corporation. London: Chapman & Hall, Ltd., 1947. (Pp. xxiv + 775, with 280 figures, including 5 folding plates.) 45s. net.

- Scientific Instruments.** Described by Specialists under the Editorship of Herbert J. Cooper, B.Sc., A.R.C.S.I., A.M.I.E.E., Head of Engineering Department, South-West Essex Technical College and School of Art. London: Hutchinson's Scientific and Technical Publications, 1946. (Pp. 293, with 269 figures.) 25s. net.
- Handbook of the Scientific Instrument Manufacturers' Association of Great Britain, Ltd.,** 26 Russell Square, London, W.C.1., 1947. (Pp. 219, with 19 illustrations.) 10s. 6d. post free.
- Chemistry in the Service of Man.** By Alexander Findlay, Emeritus-Professor of Chemistry, University of Aberdeen. Seventh edition. London, New York, Toronto: Longmans, Green & Co., 1947. (Pp. xx + 390, with 74 figures.) 12s. 6d. net.
- The Alchemist in Life, Literature and Art.** By John Read, Ph.D.(Zürich), M.A. and Sc.D.(Cantab.), F.R.S., Professor of Chemistry in the University of St. Andrews. Edinburgh and London: Thomas Nelson & Sons, Ltd., 1947. (Pp. xii + 100, with frontispiece, 29 plates and 6 illustrations in the text.) 10s. 6d. net.
- Humour and Humanism in Chemistry.** By John Read, Ph.D.(Zürich), M.A. and Sc.D.(Camb.), F.R.S., Professor of Chemistry in the United College of St. Salvador and St. Leonard in the University of St. Andrews. London: G. Bell & Sons, Ltd., 1947. (Pp. xxiv + 388, with frontispiece and 90 illustrations, including 46 plates.) 21s. net.
- Fundamental Chemistry.** An Elementary Textbook for College Classes. By Horace G. Deming, Professor of Chemistry, University of Nebraska. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. (Pp. xvi + 745, with 188 figures.) 24s. net.
- Cyanogen Compounds.** Their Chemistry, Detection and Estimation. By Herbert E. Williams. Second edition. London: Edward Arnold & Co., 1948. (Pp. xvi + 443.) 40s. net.
- The Kinetics of Reactions in Solution.** By E. A. Moelwyn-Hughes, D.Sc., D.Phil., Ph.D., University Lecturer in Physical Chemistry, Cambridge. Second edition. Oxford: at the Clarendon Press, 1947. (Pp. viii + 424, with 52 figures and 144 tables.) 25s. net.
- Tables of Properties of Gases, with Dissociation Theory and its Applications.** By E. W. Geyer, B.Sc., Ph.D., Senior Lecturer in the Theory and Practice of Heat Engines, and E. A. Bruges, B.Sc., Assistant Lecturer in the Theory and Practice of Heat Engines, University of Glasgow. London, New York, Toronto: Longmans, Green & Co., 1948. (Pp. 102, with 19 figures.) 12s. 6d. net.
- Chimie organique. I: Chimie organique générale. II: Fonctions simples.** Par A. Kirmann, Professeur à la Faculté des Sciences de l'Université de Strasbourg. Collection Armand Colin Nos. 246 and 247. Paris: Librairie Armand Colin, 1947. (I: pp. 220, with 19 figures; II: pp. 195.) Frs. 100.- each.
- A Text-Book of Practical Organic Chemistry, including Qualitative Organic Analysis.** By Arthur I. Vogel, D.Sc.(Lond.), D.I.C., F.R.I.C., Head of Chemistry Department, Woolwich Polytechnic. London, New York, Toronto: Longmans, Green & Co., 1948. (Pp. xxiv. + 1012, with numerous figures and tables.) 42s. net.
- Reactions of Organic Compounds.** By Wilfred John Hickinbottom, D.Sc., Ph.D., F.R.I.C., Reader in Organic Chemistry in the University of London, at Queen Mary College. Second edition. London, New York, Toronto: Longmans, Green & Co., 1948. (Pp. x + 481, with 5 figures and 25 tables.) 25s. net.

- Elementary Analytical Chemistry. Qualitative and Quantitative.** By C. G. Lyons, M.A., Ph.D., F.R.I.C., Vice-Principal and Head of the Chemistry and Biology Department, Portsmouth Municipal College, and F. N. Appleyard, B.Sc., Ph.C., F.R.I.C., Head of the Pharmacy Department, Bradford Technical College. Fourteenth edition. London: J. & A. Churchill, Ltd., 1947. (Pp. viii + 279, with 15 figures.) 9s. net.
- New Developments in Ferromagnetic Materials.** By J. L. Snoek, Natuurkundig Laboratorium der N. V. Philips' Gloeilampenfabrieken, Eindhoven, Holland. Monographs on the Progress of Research in Holland during the War. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1947. (Pp. viii + 136, with 52 figures and 13 tables.) 13s. 6d. net.
- Boiler House and Power Station Chemistry.** By Wilfrid Francis, M.Sc. Tech., Ph.D., F.R.I.C., F.Inst.F., F.C.S., Consulting Chemist and Fuel Technologist. Second edition. London: Edward Arnold & Co., 1947. (Pp. xiv + 274, with 83 figures, including 8 plates, and 32 tables.) 21s. net.
- The Water-Soluble Gums.** By C. L. Mantell, Ph.D., Consulting Chemical Engineer, New York. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1947. (Pp. vi + 279, with 47 figures and 45 tables.) 36s. net.
- The Dehydration of Food.** By T. N. Morris, M.A., University of Cambridge and Department of Scientific and Industrial Research, Low Temperature Research Station. London: Chapman & Hall, Ltd., 1947. (Pp. viii + 172, with 30 figures and 19 tables.) 15s. net.
- Synthetic Resin Chemistry for Students.** By S. R. W. Martin, Ph.D., F.R.I.C., A.R.C.S., D.I.C. London: Chapman & Hall, Ltd., 1947. (Pp. viii + 160, with 13 figures, 5 diagrams and 11 tables.) 15s. net.
- Chemical Engineering Fundamentals.** By Chalmer G. Kirkbride, Manager of Research and Development, Houdry Process Corporation of Pennsylvania. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. xii + 419, with 73 figures and 18 tables.) 30s. net.
- Encyclopedia of Chemical Technology. Vol. I: A to Anthrimides.** Edited by Raymond E. Kirk, Head, Department of Chemistry, and Donald F. Othmer, Head, Department of Chemical Engineering, Polytechnic Institute of Brooklyn. New York: The Interscience Encyclopedia, Inc., 1947. (Pp. xxiv + 982, illustrated.) \$20.00.
- Safety Rules for use in Chemical Works. Part I: Model Rules.** Third edition. London: The Association of British Chemical Manufacturers, 1947. (Pp. 69.)
- The Permo-Triassic Formations.** By R. L. Sherlock, D.Sc., A.R.C.S., F.G.S. London: Hutchinson's Scientific and Technical Publications, 1948. (Pp. 367, with 13 figures.) 31s. 6d. net.
- The Formation of the Continents by Convection.** By G. F. S. Hills, B.A. London: Edward Arnold & Co., 1947. (Pp. vi + 102, with 5 figures.) 7s. 6d. net.
- L'Érosion du Sol.** Par Raymond Furon, Ancien Professeur de Géologie à la Faculté des Sciences de Téhéran, Sous-Directeur au Muséum National d'Histoire Naturelle. Paris: Payot, 1947. (Pp. 218, with 16 plates and 24 figures.) Frs. 360.-.
- Catalogue des Fossiles Tertiaires du Bassin de Paris.** Par Raymond Furon et Robert Soyer. Guides techniques du naturaliste, Vol. VI. Paris: Paul Lechevalier, 1947. (Pp. 240, with 32 plates.) Frs. 380.-.

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SCIENCE PROGRESS

THE SILICONES

By H. J. EMELEUS, M.A., D.Sc., F.R.S.

Professor of Inorganic Chemistry in the University of Cambridge

THE growing importance of organosilicon polymers lends special interest to the underlying chemistry of these substances and the physical properties on which their technological applications depend. The field is relatively old, the early work being associated particularly with the names of Friedel, Crafts, Ladenburg and Kipping. Kipping, indeed, made organosilicon chemistry his life work and the recent developments are in many instances based on the principles which emerged from his pioneer investigations. His chief interest lay, however, in exploring the points of similarity and difference between carbon compounds and those containing both carbon and silicon. The use by him of the newly discovered Grignard reaction in silicon chemistry opened the way for the preparation of many alkyl and aryl silicon halides, which are the starting materials now used in silicone manufacture. Kipping even studied the hydrolysis of these halides and isolated some of the simpler substances resulting from intermolecular elimination of water from the products. These polymers, to which the term silicone was applied, have unique properties. They combine in some measure the thermal stability of silicate structures with the water-repellent property, chemical inertness and electrical properties of paraffins. During the last ten years they have been intensively studied, especially in the industrial laboratories of the United States.* Important technical applications have already been made, and there is good reason to believe that yet other uses will be found as the field is further explored.

* Much of the work done on silicones is to be found only in the patent literature. An admirable survey is, however, given in Rochow's book, *An Introduction to the Chemistry of the Silicones* (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1946). This book gives a more detailed treatment of many points mentioned briefly in this article.

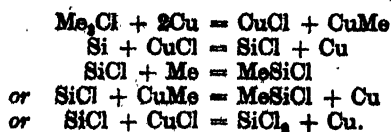
THE PRODUCTION OF INTERMEDIATES

The intermediates used in the preparation of the silicones are the alkyl and aryl silicon halides, or the alkyl- or aryl-alkoxy silanes of the type $R_nSi(OR')_{4-n}$. The chief raw material used is silicon tetrachloride, though ethyl orthosilicate, silicochloroform or hexachlorodisiloxane, Si_2OCl_4 , could also be used. The chlorine or ethoxy groups in these compounds are replaceable stepwise by alkyl or aryl radicals by the Grignard reaction. In its simplest form this entails reaction of an alkyl or aryl halide with magnesium turnings suspended in an aliphatic ether, when the alkyl or aryl magnesium halide is formed. This so-called Grignard reagent in ether solution is added to an ether solution of silicon tetrachloride or ester. The product depends on the proportions taken, but is normally a mixture with one component predominating. The following equations represent the reaction leading to a mono-substituted product :



This reaction has the great advantage of flexibility, being applicable not only to a variety of substituents, but also to the successive replacement of chlorine atoms by different substituents. The chief disadvantages are that a mixture of products invariably results and inflammable solvents are involved. The method has nevertheless been used on a large scale. Thus in preparing the methyl silicon chlorides, which are probably the most important intermediates in silicone manufacture, the Dow Corning Corporation has successfully operated a full-scale plant. The three products Me_3SiCl (b.p. 70°), Me_2SiCl_2 (b.p. 66°) and $MeSiCl_3$ (b.p. 57°) can be separated only with difficulty.

An alternative method of preparing either alkyl or aryl silicon halides is by the direct reaction of the organic halide with silicon. A second metal—usually copper or silver—is mixed with the silicon and acts as a catalyst. In the case of silicon-copper the powdered elements are mixed, the proportion of copper being about 10 per cent. by weight, and the mixture is pressed and sintered in an atmosphere of hydrogen at 1000° . The supposed course of the reaction with methyl chloride is as follows :

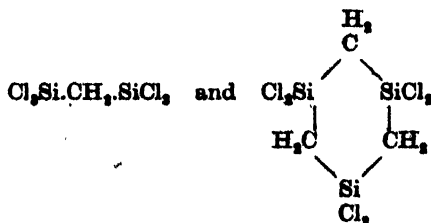


A cycle such as the above is believed to go on until the four valencies of silicon are satisfied by methyl groups and chlorine atoms.

The most effective use of the reactants occurs in the reaction, $2\text{MeCl} + \text{Si} = \text{Me}_2\text{SiCl}_2$. The reaction $3\text{MeCl} + \text{Si} = \text{MeSiCl}_2 + 2\text{Me}$ is undesirable because the free radical tends to decompose, forming C_2H_4 , CH_4 and C . In this reaction, which is favoured by higher temperatures, the carbon is deposited on the silicon-copper and is believed to catalyse the breakdown of methyl groups. The normal operating temperature in the production of dimethylsilicon dichloride is about 300° .

The mechanism of this reaction is not entirely a matter of speculation. Thus it has been shown that free methyl radicals do not react directly with silicon. If, on the other hand, methyl chloride vapour is passed at a high streaming rate and a low pressure over copper at 250° and then over a lead mirror, a copper mirror is formed and the lead mirror is removed. This is direct evidence that an unstable copper methyl can be formed, as postulated in the reaction mechanism. It is possible to pass methyl chloride vapour over films of copper and silicon deposited side by side on a glass slide. If the vapour passes over the silicon before the copper, the former is unattacked, whereas if it first passes over the copper methyl silicon chlorides are formed.

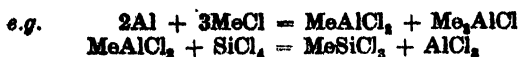
The reaction with silicon-copper takes place with other alkyl halides and also with halides containing more than one chlorine atom, the optimum conditions varying from case to case. Methylene chloride, for example, yields the two products



It has been claimed that in reactions of the above type improved yields are obtained by using a sintered mass of copper oxide and silicon. Silver is recommended as a catalyst for the formation of aryl silicon halides. In preparing phenyl silicon chlorides, for example, 10 per cent. of silver is used, and the optimum reaction temperature is $375\text{--}425^\circ$. Metals such as nickel, tin, antimony, manganese and titanium are also stated to catalyse these reactions, though there is no indication that the mechanism is the same as in the case of copper.

An alternative method which has been described recently involves passing an alkyl halide vapour mixed with silicon tetrachloride over

finally divided zinc or aluminium at 300–500°. The mechanism of this reaction may entail the intermediate formation of an alkyl aluminium halide.

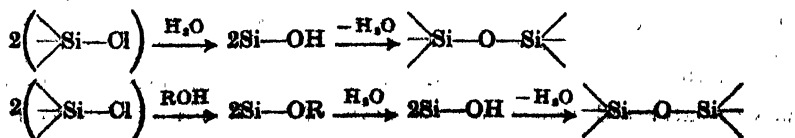


This type of reaction is fairly general, though the yields vary considerably from case to case, due, no doubt, to differences in the stability of the metal alkyl compounds. Whereas it is known that the Grignard method and the catalytic method have been used on a considerable scale, there is as yet no indication that the last method has been so used.

A final method of producing intermediates, which has the great merit of simplicity, is to react hydrocarbons directly with silicon tetrachloride. Acetylene, ethylene and carbon monoxide are all absorbed by silicon tetrachloride in presence of aluminium oxychloride, which acts as a catalyst. The products are $\text{CHCl}:\text{CHSiCl}_3$, $\text{CH}_2\text{ClCH}_2\text{SiCl}_3$, and COCISiCl_3 , respectively. Though none of these products is at present useful for silicone manufacture on account of the reactive halogen atom in the organic radical, it may well be that so simple a process will in time be developed to give a cheap source of suitable substituted silicon halides.

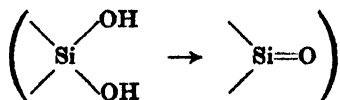
THE PRODUCTION OF SILICONES

The first stage in the production of silicones is the hydrolysis of the alkyl or aryl silicon halide to the corresponding hydroxy derivative. The patent literature on this subject is confused. Silicon tetrachloride itself hydrolyses very rapidly with liquid water, forming a gelatinous mass. For the substituted silicon halides, the rate of hydrolysis is slower, but the conditions have to be most carefully controlled if gel formation, which is generally undesirable, is to be avoided. The method used in hydrolysing a particular product should be capable of giving a mobile liquid rather than a gel. Methods described include pouring an ether solution of the compound to be hydrolysed on to ice, and the use of solid inorganic hydrated salts or of salt solutions as the hydrolysis medium. An alternative is to dissolve the silicon halide in an inert non-alcoholic solvent (*e.g.* in benzene or its homologues) and to add the solution to a mixture of water with a partially mixible aliphatic alcohol (*e.g.* *n*-butanol). The reactions under such conditions are believed to be

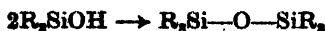


The rates of hydrolysis and alcoholysis of the silicon halide are faster than the rate of hydrolysis of the alkoxy silane. The slower rate of hydrolysis of the alkoxy derivative thus retards the overall rate of formation of the hydroxy derivative. Reference is made below to the actual method used in preparing specific products.

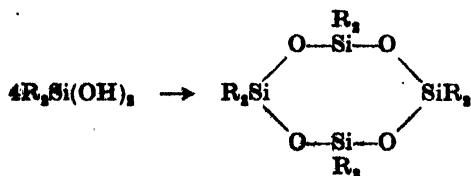
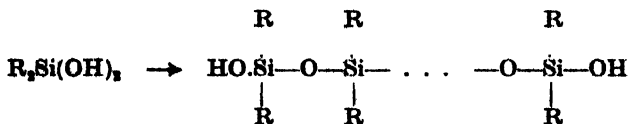
The hydroxy derivatives, or silicols, formed in the initial hydrolysis are mobile liquids which, particularly in the aliphatic series, lose water very readily, forming condensation products known as silicones. This term was originally used by Kipping, who thought that the loss of water was an intramolecular process, analogous to ketone formation



Later, however, he proved that the water was eliminated by an intermolecular condensation. The underlying principles of silicone formation are thus fairly simple. Considering in turn the three substituted silicon chlorides $R_3\text{SiCl}$, $R_2\text{SiCl}_2$, and RSiCl_3 , and their initial hydrolysis products $R_3\text{SiOH}$, $R_2\text{Si}(\text{OH})_2$, and $\text{RSi}(\text{OH})_3$, intermolecular elimination of water from $R_3\text{SiOH}$ can give only one derivative :



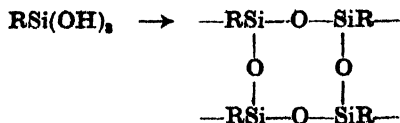
Starting with pure $R_2\text{SiCl}_2$, on the other hand, its hydroxy derivative can condense to form either a linear polymer or a ring :



These two types of condensation both occur: the length of the chain varies according to the reactant and conditions, and the size of the ring is also variable. It should be noted that the size of the ring can be greater than those which are most common in organic chemistry because the Si—O—Si valency angle is greater.

The third type of hydroxy derivative, $\text{RSi}(\text{OH})_3$, can by a similar mechanism form a two-dimensional network, and is thus

one stage short of the three-dimensional network of silica itself, which may be thought of as formed from Si(OH)_4 :



These principles may be illustrated by the condensation of the silicols from the methyl silicon chlorides. The simplest, Me_2SiCl , yields on hydrolysis and elimination of water the compound $\text{Me}_2\text{Si—O—SiMe}_2$, a stable liquid which boils at 100° . Dimethylsilicon dichloride when hydrolysed by a large excess of water forms a colourless oil which contains a variety of molecular species. The simple product $\text{Me}_2\text{Si(OH)}_2$ cannot be isolated because condensation at room temperature is too rapid. Both linear and cyclic polymers are formed, the latter being found on distillation in the more volatile fractions. Molecular species with up to nine Me_2SiO units in the ring have been isolated. The short chain polymers are not formed and the less volatile part of the hydrolysis product contains a mixture of long chain molecules. If, however, too little water is used for complete hydrolysis, shorter chains with terminal Cl groups may be isolated (e.g. $\text{Cl[Me}_2\text{SiO}]_{1-5}\text{SiMe}_2\text{Cl}$). The proportion of volatile ring polymers is increased if the hydrolysis is carried out by stirring with water and a mutual solvent, or if strong acids are present, and is decreased by the presence of ammonium hydroxide. In the hydrolysis with a large excess of water about half of the product is made up of ring compounds. Pure methyl silicon trichloride gives on hydrolysis a non-volatile product which hardens to a transparent resin on moderate heating as the process of cross linking, with elimination of water, is completed.

The aromatic silicols differ markedly from the aliphatic, particularly in the ease with which intermolecular condensation takes place. Thus the diol from diphenylsilicon dichloride, $\text{Ph}_2\text{Si(OH)}_2$, is a crystalline solid which is readily isolated. When dissolved in ether and warmed with a little hydrochloric acid it condenses to a cyclic trimer, $(\text{Ph}_2\text{SiO})_3$, whilst a similar treatment with alcoholic alkali gives the tetramer. Linear polymers are also formed together with the cyclic products by simple heating, but the process is slower. Hydrolysis of phenyl silicon trichloride yields a resin which hardens on heating. A large number of other aryl silicones have been described, as well as a range in which both alkyl and aryl groups are present in the molecule. The latter are formed either by hydrolysing an alkyl-aryl chlorosilane or by co-condensation of a mixture

of aryl and alkyl chlorosilanes. Valuable modifications in physical properties have been obtained in these various mixed silicones.

It is not in general possible to prepare a silicone in which a single type of condensation has occurred. Indeed most silicones used in practice owe their special properties to a combination of the different types. The silicool derived from R_3SiCl is particularly useful in this respect because it affords a means of stopping the growth of a linear polymer. Similarly the type $RSiCl_2$ is a means of introducing cross linking. Another important principle is the production of cross linking by oxidation, either by heating the silicone in air or by adding to the mix an organic peroxide such as benzoyl peroxide. For each organic group so oxidised one $Si-O$ bond is formed, and this method may be used not only to join chain-like molecules to one another, but also to join ring polymers to chains. The whole picture of the detailed structure of silicone polymers is, indeed, still very obscure, though the principles are well established.

Yet another type of polymer about which little has so far been published, but which may have great importance, is that in which the organic group itself is one which is able to form a polymer. If, for example, a silicool is prepared from diallyl silicon dichloride, the (R_2SiO) chain and cyclic polymers will first form, in addition to which polymerisation involving the allyl groups takes place. There are many possibilities of this sort, and in general it would be expected that the polymerisation process would occur more readily, but that the product would lack in some measure the thermal stability of the true silicone polymer.

APPLICATIONS OF SILICONES

SILICONE OILS

The methyl silicone oils are a group of materials in which are combined the properties of small change in viscosity with temperature, low volatility, high thermal stability and low freezing point. They are stated to be useful as damping fluids and hydraulic fluids, in liquid-filled condensers for high frequency, as lubricants for plastic mouldings, or as impregnants for gaskets where resistance to heat, water and chemicals is needed, and for waterproofing ceramics. Various materials of this type are available commercially, but the general properties may be illustrated by the methyl silicone oils. These are based on a combination of the properties of the hydrolysis products from Me_3SiCl and Me_2SiCl_2 . The first serves as a

source of chain-like polymers and the second provides terminal groups for the chains.

One method of obtaining this result would be by cohydrolysis of a mixture of Me_2SiCl_2 and Me_3SiCl in the proper proportions, but this would also produce cyclic polymers. A better method is to take hydrolysed Me_2SiCl_2 , containing both linear and cyclic polymers, and to mix with it the required proportion of $\text{Me}_3\text{Si.O.SiMe}_3$, the stable hydrolysis product from Me_3SiCl . This mixture is agitated at room temperature with 4 per cent. by volume of concentrated sulphuric acid. The acid apparently reacts with the Si—O—Si bonds forming unstable esters of the type Si—HSO_4 . These are hydrolysed and can reform polymer molecules. Whereas, however, the original polymer from $\text{Me}_2\text{Si(OH)}_2$ contained chains and rings there are present, as a result of the reaction with sulphuric acid, Me_3SiOH molecules which act as end groups to chains. The result is that the cyclic polymers originally present are broken down into linear polymers with terminal Me_3Si groups. The mixture is finally diluted with water to hydrolyse residual sulphate ester groups, neutralised, washed and dried. Any more volatile material present may be distilled off and the product consists mainly of linear polymer molecules with a relatively narrow range of chain lengths. If the Me_2SiCl_2 used contains a controlled amount of MeSiCl_3 , it is possible to obtain a certain amount of cross linking in the final product and so to modify its properties.

The small change in viscosity with temperature is illustrated by the following figures: A typical silicone oil and a hydrocarbon oil having the same viscosity at 35° are found at -40° to have increased in viscosity by factors of roughly 2-3 and 2000, respectively. The freezing points range from -50° to -80° or so, depending on the exact composition. The oils are inert to metals and do not attack or swell rubber. Their dielectric losses are very low, a typical power factor value being 0.0001 at 12 megacycles. This varies little with frequency and temperature. Another interesting property of these substances is that they reduce the tendency to foaming in certain engine oils, the concentration required to produce this effect being extremely low.

SILICONE RESINS

The essential difference between silicone resins and silicone oils is that in the former there is a much greater degree of cross linking. In the idealised linear polymer with the structural unit R_2SiO the ratio $\text{R} : \text{Si}$ is 2, but as cross linking is introduced this value drops, until in the completely cross linked structure based on RSiCl_2 , it is

unity. This ratio, which can be determined analytically, is thus a convenient measure of the extent of cross linking in a silicone. The actual properties also depend on the nature of the group R and the method of preparation, but, taking the methyl silicones as an example, the polymers with less than 1.2 methyl groups per silicon atom are syrups which change at room temperature or with mild heating to brittle glassy solids. When the ratio exceeds 1.7 the silicones are oily and change to soft gels only after prolonged heating at 200°.

The methods of obtaining cross linked products are: (1) Co-condensation of the hydrolysis products from mixtures of halides of the type R_nSiCl_{4-n} and $RSiCl_3$; (2) air oxidation of the silicone derived from a halide which is largely of the type R_nSiCl_{4-n} . This oxidation may also be brought about or facilitated by the addition of peroxides to the liquid silicone. As already pointed out, oxidation actually removes organic groups and replaces them by an Si—O— bond, a process which may be followed by analysis of the gaseous products.

The properties of silicone resins vary greatly according to the organic groups present. All require some measure of heating before they dry to resinous films. Such films have a high thermal stability and resistance to oxidation, and their chief use is for electrical insulation. Varnishes and resins are used to impregnate materials such as fiberglass, mica or asbestos. Insulating materials of this type may be operated continuously at 175° and show a high dielectric strength. Silicone varnishes are indeed available for a wide variety of uses, the only limitation being that they must be heated to 200–250° before they become solid and stable. The following data, quoted by Rochow (*op. cit.*), illustrate the excellent electrical characteristics in the case of a methyl silicone resin: dielectric constant: 3.7 at 26°, 3.6 at 56°; power factor, measured at 60 cycles per sec.; 0.008 at 26°, 0.0045 at 56°. In general, the rate of hardening of a resin film decreases as larger and larger aliphatic groups are substituted for methyl groups in the simplest silicone, whilst the ease of oxidation increases. This is yet another indication of the possibilities that exist for modifying the properties of these materials to obtain any desired characteristics.

SILICONE RUBBER

The production of rubber-like silicone polymers having the good thermal and electrical properties which characterise the silicones as a group is one of the major achievements so far. The starting material for a methyl silicone rubber is substantially pure dimethyl-

silicon dichloride, which is hydrolysed and then treated in such a way as to open the silicon to oxygen bonds and allow the growth of long chain molecules without cross linking. This is done by heating the original hydrolysate with hydrochloric acid and a metallic chloride (e.g. FeCl_3). The product is milled with inorganic fillers (e.g. TiO_2) and a curing catalyst, such as benzoyl peroxide, moulded and heated to about 150° . The last-named operation, known as curing, probably entails oxidation, with the establishment of a certain amount of cross linking.

The silicone rubbers are in no sense substitutes for natural or other synthetic rubbers, chiefly because their mechanical properties are inferior. The tensile strength, which increases with the time of heating and with the amount of benzoyl peroxide up to about 5 per cent., is inferior and the elongation is poor. The rubbery property is, however, maintained at temperatures as low as -60° , and the electrical resistance and thermal stability are much the same as for other silicone products. The chief uses so far suggested are for electrical insulation, shock absorbers and gasket materials.

OTHER APPLICATIONS OF SILICONES

Silicone greases are at present available which, in their physical properties, are intermediate between the oils and the resins. Thus a plug cook grease is manufactured which retains a vaseline-like consistency from -35° to 250° and which is inert to steam and a wide range of chemical substances. A laboratory stopcock grease with similar properties is also available. A somewhat similar material is the so-called ignition sealing compound, used to prevent access of moisture to disconnectable junctions, particularly the ignition harness of aircraft engines. A grease for this purpose may be made by hydrolysing a mixture of SiCl_4 , Me_2SiCl_2 and MeSiCl_3 . Its consistency changes little between -40° and 200° .

One further use of methyl silicon halides, apart from the actual manufacture of polymeric materials, is in the treatment of surfaces to render them water repellent. The effect produced may be demonstrated by exposing a filter paper, which has not been specially dried, to the vapour from a mixture of methyl silicon halides. The surface moisture in the paper reacts with the organosilicon compound and forms a film of silicone on the paper which, as a result, is no longer wetted by water. Indeed a paper so treated may be used to filter off a light oil from admixed water, which remains in the filter paper. Not only paper, but wood, cotton and other materials may be similarly treated and it is quite possible that superficial silicone films of this sort may be used in waterproofing

various types of surface, particularly as no heating is needed to produce a permanent film. The method has already been used on a large scale for treating glass and ceramic surfaces. Steatite insulators in aircraft radio equipment, for example, when so treated, do not become covered with a water film when, under operating conditions, condensation of water would otherwise occur. The surface resistivity remains extremely high. In treating a surface the hydrochloric acid formed will usually escape: only in the case of certain fabrics is it sometimes advisable to neutralise this acid.

THE NATURAL LIMIT OF ACCURACY OF OBSERVATION

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1.—Towards the end of the last century, when the concept of what we now call “classical physics” had been finally established, it seemed reasonable that further progress in the knowledge of the laws of physics could only consist in the quantitative refinement of already qualitatively known relationships. Hence the foremost task of the experimental physicist of that period was to increase the accuracy of his observational methods by devising new and more accurate and sensitive instruments, and by carefully excluding all unwanted external factors, and to apply these methods to some new precision measurements. Indeed, a physicist who had succeeded in measuring a physical “constant” with a hitherto unknown degree of accuracy could be sure of making his way in academic life.

Nobody doubted at that time that the process of increasing the accuracy of observation could, at least in principle, be continued indefinitely, and thus the values and functional relationships of physical quantities be determined experimentally to any desired degree of precision. It is, of course, evident that the process of measuring necessarily requires a mutual interaction between the object to be measured and the measuring device, which must, to a certain degree, affect the former. However, it was assumed as a matter of fact that this disturbance was of a completely regular character and hence could be accounted for by introducing a corresponding “correction factor.” Also it was supposed that by increasing the sensitivity of the measuring instrument one could reduce indefinitely its reaction upon the object of measurement.

On the other hand, the kinetic theory of matter was already well established, and within this theory certain physical quantities like density, pressure, electric current, etc., appeared to be statistically defined, that is as averages over a very large number of molecular events. It follows from this that the values of such quantities

must vary irregularly in space and time, which will prevent measurements being taken beyond a certain limit of precision. However, in the first place no real proof of the reality of the molecular structure of matter had been given at that time, and further, as a result of simple probability considerations, it appeared that within "macroscopic dimensions" these fluctuations would be negligibly small.

Since then the situation has radically changed, partly because of new developments in experimental technique and partly as a result of new insight into the very nature of the laws of physics.

It was A. Einstein [1] who, in a series of classical papers round about 1905, first showed that the above-mentioned statistical fluctuations, due to the irregular thermal movement of the molecules, should under certain circumstances be observable by methods already available. In particular, it was shown by him, and independently by M. v. Smoluchowski [2], that the Brownian motion of small particles, which is observable by means of ordinary microscopes, is a manifestation of the thermal molecular movement, in fact the outcome of the irregular pressure fluctuations in the surrounding medium. Soon afterwards the development of the ultra-microscope made it possible to observe the density fluctuations in colloidal solutions, and a very thorough theory of this and other fluctuation phenomena was laid down by Smoluchowski in a series of papers [3]. In his first investigation on the Brownian motion Einstein [1] had already predicted the existence of spontaneous electric currents of irregular character in closed electric circuits, owing to the thermal movement of the electrically charged carriers in conductors of electricity. This effect became observable when physicists had learned to amplify weak currents with the help of electronic valve amplifiers.

The discovery of these and other fluctuation phenomena made it clear that there was no point in trying to increase the accuracy of measurement of such quantities as are defined as statistical averages beyond the inherent statistical error. But, even if one tried to do so, one would be faced with another difficulty arising from the fact that the internal thermal movement of the constituent particles of the measuring instrument must of necessity produce an irregular fluctuation of the readings, which increases with increasing sensitivity. It was indeed predicted by Einstein [1] and Smoluchowski [2] that any microscopic system under the action of elastic restoring forces will perform an irregular Brownian motion about the equilibrium position with kinetic energy kT per degree of freedom. This applies to almost every type of mechanical measuring device and should become evident in instruments of

extreme sensitivity and small inertia. Since then the phenomenon has indeed been detected and made the object of thorough experimental and theoretical investigations [4]. Fig. 1 shows a record of the fluctuations in the deflection of a supersensitive torsion balance according to E. Kappler [5].

But this limitation does not apply to mechanical measuring devices only. Purely electrical devices, like electronic valves, cathode ray tubes, etc., are also subject to irregular intrinsic fluctuations owing to two circumstances. First of all, the above-mentioned thermal fluctuations of electric current within the electric circuits of the recording instruments must in turn produce an irregular fluctuation of the recorded deflections. Moreover, it was pointed

30 sec.



FIG. 1.—Record of the deflections of a supersensitive torsion balance. (After E. KAPPLER.)

out by M. Schottky [6] in 1918 that the transfer of electricity through electronic valves must also be subject to fluctuations due to the "grainy" structure of the electric current; this effect is called the "shot-effect." * If devices of this type are used for the amplification of weak signals in electrical telecommunication, these fluctuations become apparent as a background "noise," which makes it more and more difficult to discern the signals when the "signal to noise ratio" becomes smaller than unity, and which will eventually completely drown the signals. Fig. 2, Plate I, is a cathode ray oscillograph record of the electrical fluctuations in the network of a wireless receiver, obtained by D. K. C. MacDonald [8]. Owing to the low damping of the oscillating circuits of the receiver the fluctuations have the character of harmonic oscillations with irregu-

* The question of whether these two effects are essentially different phenomena has recently been discussed by the author [7]; he believes to have shown that they are in fact only two aspects of one and the same statistical phenomenon.

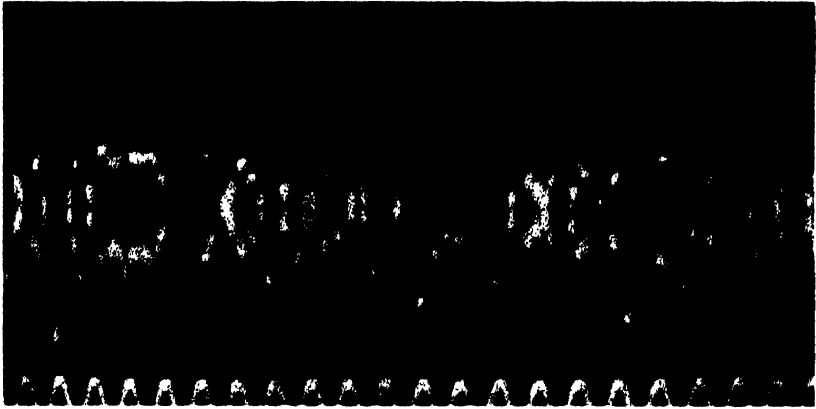


FIG. 2. Cathode ray oscillograph record of the electrical fluctuations in the network of a wireless receiver, after D. K. C. MacDonald. (Timing wave at the bottom corresponds to 1 kc/sec.)



FIG. 3.-- Cathode ray oscillograph record of the amplified current through an ionization chamber due to the penetrating cosmic radiation; Birkbeck College research team on cosmic radiation. (Total time appr. 1 hour.)

larly varying amplitude. The record shows only the "envelope" of the real fluctuation curve.

In view of these new facts, the older ideas about the reaction of the measuring instrument on the measured object have also to be revised. In consequence of the Brownian motion of the measuring device, this reaction will have an irregular character and can therefore not be completely accounted for. This can again best be demonstrated by a special example, namely the measurement of a weak electric current by means of a very sensitive coil galvanometer. Not only is the current itself subject to irregular fluctuations by its very nature, but also the mechanical Brownian motion of the mobile system will produce fluctuations of the light beam used as pointer; and finally the motion of the coil within the magnetic field will in turn generate an irregularly varying current, which will be superposed over the current to be measured. Similarly, by measuring weak radiation intensities by means of a highly sensitive radiometer, one has to reckon not only with the intrinsic fluctuations of the radiation itself, but also with the Brownian motion of the radiometer vane and with the irregular changes in the radiation field produced by this movement.

Parallel to this development runs another which was started by E. Schweidler [9], also in 1905, when he for the first time explained the law of radioactive decay as the outcome of the fact that for each atom of a radioactive element there existed a fixed disintegration probability. He argued that consequently the intensity of a radioactive radiation ought to fluctuate irregularly in time. This was indeed experimentally proved soon afterwards, and, with the development of the experimental technique for detecting and counting single radioactive particles, it has become an everyday experience to nuclear physicists. Fig. 3, Plate I, is an example of an oscillographic record of such a phenomenon, showing the fluctuations in the amplified current through an ionisation chamber caused by the penetrating cosmic radiation (obtained by members of the research team on cosmic radiation at Birkbeck College).

Thus, radiation intensity and other radioactive quantities were shown to be of a statistical nature and consequently incapable of being measured with indefinite precision. The nature of this chance character of nuclear phenomena, which has evidently nothing to do with the thermal fluctuations discussed above, was also clarified later on the basis of quantum mechanics. But it had been pointed out by Einstein [10] in 1912 that the quantum character of light radiation must be reflected in the statistical features of the radiation fluctuations. Later on the same author [11] dis-

discovered that the emission and absorption of light by atoms was governed by laws of the same type as those governing radioactive disintegration, and he discussed in detail the mutual interaction of radiation and Brownian movement of material systems in the radiation field. It thus became clear that the quantum structure of radiation must impose another limitation on the measurement of radiation quantities.

But it was not until 1926 that M. Born [12] put forward the statistical interpretation of the fundamental equations of quantum mechanics which is now generally accepted. According to this interpretation, the laws of quantum physics do not predict the fate of an individual system, but only that of an assembly of identical systems; in other words, they give only probabilities for the behaviour of an individual system. This implies that, not only certain classes of physical quantities, but all of them, with the exception of the universal constants, are of a statistical nature and must, therefore, exhibit fluctuations which will prevent precise measurements being taken beyond a certain limit. In fact, the observed object and the measuring device cannot be considered separately but, from the point of view of quantum theory, they form a single system, which in itself determines the statistical character of the measuring process, so that the distinction between the three different sources of observational error can no longer be maintained. Roughly speaking, this is the outcome of the fact that interaction between different fundamental particles always occurs in quanta of finite size, and thus, as we shall see later, the ultimate limit of accuracy of observation is determined by the value of the characteristic universal constant of the quantum theory, Planck's constant h . This was first realised in 1928 by W. Heisenberg [13] in his famous uncertainty principle.

2.—So far our contention that the irregular fluctuations of the readings of measuring instruments present a definite limit to the accuracy of observation has been based on instinct rather than on strict reasoning. It is the main aim of this paper to analyse the problem from different points of view and to put the results in a precise mathematical form.

Although it is undoubtedly true that a measurement becomes worthless when the mean observational error is of the same order of magnitude as the observed quantity, it is very well known that the accuracy can be improved by a repetition of the measurement. For it follows from the general theory of probability that the mean error of the average of n independent observations of the same quantity is $\sqrt{n}$ times smaller than the mean error of a single observa-

tion. Thus, by increasing n indefinitely the probable error can, at least in principle, be reduced below any standard. Indeed, definite results may even be obtained from measurements which seem hopelessly erratic if only the number of observations is sufficiently large ; and without this procedure the work of the statistician would be entirely impossible.

The same result can be achieved by using a recording instrument which measures the observed quantity continuously over a certain period of time, and by averaging or "smoothing" the graph either geometrically or with the help of some mechanical device (planimeter or integrator). Alternatively, one can use a measuring instrument designed in such a way that it will automatically "integrate" over a certain time interval and so directly give the required time average. Examples of such procedures are the measurement of the mean intensity of a weak light source by long photographic exposure and the measurement of a weak electric current by means of an electrometer of large capacity.

There is, however, a very serious objection to this scheme for reducing the observational error, for it presupposes that the observer already knows that the quantity to be measured is constant in time. But how is one to know this, unless by taking repeated measurements and comparing them, so as to find out whether the value is maintained or not ? And, as the readings or records will inevitably, to some extent, vary in time because of the fluctuations, one will not be able to decide whether the quantity has really remained strictly constant or has varied in time within the limits of the recorded fluctuations. Even in such cases where there is strong reason to believe that the quantity in question is a constant in the strict sense of the word, *e.g.* the velocity of light in empty space, it can never be proved experimentally, but only be made more or less plausible.

Actually, the principal task of the experimenter is to measure a certain physical quantity, say x , as a function of time, that is to determine the function $x(t)$ with the greatest possible accuracy. If one restricts oneself to a short time interval, this becomes equivalent to the problem of simultaneously determining the quantity x and its time derivative $\dot{x}$ as precisely as possible. The question to be answered is then : is there a natural limit to this precision imposed by the thermal fluctuations and by the statistical nature of the physical laws ?

Any measuring device can be characterised by its "relaxation time," that is the time which has to elapse between two successive observations so as to make them independent of each other or, in

other words, the time interval over which the instrument automatically "integrates." This relaxation time is about 1/20 second in the case of the human eye or ear as instruments of observation, of the order of minutes in the case of an ordinary mercury thermometer, and of the order of millionths of a second for some electronic relays and counters. Evidently, by using an instrument with a very large relaxation time, the accuracy of the measurement of the quantity x will be very great because of the above-mentioned "integrating effect," but no information will be obtainable about the values of $\dot{x}$ during this time interval. For example, by measuring a weak current with an electrometer of large capacity, it is not possible to state whether the current is strictly constant or has some sort of "ripple." On the other hand, by using an instrument with a very short relaxation time, one can repeat measurements at short intervals and so approximately measure the time derivatives $\dot{x}$ but, at the same time, the fluctuations will make themselves very strongly felt. Thus, the gain in resolving power by the use of an electronic amplifier plus cathode ray oscillograph for the measurement of a weak current may be lost because the irregular fluctuations which are exhibited may completely mask any periodic ripple.

3.—Let us now first consider the effect of the mechanical Brownian motion of the measuring instrument on the accuracy of the simultaneous observation of its deflection x and its time derivative $\dot{x}$, assuming that it is essentially a critically damped system bound to the equilibrium position $x = 0$ by an elastic restoring force. The problem of the Brownian motion of such a system has been extensively dealt with by Smoluchowski [2], L. S. Ornstein [14] and others. Calling x_0 the value of x at $t = 0$, one obtains for the average values $\bar{x}$ and $\overline{x^2}$ of x and x^2 respectively for repeated observation the expressions :

$$(1) \quad \bar{x} = x_0 e^{-\beta t}$$

$$(2) \quad \overline{x^2} = \frac{D}{\beta} (1 - e^{-2\beta t}) + x_0^2 e^{-2\beta t}$$

where $1/\beta$ is the relaxation time of the system and D is given by

$$(3) \quad D = BkT$$

the "mobility" B being defined as the steady velocity the system would acquire under the action of a constant unit force by virtue of the damping mechanism.

Introducing the deviation

$$(4) \quad \delta x = x - x_0$$

one obtains from (1) and (2)

$$(\overline{\delta x})^2 = \overline{x^2} + x_0^2 - 2x_0\bar{x} = \frac{D}{\beta}(1 - e^{-2\beta t}) + x_0^2(1 + e^{-2\beta t} - 2e^{-\beta t})$$

and

$$dt \left(\frac{(\overline{\delta x^2})}{2} \right) = D e^{-2\beta t} + x_0^2 \beta (e^{-2\beta t} - e^{-\beta t})$$

which for small values of t becomes

$$(5) \quad \frac{d}{dt} \left(\frac{(\overline{\delta x^2})}{2} \right) \approx D$$

Finally interchanging the two operations of time derivation and averaging one can rewrite (5) in the form [14]

$$(6) \quad \left(\delta x \cdot \frac{dx}{dt} \right) = \Delta x \cdot \Delta \dot{x} \approx D$$

Δx is clearly the uncertainty of the measurement of x owing to the Brownian motion and $\Delta \dot{x}$ the analogous uncertainty of the measurement of $\dot{x}$. Thus, in the average, the combined error in the measurement of x and $\dot{x}$ is seen to be of the order of magnitude D , that is, independent of the relaxation time of the instrument, as already indicated in section 2.

Suppose now that the instrument is being used for the measurement of a certain physical quantity y and its time derivative $\dot{y}$. It then follows easily from (6) that the product of the error Δy in the determination of y and the error $\Delta \dot{y}$ in that of $\dot{y}$ will satisfy a formula of the type

$$(7) \quad \Delta y \cdot \Delta \dot{y} \approx D' = kTB'$$

where B' is a constructional constant of the instrument which does not depend on its relaxation time.

It is clear from dimensional considerations that a mere magnification of the scale of x cannot change the right-hand side of (7). Thus, once the Brownian motion of the instrument has become apparent, no increase of the accuracy of observation will be gained by, for example, optical magnification of the deflection or by any of the very ingenious means for increasing the accuracy of the reading of a light beam deflection, like the thermo-relay and the photo-relay. The only effective way of diminishing the experimental error arising from the thermal fluctuation of the instrument is to

decrease the temperature T . But, as the derivation of (7) is based on classical statistics, this argument breaks down at very low temperatures. Indeed, we shall see later that under such conditions Heisenberg's uncertainty rule has to replace (7).

Very similar considerations can be applied to non-mechanical, *i.e.* purely electrical or optical, means of observation, and also to mixtures of all these. Again a formula of type (7) is obtained, where T is the temperature of the observing device and B' a characteristic constructional quantity of the instrument. Again no increase in the accuracy by pure amplification is possible beyond a certain limit. For example, no useful purpose is served by increasing the gain of an electronic amplifier once the electrical fluctuations have become apparent.

We now turn to the question of the statistical fluctuations of the observed quantity itself. It will suffice to analyse in this light the most fundamental type of measurement, namely the simultaneous determination of the position x and the corresponding velocity $v = \dot{x}$ of a small material particle subject to Brownian motion.

Imagine first the object of observation to be a swarm of many identical particles which at $t = 0$ are supposed to be ideally localised, *i.e.* to be contained within a volume element surrounding x . After a short time t the swarm will have spread out by diffusion, and thus the "position" x of the swarm will have become subject to an error equal to the mean spread Δx of the Gaussian density distribution around x . At the same time the mean drift velocity of the swarm will be subject to an uncertainty Δv owing to the superposed "diffusion velocity" of the swarm particles in all directions. It has been shown by the present author [15] on the basis of the classical theory of diffusion that

$$(8) \quad \Delta x \cdot \Delta v > D$$

where D is the diffusion coefficient of the swarm particles in the surrounding medium. This is analogous to (6) and the analogy becomes complete when one remembers that according to Einstein [1] the diffusion coefficient D satisfies the relation (3).

This result can immediately be applied to the observation of a single particle, as the statistical distribution of a swarm of identical particles is the same as that of the repeated observation of a single particle. Suppose, for example, that the particle considered is subject to the action of gravity and hence would move downwards with a constant velocity ϑ in the absence of Brownian motion. The actual position-time record $x(t)$ will deviate from a straight line as shown in Fig. 4a. In order to determine ϑ from this diagram one

can construct the corresponding slope diagram which is shown in Fig. 4b, and which exhibits the spread of the observational accuracy of $\dot{x}$. If one again tries to increase the precision of the measurement of x by using a microscope of greater magnification, one will obtain something like the diagram 4c, which represents the beginning of

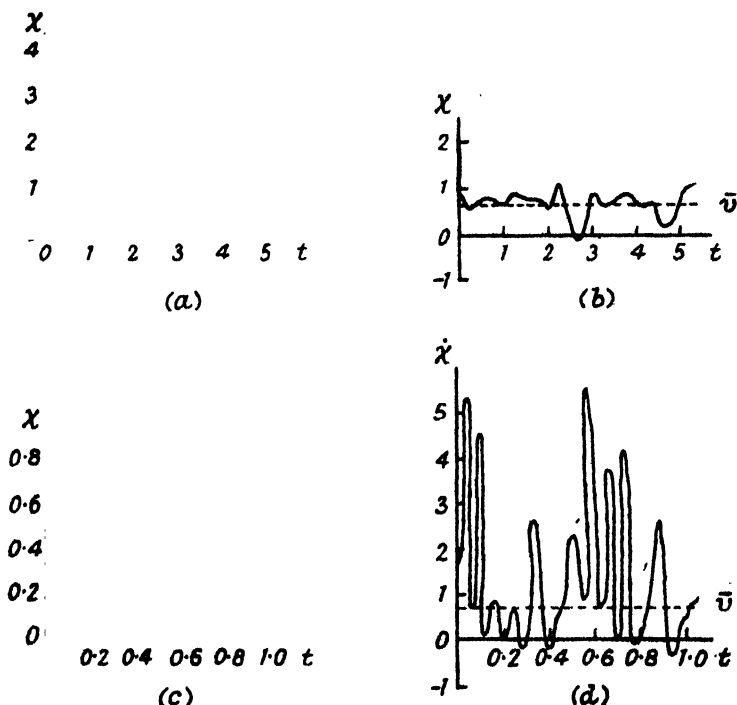


FIG. 4.

- (a) Schematic diagram of the position x of a uniformly moving particle with superposed Brownian motion plotted against time t .
 (b) Slope diagram of (a), showing the velocity v as a function of t ; $\bar{v}$: mean velocity.
 (c) First part of diagram (a) at higher resolving power.
 (d) Slope diagram of (c).

(After R. FÜRTH.)

the curve 4a at higher resolving power. But, again constructing the slope diagram 4d, one realises at once that what is gained in the accuracy of x is lost by the apparent increase in the spread of v about $\bar{v}$. This reflects the well-known phenomenon that the path of a Brownian particle seems to become more and more complicated the more one increases the magnification of the observing microscope.*

* This, according to G. Bouligand [16] has a very interesting analogue in the geometry of curves representing functions without derivative. A

In actual practice the thermal fluctuations of both the observed and the observational systems, and their mutual interaction, have to be taken into account. In the simplest case both systems form parts of one common system in thermodynamic equilibrium at the same temperature T . It can be deduced from general principles of statistical thermodynamics that, under these circumstances, the Brownian movement of the measuring instrument is not affected by the thermal fluctuations of the measured quantity and, *vice versa*, the magnitude of the latter fluctuation is not altered by the reaction of the measuring instrument on it, as indicated in section 1. For example, the fluctuations exhibited by a supersensitive galvanometer can be equally well attributed to its mechanical Brownian movement as to the electrical fluctuations in the connected circuit or the fluctuating interaction between both. Thus, formula (7) will remain in force where B' now depends on the mechanical and electrical properties of the galvanometer plus attached circuit.

The more general case, where the temperature is different in different parts of the whole system, was first treated theoretically and experimentally by L. S. Ornstein [17] and his collaborators in the above-mentioned particular case of a galvanometer with different parts of the circuit kept at different temperatures. It appears that the formulæ for the Brownian motion remain unchanged, except for the fact that the temperature T is to be replaced by an "effective temperature" T^* which is a sort of average temperature of the system, the mode of averaging depending on the coupling mechanism between the different parts. Accordingly formula (7) will again apply when T is replaced by T^* .

In order to increase the accuracy of observation it will obviously be advantageous to keep T^* as low as possible. But one cannot proceed indefinitely in this direction, as the very nature of the coupling mechanism may not permit this to be done without impairing the sensitivity of the method. Take again as an example the observation of the path of a small particle either by direct visual observation or by photographic registration. Here the light scattered by the particle into the observing device plays the rôle

tangent to such a curve can only be defined, within a finite arc Δs surrounding a point A , as a straight line through two points at distance Δs . Owing to the ambiguity of this construction rule, the direction of the tangent will be defined only with a certain finite accuracy Δt . As Δs is made smaller (by a change of scale) Δt increases and becomes infinitely large for $\Delta s = 0$, because of the non-existence of the derivative. But the product $\Delta s \cdot \Delta t$, which constitutes the "geometrical uncertainty" of the curve, is independent of the scale.

of the coupling mechanism. Evidently if weak illumination is used the magnification has to be low, so as to make the observation of the particle possible with the given sensitivity of the eye* or photographic emulsion; on the other hand, by using strong illumination, that is high temperature radiation, one will be able to observe under high magnification, but at the same time an additional source of fluctuation will be introduced. In other words, the measuring process will react on the measured object in such a way that it will increase its Brownian fluctuations by the irregularity of the scattering process.

4.—So far our analysis has been completely based on classical considerations. We now have to revise it in the light of the theory of quanta. Let us again first discuss the influence of the measuring instrument on the accuracy of observation, as before. According to the classical conception, a material system under the action of a restoring force ought to settle down to an equilibrium at zero temperature. Not so in quantum mechanics, from the fundamental equations of which it follows that a "one dimensional harmonic oscillator" of natural angular frequency and mass m has a finite "zero point energy" $\frac{\hbar\omega}{2}$.† This means that the system will perform a fluctuating movement about the equilibrium with energy $\frac{\hbar\omega}{2}$, and hence the mean squares of the co-ordinate x and the corresponding momentum p will be finite.

More precisely, it follows from Schrödinger's wave equation that the square of the modulus of the wave function $\psi(x)$ which represents the probability of finding the system in the position x is, for the lowest quantum state, equal to

$$(9) \quad |\psi_0|^2 = \text{const.} e^{-\frac{m\omega}{\hbar}x^2}$$

Accordingly one has

$$(10) \quad \overline{x^2} = \frac{\hbar}{2m\omega}$$

* Another point to be considered is the fact that the eye itself is at a finite temperature and its function therefore to a certain extent subject to thermal fluctuations. It has been pointed out by de Vries [18] recently that these are most probably negligibly small in the case of vision, but not so in the case of hearing, and may thus become significant for the ultimate limit of acoustical methods of observation.

† Here and in the following we use the convenient notation of Bohr:

$$\hbar = \frac{h}{2\pi}$$

This, at the same time, is the uncertainty of the position which, of course, can be made as small as one likes by making $m\omega$ sufficiently large.

Similarly one can derive the wave function $\chi(p)$ for the wave equation in "momentum space," the square of the modulus of which again gives the probability of observing the value p for the momentum of the oscillator. For the lowest quantum state one obtains

$$(11) \quad |\chi_0|^2 = \text{const.} e^{-\frac{1}{\hbar m \omega} p^2}$$

from which follows

$$(12) \quad \overline{p^2} = \frac{\hbar m \omega}{2}$$

Thus the error in the observation of the momentum can be made as small as desired by making $m\omega$ sufficiently small.

One sees at once that the two conditions for reducing the observational error of x and p , formulated at the end of the last two paragraphs, contradict each other. Thus, it comes about that the product of the uncertainties $\Delta x = \sqrt{\overline{x^2}}$ and $\Delta p = \sqrt{\overline{p^2}}$ of x and p , which represents the error of the simultaneous measurement of these quantities, has a finite universal value

$$(13) \quad \Delta x \cdot \Delta p = \frac{\hbar}{2}$$

Using the relation $p = m\dot{x}$, this can be rewritten in the form

$$(14) \quad \Delta x \cdot \Delta \dot{x} = \frac{\hbar}{2m}$$

which is strikingly analogous to (6) and (7). It shows that, by making the inertia of the measuring instrument sufficiently large, the quantum mechanical limitation of the accuracy of the simultaneous measurement of a physical quantity and its time derivative can be made very small indeed, but can never be reduced to zero, not even at zero temperature.

Next we consider the problem of the definition of the movement of particles in the light of quantum mechanics. As already pointed out in section 1, this definition is in principle a statistical one, as the solution of the wave equation gives a definite result only for the behaviour of an assembly of identical particles. It was first realised by P. Ehrenfest [19] that the time-dependent Schrödinger equation is formally identical with the classical differential equation of diffusion, and that the centre of gravity of a swarm of identical particles under the action of an external force moves according to

Newton's laws of dynamics. Consequently, the drift motion of the swarm with velocity v and a diffusion of the swarm particles about the centre of gravity are superposed upon each other.

On the basis of this idea Heisenberg [13] was able to show that the uncertainty Δx of the position x and the uncertainty Δv of the velocity v satisfied the inequality

$$(15) \quad \Delta x \cdot \Delta v > \frac{h}{2m}$$

or

$$(16) \quad \Delta x \cdot \Delta p > \frac{h}{2}$$

where the equality sign holds when the density distribution within the swarm has a Gaussian character. The analogy between (15) and (8) on the one hand, and between (16) and (13) on the other hand is striking [15].

Let us first interpret the result in terms of the measurement of position and velocity of a single particle. In order to account for the diffusion spread of the swarm, we may represent it as a packet of de Broglie waves of different directions and frequencies, each plane wave of definite direction and frequency corresponding to a particle of finite velocity. But, as there is no way of correlating individual waves to individual particles, their movement will not be strictly defined, which, according to Schrödinger [20], can be expressed by saying that they perform a kind of "wobbling motion" about their classical paths. This makes the parallelism to the superposition of the thermal Brownian movement over the regular path evident, and so one sees that the main conclusions about the limit of accuracy of the measurement of position and velocity, discussed in section 3, remain in force, except for the fact that the limit expressed by (15) is independent of temperature.

The analogy between (16) and (13), already referred to, points to the essential feature of the quantum mechanical limitation of accuracy, namely its universal character. Whereas the limits of accuracy of observation, imposed by the thermal fluctuations of the object and the observing device, become identical *only* in the case of thermal equilibrium between them, the limits imposed by the quantum nature of the movement of object and observing instrument are *always* identical. (That (16) is an inequality and (13) an equation is only due to the fact that the particular measuring mechanism used for our considerations is characterized by a Gaussian distribution.)

Consequently we shall have to assume that the third cause of uncertainty, namely the interaction between object and measuring instrument, which constitutes the measuring process, is also generally identical with the two others. Indeed, some authors prefer to explain the quantum mechanical limitation of observational accuracy solely as the outcome of the finite measuring interaction. This aspect has been most thoroughly analysed in famous papers by Heisenberg, Bohr, and others [13], both from a general point of view and illustrated by special examples. One of the best-known and most revealing of these examples is Heisenberg's analysis of the observation of the movement of small particles by means of a microscope, where he showed how the increase in resolving power by the use of short-wave radiation is balanced by the increased disturbance due to the recoil of the observed particle in the process of Compton scattering of the illuminating photons. This results in a limitation of the accuracy $\Delta x \cdot \Delta \dot{x}$ of the order of magnitude $\frac{h}{m}$. This, by the way, is the exact corollary to the discussion at the end of section 3.

Now, whereas for "macroscopic" objects $\frac{h}{m}$ is negligibly small, it is of the order of magnitude unity for electrons. On the other hand, the atomic dimensions are of the order of 10^{-8} cm. and the electronic velocities within the atom of the order of 10^8 cm./sec. Thus, their product is also of the order of magnitude unity, and it becomes clear at once that an observation of the movement of the electron within the atom is out of the question, as the error involved is of the same order as the quantity to be measured; in other words, the observational method is so crude (and that on theoretical grounds) that it completely upsets what it endeavours to investigate. Similar considerations apply to the investigation of intranuclear processes by means of the bombardment of nuclei and the observation of the effects of this bombardment. We can avoid these difficulties by confining ourselves to the observation of the radiations from large assemblies of atoms or nuclei. But even so, we have to put up with the radiation fluctuations mentioned in section 1.

To summarise, we may say that the aim of quantitative observation is the simultaneous measurement of physical quantities and their time derivatives, and that the accuracy of this process has a natural limit imposed upon it, on the one hand, by the chance character of the thermal agitation and, on the other hand, by the statistical nature of the quantum mechanical interaction processes between fundamental particles.

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THE THIXOTROPY OF CERTAIN SEDIMENTARY ROCKS

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THE interesting field of study that includes the thixotropy of geological deposits seems to have been but little explored. Herbert Freundlich's pioneer work of twenty years ago has yet to be systematically followed up, notwithstanding that engineering practice, as well as more fundamental geological work on the origin of sedimentary rocks, has shown the need for precise information about the physical properties of natural sediments in the moist condition. In this article (unless otherwise stated) the term thixotropy is used in its original, restricted sense (Péterfi and Freundlich [1]), for the isothermal reversible gel-sol transformation.

Reference to the thixotropy of individual minerals will doubtless be familiar, since some of them (and the rheology of rocks *en masse* as structural units) have been the subject of notes in the scientific press from time to time [2]. In most instances, however, the work has been carried out on artificially powdered samples or on mechanically-separated "fines" from deposits such as clays. Data concerned with the thixotropic behaviour of bulk samples of naturally-occurring sediments are therefore few.

Freundlich and his collaborators [3] studied the thixotropy of a large number of substances, but included only a few rocks in their survey: these were blue roofing slate, Solnhofen "slate," quicksand and certain clays (unspecified except for bentonite). As a result of his investigations, Freundlich linked the remarkable properties of the Solnhofen lithographic limestone, moulding-sands and quicksands, respectively, with their noteworthy thixotropic behaviour and original loose packing. Moreover, he ventured on the generalisation that almost all non-uniform geological sediments would prove to be thixotropic. From my preliminary examination of such sediments, I believe he was right in his conjecture.

His views on Solnhofen "slate" and quicksands are worth recalling. The famous Solnhofen lithographic stone (actually a

limestone containing a small amount of clay) proved to be strongly thixotropic when ground to a fine powder, the setting being so rapid that, although it could be heard, it could not be seen. (I still remember the characteristic "click" of the samples in the tubes he carried around in his pocket, and the engaging boyish pride with which he exhibited his skill in this jugglery.) Othenius Abel had explained the remarkable petrification of the organic remains in the Solnhofen stone (which included delicate forms like jelly-fish and insects, and ammonites which not only lay flat but occasionally remained upright on their keels and uncrushed) by suggesting that they were enveloped in wind-blown dust that had formed a sludge on the sea-shore. It seemed that the paste must have "solidified" quickly, since putrefaction was delayed sufficiently to permit the retention of fine impressions of soft parts that would rapidly decompose: yet, the absence or rarity of shrinkage-cracks showed that the deposit was not quick-drying. Freundlich considered that its thixotropic properties best accounted for these peculiar features.

He brought out the further point that thixotropy is often increased by the addition of an electrolyte, and that many substances proved to be more thixotropic in salt water than in pure water. For example, powdered quartz (particle diameter 1 to 10 μ) is thixotropic in the former but not in the latter. Also, the addition of a small quantity of a fine clay or other colloid which was strongly hydrophilic served to make a dilatant sand thixotropic. At Professor F. G. Donnan's suggestion, he investigated some British "quicksands" and found that one of them, from Knott End, Fleetwood, was markedly thixotropic, but that others from New Brighton and Southport were not. Now it is an interesting fact that, from my knowledge of these deposits, I should have said that the New Brighton and Southport examples were, unlike that from Knott End, not true quicksands. The Knott End sample was thixotropic in a mixture consisting of 3 gm. of sand to 1.4 c.c. of sea-water after a rest-period of 5 to 10 minutes. It contained 2.1 per cent. of clay disposed as pellicles round the quartz-grains. After this clay was eluted, the sandy residue became dilatant and non-thixotropic, but the clay fraction itself was strongly thixotropic and plastic. At the thixotropic limit the volume of water evidently amounted to 55 per cent. of the whole.

EVALUATION OF THIXOTROPY

The methods of measuring thixotropy in absolute units do not seem to be readily applicable to these naturally-occurring sediments.

However, their thixotropic limits can be approximately determined by the inverted tube method. Representing by N the ratio : volume of liquid to volume of solid at the point when, after a rest of 1 minute, the mixture just does not flow in a vertical inverted tube, but flows freely if a slight tap is given to the tube [4], one finds that preliminary tests show values for N varying from 0.6 in the case of ferruginous or slightly clayey sands to 2.5 with typical samples of marine muds such as the Lias, Oxford Clay, Kimmeridge Clay, Gault or London Clay. Exceptionally, as in the case of the freshwater Oligocene or Eocene ball-clays from Devon or Dorset, or certain marine clays ("fat" clays), N reaches values from 2.7 to more than 3.0. Other results are as follows, the range in N being due to the samples having been obtained from various localities, or to treatment with pure water, river-water or sea-water respectively.

Recent estuarine or marine muds 1.4 to 2.5 ; Glacial brick-earths and boulder clays 1.1 to 1.9 ; kaolin (china-clay) c. 3.7 ; Chalk, Upper and Middle 1.1 to 1.6, Lower (Chalk Marl) 1.8 ; drewite 1.9 to 2.6 ; ferruginous sands such as those of the Greensand and Bunter 0.7 to 1.1 ; Palæozoic shales 0.9 to 1.4 ; moulding-sands, including mixtures, from various geological formations 0.5 to 1.6.

From the last figures mentioned, it will be noted that I have been unable to confirm Freundlich's speculation that moulding-sands are characterised by strong thixotropy, although they are necessarily materials with loose packing and are plastic.

The results summarised above give an idea of the degree of thixotropy that may be displayed by natural sediments which consist, of course, of mixtures of particles differing in size, shape and mineral character, and vary from place to place. Individual minerals that have been powdered or artificially sorted so that the bulk consists of grains of equivalent diameter $< 10 \mu$ show higher values for N , e.g. kaolin 2 to 16, mica 4 to 6.

With these figures may be compared that for N in the case of the remarkable clay, bentonite, from Wyoming, which reaches 27.0, a value far in excess of that for any other sedimentary rock known to me.* Indeed, bentonite is *sui generis* ; it differs from the next most thixotropic sediment as greatly as does diamond in its hardness

* This figure is probably not strictly comparable with the others, since much of the liquid may be absorbed within the loosely-bound layer-lattice of its essential mineral, montmorillonite, and thus does not assist the thixotropy.

from the next hardest mineral, corundum. In each case, however, artificial products have been prepared from natural materials in order to bridge the gap. Among British sediments, Fullers' Earth appears to resemble bentonite most closely. Although samples collected in the field and subsequently rubbed down give values for N not greatly above those of some of the fat clays mentioned, the marketed fine-grained products show exceptionally high thixotropy (N , 4 to 9.8).

For convenience, the volume at a setting time of 1 minute has been taken, but it is well known that the time increases with the ratio of the volume of liquid to volume of solid up to the limit when the system is no longer thixotropic. Experiments have shown that the time taken for clay-water mixtures to set may vary from less than a second to more than a week. The length of time that the process may occupy in nature is a matter for conjecture.

The shape of the solid particles as well as their degree of fineness has long been known to influence the thixotropy of their pastes, a flaky or acicular habit promoting it. Sediments composed dominantly of platy minerals such as the micas, chlorites and most clay-minerals exhibit the property to an enhanced degree as compared with those consisting of approximately equidimensional grains like calcite or quartz (*cf.* the figures for clays and chalks or sands). Of the clay minerals, the montmorillonite group is far more reactive than any other.

When we recall the remarkable behaviour of vanadium pentoxide, where less than 1 per cent. forms a system with water, our attention is naturally drawn to the possible effect of rod-shaped minerals in sediments. But, whereas platy minerals are very abundant as rock-constituents, acicular or rod-like minerals play only an accessory role in sedimentary rocks—with one exception, aragonite. (There is a possible second exception, the rare clay-mineral, attapulgite.) Aragonite, the orthorhombic form of calcium carbonate, makes up the bulk of the peculiar sediment known as drewite, a mud now being deposited around the coasts of the Bahama Islands and Florida, presumably through the agency of bacteria. Although its chemical composition is practically identical with that of common chalk (which is composed, however, of equidimensional calcite), its thixotropy is much more pronounced.

It is common knowledge that the thixotropic properties of clays have long been utilised in the arts and, perhaps most significantly from the standpoint of applied geology, in the preparation of muds for wall-building when borings are in progress for oil or water. Of the output of bentonite clay in the U.S.A., for example, no less

than 38 per cent. is used in drilling, a larger quantity than for any other single purpose. Here, a system with high thixotropy is required: by way of contrast, in the cement industry, in which a thixotropic limestone-clay slurry has to be pumped through pipes, low thixotropy and a minimum content of water are desirable. Turning from applied to fundamental science for another example, we may note that G. Chapman and G. E. Newell [5] have recently illuminated a different aspect of the subject by describing how lugworms are dependent on the thixotropic properties of sea-shore muds, into which they are able to burrow after promoting the gel-sol transformation by shock. These investigators found by experiment that a small disc would penetrate such muds, when in the sol condition after disturbance, at a pressure of only 41 gm./cm.², but that when the gel condition was restored after a short rest, a pressure of 412 gm./cm.² was necessary at the surface and 720 gm./cm.² below a depth of 6 inches to 1 foot. Thus the resistance became almost 18 times as great. Geologists will welcome data such as these since they indicate, for the first time, the order of magnitude of the yield value. When this work is followed up, we may hope for light on many puzzling problems in geology, such as the effects of differential loading of sediments or of earthquake shocks, the conditions requisite for sliding and internal disturbance of bedding, and the rate at which compaction and diagenesis are likely to proceed.

Measurement of the thixotropy, not only of present-day sediments, but also of deposits laid down in the past, is desirable. Some of the well-known horizons of the geological column in Britain (like the Gault and London Clay) are already being drawn upon to provide clay, in substitution of imported materials, for purposes which depend on the properties of which their thixotropy is a manifestation. Any economic method of activating such materials is worthy of further research.

The volume-relations in thixotropic mixtures of minerals and rocks are of some significance. As Freundlich and others long ago showed, many substances form thixotropic pastes over a range in which the solid may vary by as much as 30 per cent. by weight, i.e. up to 18 per cent. by volume in pastes of ore-minerals. I have found that the range in natural sediments may be as much as 13 per cent. by weight, i.e. about 10 per cent. by volume. Obviously, the figure for N expresses the void-ratio at the limit where the quantity of liquid in the mixture is a maximum for thixotropic behaviour after a brief rest. The "porosity" at that limit (i.e. the volume of water expressed as a percentage of the total volume)

is given by $100 \times N/(N + 1)$. In calculating the value of N , the densities of the solid and liquid must either be determined or assumed from a knowledge of the type of deposit. Calculations show that the error in N caused by a small error in the figure for density is not serious. The values of N given above show that the liquid content ("porosity") of the thixotropic mixtures may be as low as 35 per cent. by volume or as high as 75 per cent., if we exclude the exceptional sediments; in the case of the latter, gels are formed with a liquid content ranging from 80 to 97 per cent.

With few exceptions, the thixotropic values hitherto published refer to mineral pastes [4] made with organic liquids, pure water, or water to which electrolytes such as NaOH, KOH, NaCl, etc., had been added in varying amounts. These results (which show that N is usually, but not invariably, increased), although of much interest in themselves, cannot be related to geological phenomena in the same way as mixtures with river-water or sea-water. Experiments with fresh water from the Thames and sea-water from Plymouth, of known composition, show (as would indeed be expected) that the thixotropy of particular sediments differs according to whether river-water or sea-water is used and differs in turn from that of the pure water control mixtures. It is interesting to find that, as a general rule, the values of N seem to be higher for river-water than for sea-water, notwithstanding the greater content of electrolytes in the latter. There are many exceptions, however, which make it evident that the work needs to be systematised and extended before the significance of the figures can be properly appreciated.

The addition of a small quantity of a colloid such as water-glass (1 to 3 per cent. by volume according to the type of sediment) often causes a substantial increase in N , which may become as much as 60 to 120 per cent. higher with a fat clay, and about 30 per cent. greater with a clayey sand. Colloidal silica produces similar but less strongly marked effects. It is noteworthy, however, that calcareous sediments such as chalk, limestone and drewite are but little "improved" by such means. Apart from treatment of this kind in order to increase the thixotropy of natural materials for industrial purposes, the use of weathered, in preference to unweathered, clays is supported by such values of N as those for grey London Clay 1.7, and weathered brown London Clay 3.0. Tempering and ageing of clays thus causes the thixotropy to increase; also, there remains the obvious method of increasing it by rough washing which removes small fractions of coarser grades (e.g. for natural Gault N may be 2.8; after rough washing and settling, 3.4).

THIXOTROPIC SEDIMENTS AND ENGINEERING

The correlation of thixotropic measurement with data obtained from soil-mechanics tests, such as those for liquid limit, plastic limit and constants of compressibility, will undoubtedly prove to be of value. Although the respective figures are available for certain sediments (*e.g.* Oxford Clay, London Clay, drewite, kaolin and others) the thixotropic and other tests have not been carried out on one and the same sample in each case. For the moment, progress is halted in this direction, but reference may be made to the kind of practical problem that is related to this aspect of the subject.

The fact that pile-driving may liquefy sediments has long been on record, but it may not be generally known that there are clays which are firm when dug, but which subsequently turn liquid as they are dumped from drag-line buckets. For example, R. F. Legget [6] has recently described an interesting case of the sudden flow of a previously-stable lake sediment in Ontario, after it was disturbed at the base of its steep slope. Such "mud runs" occur with surprising speed when once started and may involve as much as 300,000 cubic yards of material. The surface of such slides dries out more quickly than the original deposit, presumably because of the loss of excess water. These muds are doubtless thixotropic and, to the valuable figures given by Legget for water-content, liquid limit, plastic limit, etc., I would add that the estimated void-ratios at various depths worked out at from 2.5 to 1.5, *i.e.* just about the thixotropic range that these muds might be expected to show if we have regard to their mechanical constitution.

Most of the laboratory tests that have been standardised for the purposes of soil-mechanics require apparatus which is not easily transportable or suitable for use by geologists engaged on field-work far from a base. Just as blowpipe-tests were introduced for convenience in mineral prospecting to replace chemical analyses, field-methods can be substituted for certain of the soil-mechanics tests (*e.g.* liquid limit). Thixotropic limits of sediments are to my mind more informative to the geologist than liquid limits, which lie somewhere between the upper and lower thixotropic limits; moreover, the evaluation of thixotropy requires no more field apparatus than a portable balance, a few 10×1 cm. test-tubes and a dropper. The plastic limit and natural moisture-content can be found if the equipment also includes a small drying-oven. Mechanical analyses (for what they are worth) can be carried out by sedimentation, and densities determined by field-methods familiar to geologists.

GEOLOGICAL SIGNIFICANCE

Although the tests so far made on geological sediments number only a few hundreds (small in relation to the extent and variety of deposits that should be investigated), it may not be unprofitable to reflect on the implications of the figures when we try to envisage the conditions on or below the sea-bed or bottom of a lake where a representative sediment such as a sandy mud or clay is being laid down. At the same time, we shall be wise to bear in mind that, in the history of geology, the grist put into the mathematical mill has usually been so rough that the products have proved to be rather indigestible.

An interesting field is evidently here in prospect of being opened up, but our efforts to explore it are handicapped by the scarcity of information about certain features of present-day sediments, notably their moisture-content at various depths. From numerous samples, it is evident that muddy sediments within the intertidal belt around our shores are in the thixotropic state. The shore muds have a void-ratio which is not high, varying from 0.7 to 1.4, doubtless because they are partially dried out between tides. On the floors of estuaries or off-shore areas (where the muds are more fluid) the value may become 1.9 to 2.5 or more. As the depth increases, the pressure exerted by the overlying water causes compaction, as shown by a gradual reduction in the void-ratio with depth. From the surface of the sediments downwards, the compaction is liable to be further increased by the load of the column of mud, and the void-ratio correspondingly decreases. Although the rate has been experimentally determined with some accuracy [7], the samples have to be slurried before being tested, so the results are not comparable with those for natural sediments.

If the few available data relating to void-ratio and depth of sediments now being accumulated are representative of conditions of deposit, we can obtain a general idea of the range in depth within which the muds are likely to remain thixotropic. The great majority of sediments examined from Britain and abroad have void-ratios (with river- or sea-water) at the limit N varying from 1.3 to 2.7 and are thus presumably thixotropic down to depths of the order of 100 feet, or less if they are also under a load of water. For each individual sediment the depths corresponding to its upper and lower thixotropic limits will depend on its mechanical and mineral composition. Some experimentally determined differences in the void-ratios at the upper and lower thixotropic limits of particular sediments are: for "fat" clays between 1.5 and 1.1, stiff clays and

estuarine (probably organic) silts 0.5, 0.4 and 0.35, sandy ("lean") clays 0.2 and 0.3, chalk 0.27, and ferruginous sands 0.1 to 0.2. The depth ranges for these thixotropic limits appear to be of the order of 10 to 30 feet near the surface and possibly rather less at greater depths, but these estimates may have to be revised when we have more accurate void-ratio curves for existing sediments. At least we can conclude that the depth-differences are not likely to amount to hundreds of feet.

Apart from the bearing that thixotropic properties may have on the conditions that favour the preservation of organisms in the chalk or other limestones, flint or chert, coal-gels and certain clays, the possible influence of thixotropy on the behaviour of sediments during the early stages of their diagenesis is not without interest.

Below the bottom of its range (or thixotropic zone, as it may be called) a sediment will not, *ex hypothesi*, contain sufficient water to be thixotropic (s.s.), but will be more or less plastic according to its constitution.* It will still be thixotropic in the broader sense, however, since such a clayey material becomes more plastic under stress and disturbance, but reverts to a condition of greater rigidity when left undisturbed. At the bottom of its thixotropic zone the sediment will contain just enough water to flow under shock, but will become firm again so soon as disturbance ceases: and at some distance above that level, the thixotropic value N as defined above (i.e. after one minute's rest) will be attained. Thence, as it is followed upwards to the top of its zone, the sediment will behave as a thixotropic mixture, but will take longer and longer, as the depth decreases, to set after the cessation of disturbance. Above the top of its zone the water-content will be too great for the transformation to take place. However, the water-saturated sediment will still be anomalously viscous and in that sense thixotropic. The zone for strongly thixotropic materials will obviously tend to be nearer to the sea-floor, and that for clayey sands or other sediments of low thixotropy will extend to greater depths.

Remembering that thixotropic mixtures offer relatively great resistance to shearing stress, we may expect the compaction of a column of sediment to proceed, not evenly with the passage of time, but *per saltum* as the yield-value is reached for each thixotropic stratum. If the mechanical constitution and lithology of the various

* Freundlich noted that plasticity and thixotropy are usually (but by no means invariably) sympathetic properties, and that dilatancy and thixotropy are antipathetic (which is also true of geological deposits). E. L. McMillen went so far as to style thixotropy and plasticity "twin phenomena."

constituent beds in a column differ appreciably (as is frequently the case), the resistance to the stress will also vary, and compaction is likely to proceed irregularly, older beds in some cases taking longer than younger ones to consolidate. Since the reduction in volume of columns in adjoining areas may also proceed irregularly, the adjustments may be spasmodic, possibly even giving rise to faults. The nature of the colloids that envelop the particles of the sediments will also influence the rate of compaction; if they are of suitable composition and highly resistant to dehydration, compaction of even ancient sediments may be long delayed, as in the case of the famous Cambrian clays of the Leningrad district, which are still unconsolidated and plastic, although their age must be almost 600 million years [8].

It thus seems possible, to put it no higher, that the physical characters of muddy sediments which give rise to phenomena such as thixotropy will exert an influence on their behaviour under load or shock, especially where beds of different lithology alternate in a geological succession. Here, indeed, we may have a clue to some at least of the puzzling features of intraformational disturbance or of irregularities of bedding, jointing, or other fractures. In those cases where small residual masses of bedded sediment occur as boulder-like bodies in unstratified material, the phenomenon can be most simply explained as the result of variation in the thixotropic limits in different parts of the deposit due to differing mechanical and mineral constitution.

The only reference I have found to thixotropy in the purely geological field is that by H. V. Dunnington [9], who made passing mention of the momentary mobility of a mud under seismic shock as a possible explanation of the disturbance of an argillaceous bed in a Carboniferous limestone-shale succession. We await experimental evidence, however, in support of the view that the type of disturbance described is connected with thixotropic behaviour, but we know that the ranges and limits of thixotropy and plasticity for clays differ considerably from those for lime-muds.

Gravitational sliding of muds on the sea-floor will take place only if the void-ratio is greater than N , and at that figure shock is necessary to promote movement, even at a very high angle of slope (which in the tests is vertical). Since the average floor-gradients are usually low, we are justified in concluding that earthquakes or sudden adjustments to differential loading would be required to start the movement. Further, it appears that only a small thickness of muddy sediment is in a condition to flow, a view which accords with present-day records of the small scale of

subaqueous sliding, the only examples of which (apart from conjectured slips on deep-sea faults) indicate that landslips or loading by structures near the shore were contributory factors.

The thixotropic transformation and breakdown of the system in waterlogged clays or sands will result in the extrusion of water where conditions are favourable for its escape. Such prorrptions of water at the surface are a common accompaniment of earthquake disturbances.

The difficulty hitherto met in accounting for beds of unstratified material in the geological column may be resolved if it is established that the original bedding-planes were eliminated by shock due to adjustment to differential load, the passage of earth-waves, or other causes while the sediment was still in a thixotropic state. The action of boring organisms in re-sorting sediments and thus destroying stratification has often been called to aid, but the thoroughness with which the process had to be carried out seemed to some critics to necessitate organised and "directed" labour! However, from the work of Chapman and Newell referred to above [5], it is probable that such boring organisms penetrate the deposit only by taking advantage of its thixotropy. The purely physical and the biological explanations may therefore not be mutually exclusive. It would be interesting to know what is the sphere of influence of a living animal like the lugworm when it induces by shock the gel-sol transformation in order to penetrate a sediment.

The study of the thixotropy of sedimentary rocks appears to be an appropriate field for co-operation between civil engineers and geologists, and one where both parties would benefit by the advice of physical chemists—as, indeed, I have benefited (and here make grateful acknowledgment) from discussions with Sir Chas. Goodeve, F.R.S., and Dr. F. J. Burger.

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PURE AND APPLIED SCIENCE OF THE SEA *

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IN these days when discussion of the relative values of pure and applied research is so rife, it is as well for us all to weigh matters, each for his own branch of science. It is true that the advent of the atomic bomb, which was burst so suddenly upon the public, gave an enormous boost to the advocates of fundamental research ; for all could realise that it was pure research which led to the knowledge necessary for the liberation of atomic energy. But now, owing largely to the present economic situation of this country and the world, the supporters of short-term applied research are striving hard to get the lead. At the outset all should realise, however, that the first question raised when any applied problem is attacked is, what is the fundamental knowledge with which we can start ?

A study of the history of the science of the sea teaches us, as well as any other branch of science, how the quest for knowledge for its own sake and the search for information bearing on applied or economic problems are in fact mutually interdependent. In the course of this lecture I shall give several modern instances in which we may see the thread of knowledge passing to and fro between the pure and the applied in the growing fabric.

For centuries man has navigated the seas and oceans, and intrepid explorers have gone forth to chart and to map. But in early days the chief interest lay in determining the limits of the ocean, so as to map the land masses of the globe, and in producing accurate charts of the coasts and shallow waters for purposes of navigation.

Early last century for various reasons thoughts were turned seriously for the first time towards the study of the sea itself.

* Being the Ludwig Mond Lecture, delivered at the University of Manchester on March 11, 1948.

Biologists were, for instance, becoming interested in the variety and distribution of marine animals and plants and the knowledge of the shallow seas was growing apace. But the deep waters of the oceans remained as a largely unknown and unplumbed world. Indeed, it was generally supposed that conditions in the depths were such that no life could exist beyond a certain limit.

It was at about this time that advances in an entirely unrelated and applied branch of science had led to a method whereby messages could be sent along wire cables. Lines of cable were to be laid for that purpose across the oceans. To do this there was need for a knowledge of the contours of the ocean bed, and as a result some attention was diverted from the study of the ocean's boundaries to that of its bottom configuration. In the year 1860 one of these cables parted; when the broken ends were brought to the surface biologists were astonished to find living organisms attached to the cable covering. So life existed beyond all doubt in great depths after all. This discovery gave a great stimulus to ocean research. At the same time the students of the pure science of evolution imagined that in the ocean depths would be found all kinds of unknown and primitive creatures, a knowledge of whose structure would throw light on that greatest of all problems, the origin of life.

Such happenings and thoughts were the driving force that resulted in the sailing of H.M.S. *Challenger* on one of the greatest voyages of exploration of all time, with its chief aim the study of the sea in all its aspects. The science of oceanography thus had its foundations firmly laid, but sad to relate no great number of primitive creatures was found, and it is now generally thought that the greater part of the deep-sea fauna has been derived by migration and evolution from shallow waters.

Now, at the time of the *Challenger* Expedition and for many years after, when expeditions from all nations sailed the oceans, the only means of finding the depth was by sinking a weight on the end of a line, at first of rope and later of wire. This was very laborious, and in deep water a single sounding often took several hours to complete. A chance discovery made during applied research in the first world war has changed all this and led to great advances in fundamental knowledge of the sea in different directions and now once more to new economic applications.

While experimenting on the detection of submarines by echoes from sound waves directed horizontally, it was incidentally noticed that echoes could also be received vertically. The modern echosounder resulted and during the period between the wars was

developed both for purposes of navigation and for the charting of the greatest ocean depths. The laborious sounding by lead and line was superseded and our ideas of the configuration of the ocean floor were radically changed. It was now possible to measure the depths at very close intervals. In the past the few and widely scattered soundings had given the impression that the beds of the deep ocean were vast level, or at most slightly undulating, plains. The echo-sounder showed in many places hills and dales and even mountain chains as on land. A detailed knowledge of this new picture has yet to be built up.

At times, while working in shallow water, a double echo was noticed; it was found that echoes were being received from the surface of the mud bottom and the rock underlying the mud deposit. In this way it was possible to measure the depth of the bottom deposit. Other subtle distinctions in the records indicate the type of bottom.

A natural development from this has been the study of the sea bottom by seismic methods. If an explosive charge is detonated on the sea floor waves of compression pass through the bottom deposit at all levels, and through the underlying solid substratum forming the foundation rock of the sea bed. By measuring the differences in the times taken for the several waves to traverse their courses, it is possible to calculate the depths of deposits in deep water. A new vista in knowledge is being opened up as a result, which it is hoped will throw much light on the structure of the ocean basins and their geological past. Results have, for instance, already shown thicknesses of sediment on the continental shelves on both sides of the Atlantic exceeding a mile in depth. Here we have a valuable new tool for the science of geology, with academic and commercial implications.

And now the echo-sounder is being put to a new use. Its sound waves are reflected from solid objects in the water as well as from the sea bottom. It can, therefore, be used to detect fish. In the commercial fisheries shoals of herring and cod are now being located by this means. The depth of a shoal in the water is accurately indicated, and the extent and density of the shoal itself made evident. The fisherman can, therefore, tell at once whether the fish are sufficiently numerous to be worth fishing for and the depth at which his lines or drift nets should be set. It is even claimed that in some instances the species of fish can be distinguished. Apart from its applied value, it is likely that much of academic interest on the habits of the fish will now be learnt. These acoustic methods are now also being used to study the vertical movements

of minute drifting animals which live in the water layers above the bottom, and it is possible that organisms of whose existence nothing is yet known may thus be discovered in the deep waters of the ocean.

It was perhaps especially during the second world war that investigations with modern underwater sound detecting gear received most attention, because of its importance in submarine warfare and harbour defence. One of the most interesting discoveries made during the war in the course of these investigations was that in some localities the sea is far from being the silent world one has always pictured in the imagination. It was found that some areas were exceedingly noisy. Most prevalent was a high-pitched crackling sound which was almost continuous day and night. Other sounds showed seasonal or daily variations in intensity. The continuous sound was tracked to a shrimp, a special variety with a large claw, the joints of which it periodically snaps together. Species of this kind occur in British waters, and if an individual is brought into the laboratory the occasional clicking-snap of the claws can be clearly heard. What was not known was that beds of these shrimps covered considerable areas, and the sum total of the periodic occasional clicks by each individual is sufficient to create a considerable crackling background of noise. One can imagine it sounding to the fish much the same as the grasshoppers in a field on a summer's day do to us. A new method of attack for studying the distribution and habits of these animals in nature has thus been opened up and their beds can be found and their extent plotted. The attention of the pure investigator is at once invited also to find out why the animals make these noises.

Another line of investigation of great academic interest taken up during the war was the study of waves. The aim of this research was first and foremost to obtain information on surf conditions, which was wanted in considerable accuracy for landings on the beaches of Normandy, North Africa and elsewhere, and this demand was fulfilled to a remarkable degree. But the researches also led to a detailed study of the waves themselves, whereby a number of different wave systems produced by meteorological conditions miles apart could be disentangled. To obtain the initial records of the integrated wave motions, pressure gauges were laid on the sea floor and the fluctuations in pressure due to the heights of the waves recorded electrically on the shore. But I mention this research especially here because another method employed was to place an echo-sounder on the bottom of the sea and measure the depth of the water continuously by reflection back from the

surface—a method which had in fact been used just before the war ; and now the microseismic waves recorded on seismographs at Kew and other places have been traced to wave activity in the sea.

I think I have said enough on this one field of oceanography to show how many interests are interrelated and how introduction of new methods and the impact of the driving forces of pure and applied science lead to advances on many fronts. But I am a biologist, and it is in the biological field that I am more competent to roam. The unusual is always attractive, and perhaps as a result undue attention has been paid to the detailed study of the rarer creatures of the sea. From the academic point of view this is perhaps natural ; in the fundamental problem of evolution, around which so much of biological research has centred, it is the unusual animal which is often of greatest interest. This was the incentive which drew so many zoologists to study systematically the great variety of life in the sea and build the foundations of our knowledge of the structure and classification of members of the animal kingdom ; for, insects apart, by far the greater variety of animals live in the sea than elsewhere and many groups are found almost exclusively therein.

It is only in recent years that the lives of some of the most common marine animals have formed a major field for research. This has paid, because by their abundance alone they afford material for statistical observations which cannot be made on rarer animals. It will pay because it is the common animals which are most important in the economy of the sea, and sooner or later the knowledge will be needed. During the war, for instance, when certain raw products were denied us, it was found that we knew hardly anything about our common seaweeds from which these products could be derived.

The original cause of this alteration in direction in research was economic. The increasing efficiency and size of our fishing fleets brought fear to some minds that too great inroads might be made on the stocks of our food fish supply in the sea. As a result biological studies of the sea, which had in the past primarily been undertaken by private enterprise or on specially financed expeditions, came under the direction of national governments. To answer the vital overfishing problem it was necessary to know rather quickly a great deal more about the sea and life in it than could be discovered in the time by unco-ordinated investigation. Research was planned on broad lines and co-ordinated at an International level. The International Council for the Exploration of the Sea came into active being in 1902 and has been in operation ever since.

But, while the urge was economic, the immediate need was for fundamental knowledge. In this instance it was the economic aim which made available sufficient funds for the pure research. A classic example of this in more recent years is the *Discovery Expedition*, which was started for the study of the life history of the whale and has led to a complete fundamental study of the southern oceans. The main outlines of the hydrology of European seas were worked out, that is the physical features such as temperature, salinity, and water currents. The distribution and seasonal occurrence of the drifting planktonic plants and animals, so necessary as the basis of all food chains in the sea, were described. For the first time full-scale attacks were made on the study of the life histories of fish, to answer the unknown questions regarding population size, breeding, food and growth, migrations and so on—questions which had to be answered before the capacities of fish populations as stock supplies could be assessed. It was only natural that the most important commercial fish received first attention, and the four species, the plaice, the herring, the cod and the haddock, have probably had more careful study in all aspects of their life histories than any other animals.

Two major facts of biological interest have resulted from this intensive study. The fish populations of our northern waters are composed of a number of local self-contained aggregations, like nations; and these populations are liable to periodic large fluctuations in abundance independently one of another. This discovery concerning fluctuations, now known to occur throughout the animal kingdom on land as well as in the sea, has had a very strong influence on the direction of marine biological and oceanographical research. It is fundamental in nature and the clues to its cause can only be sought by fundamental enquiry. Thus, out of knowledge obtained for applied ends new branches of pure science have developed. It can be said that without question the major efforts of the many marine laboratories now established throughout the world are directed towards the fundamental problem of the productivity of the sea and its changes from time to time. In this research there is a great deal of stress on the study of sea water itself; for it is the sea water which is the medium in which the animals live and ultimately draw the salts and substances on which their lives depend, and it is the conditions in the sea water which constitute their environment. So from fish we are led back to the chemistry and physics of sea water.

Sea water contains, as well as those substances which are the major constituents of sea salt, a number of substances in very small

concentrations which are necessary for plant life. These are, as might be expected, much the same as those necessary for plant life on land, of which the most conspicuous are compounds of phosphorus, nitrogen and iron. But their low concentration in the sea renders them limiting factors for plant life and also makes their quantitative analysis very difficult. Consequently, much time has been spent on the development of methods of research for following the seasonal cycles in the abundance of these nutrient salts from place to place. A generalised picture has now been produced, showing that in our northern waters the nutrient salts are at a maximum during the winter months. They are rapidly removed from the water in the early spring and, after remaining at exceedingly low concentration during the summer months, are slowly regenerated in the late autumn and early winter to their maximum again.

Sea water contains also at certain times of the year enormous numbers of microscopic single-celled plants, which drift in the water at different levels and form the bulk of the vegetable food upon which many microscopic drifting animals depend. This is the end of the chain of living matter which leads through successively larger animals to the fish themselves.

Seasonal observations on the abundance of these minute plants show that their numbers are directly related to the amount of nutrient salts, and that the great reduction in concentration of phosphates and nitrates which occurs in the spring is due to rapidly increasing numbers of the microscopic plants. It is the spring time in the sea as on land, and in the sea also the onset of spring is caused by the increasing strength of the sun's rays. The dependence of plants upon light has required new lines of research in the sea to measure the penetration of the light into the water and the extinction of the different colours with depth. It has posed many problems of purely academic interest, but it has its economic possibilities also. It is, for instance, well known that fishermen recognise poor or productive areas by the colour of the water. It has also been found that the drifting plankton animals, on which herring and mackerel feed, keep different levels in the water according to the strength of light, and the fish themselves may also move under the same influence, following the fading light to the surface at dusk, at which time they are caught in the drift nets.

It is interesting also to record that, during the war, when it was urgently necessary to know accurately the depths of water off enemy beaches, the knowledge of light conditions in the sea gained before the war proved valuable. It certainly enabled a method to be developed more quickly in which, by comparing photographs

taken vertically from the air through green and red filters, the depth of the water could be estimated with considerable accuracy. This method, evolved for warlike purposes, has also its peacetime uses, such as for charting sand banks in harbours and navigable channels, the rapid survey of uncharted coasts, and the surveying of underwater beds of seaweed.

The attack on the chemistry and physics of sea water continues, and a recent discovery is the importance of trace elements, such as manganese, the absence of which can render otherwise nutrient rich waters infertile for particular kinds of plants.

As in meteorology, so in the science of the sea, continuity of observations is a prime necessity. Once methods for determination of phosphorus in the sea were established, regular observations have been made from year to year throughout the seasons. Such observations made in the English Channel showed a recurring sequence of events, with minor variations, each of which has been the subject of enquiry. But in the early 1930's a change of a major character was observed. Until then each winter the phosphorus returned approximately to the same maximum strength, but thereafter until the present day the winter concentration has been consistently lower. Concurrent observations on the abundance of planktonic animals and young stages of fish showed a simultaneous falling off in abundance in marine life. The waters were less fertile.

It has now become a major problem to trace the causes of these long-term changes in fertility of water masses. It was at first thought that the clue might be given by a study of the normal hydrological features in the area, such as salinity and temperature. This, however, failed to supply an answer. But, as so often happens, a most useful tool has been supplied of quite another character. It has long been known that the species of animals and plants composing the drifting plankton community differ from region to region. It was a very early hope that these organisms could be regarded like drift bottles, liberated to indicate water movements, and that by their study the movements of water masses could be followed. The hope was premature because insufficient was known about the distributions of the different species and their habits and life histories. For many years biologists have studied these organisms for their own sakes, and fortunately now our knowledge is sufficiently advanced to enable the original wish to be fulfilled. It has been discovered that certain species prevalent during the period when fertile water filled the English Channel are no longer present. Search for these species has revealed their presence in rich water lying to the west of the Channel mouth and south of

Ireland. It is reasonable to suppose that this water draws its fertility from the upwelling of deep ocean water on the Continental Slope rich in nutrient salts. Other water lying to the south, and again distinguishable by its plankton content, is much less fertile, and it is possibly this which is now filling the Channel, drawn from impoverished surface ocean water. The continental edge now becomes our immediate field of enquiry and it seems likely that once more we must revert to our echo-sounding tools. We wish to know exactly where and why water upwells from the deep ocean, and to do this it is necessary to have an exact knowledge of the contours and configuration of the Continental Slope.

We also need to know the oceanic conditions far from the area of study, for the changing movements of water round our coasts are reflections of alterations in the oceanic circulation away down at the source of the Gulf Stream in the Florida Straits and far north in the arctic polar seas. During the past thirty years there have been widespread climatic changes in the north. The winter temperatures have risen and unusually warm ocean waters have been penetrating farther north, thus emphasising the interrelation between oceanography and meteorology. As a result, the geographical distribution of some species of fish, whose limits are normally set by temperature conditions, have been greatly expanded northwards. Herring and cod have penetrated far up the Greenland coast, giving rise to new fisheries, while the well-known cod fishery of Bear Island sprang into importance at the same time. This, of course, is a regional fluctuation of abundance of fish in an area the course of which is quite unrelated to changes in fertility. But the two are no doubt interrelated, since it is the changes in general oceanic circulation which have probably caused the alteration in distribution of fertile water masses off our south-western shores.

There are many other factors to which periodic fluctuations in abundance of marine animals can be attributed, but I have said enough to indicate how this one central problem requires the study of the sea as a whole for its solution, and how the manifold details supplied by both pure and applied research are needed to build the picture. And in the building of the picture many will be the by-products. The liver oils of many fish are rich in certain vitamins and have great medicinal value. The origins of the vitamins are, therefore, a matter of interest. This has led to analyses of the microscopic animals and plants on which the fish ultimately depend for food. Information has thus become available on the pigments, such as carotins, which are the precursors of certain

vitamins. Research on the productivity of the sea has also necessitated accurate assessments of the total crop of plankton plants and animals present in any area.

Such information has proved valuable to investigations of quite another character. Examination of cores of bottom deposits, drawn from the sea floor, made by workers with geological interests, has revealed the presence of carotenoid pigments in the sediment. Indeed, these substances have been found at levels in the core which, from knowledge of the rate of sedimentation, must be several thousand years old. The quantities present can now be compared with the amount known to occur in plankton organisms from estimates of crop. At the same time, most other marine animals are pigmented. These pigments have been studied for a variety of reasons. The origin of pigments in deep-sea deposits is of special interest in the study of the origin of petroleum oil bearing deposits, much of which is now generally regarded as being marine in origin. In this connection studies of the bacterial content of marine muds are of great importance, since bacteria are known to be capable of producing those substances necessary for the ultimate formation of petroleum. The pigments are precursors of these substances. In passing, it is worth recalling the great fascination that those microscopic skeleton-building protozoa, the foraminifera, had for the early microscopists. The skeletons were a delight to the eye, owing to the variety and symmetry of their form, and much pleasure must have been derived by the systematists in their description and classification. This early work has proved of the greatest value to petroleum engineers, for the different strata are distinguishable by their contained fossil foraminifera. Foraminifera have also now been used as indicators of past climatic conditions from their occurrence in cores of deep ocean deposits.

I have recently referred to the presence of pigment in marine animals. The study of the distribution of this pigment is absorbingly interesting. It is well known that many marine animals exhibit, as do land animals, a variety of methods of camouflage. Some decorate their bodies with sand, or stones and shell, or seaweeds, to match their surrounding environment. Others have fixed patterns and colours which conceal them when in their normal habitats. Still others have remarkable powers of changing their colours and patterns, chameleon-like, to suit different backgrounds. In most animals the pigments, usually black, red and yellow, are contained in small, much-branched cells scattered over the surface of the body. Within these cells the pigment granules may be dispersed throughout the branches or concentrated in a point in the

centre. It is the various degrees of dispersion or concentration of the pigments in the many cells which give the whole animal its coloured pattern. It has been known for long that some of the most potent factors in causing these colour changes are the strength and direction of the light and the nature of the background. The colour changes in fishes and crustaceans is the resultant of two responses to these factors, one mediated through the skin, the other through the eyes. The first seems to be a nervous affair in fishes; as for the second, a study of the time relations has suggested that it is normally co-ordinated by one or more hormones, or chemical messengers, produced in the pituitary gland. It is, however, interesting from the evolutionary point of view that in some fishes the response has been brought more or less under nervous control, thereby gaining in speed and efficiency. These researches have been of great importance in the development of endocrinology. And even the lowly crabs and prawns have their contribution to make. In them hormonal mechanisms have been demonstrated very similar to those of vertebrates, and some of them also display curious diurnal rhythms of colour change which occur independently of daylight. The nature of rhythms such as these is an unsolved problem of physiology.

These thoughts seem to be taking me far away from the science of the sea, but I wish to take this opportunity of showing the value of research on the physiology of marine animals, which after all can justly be included in the science of the sea, for they are the marine inhabitants. A knowledge of their physiology is quite necessary in the study of their habits and especially in such problems as the migrations of fish. But these academic researches on the physiology of marine animals can really be linked with another field of applied science, for the results of the research have definite medical implications. For many fundamental physiological problems marine animals form ideal subjects, since they are easily obtainable and often simple in structure. It is not surprising, therefore, that the marine laboratories have afforded great opportunities for physiological research. Problems of development and fertilisation have been made clearer by the study of developing sea-urchin eggs. Quite considerable advances in knowledge have been made by researches on the heat formation in crustacean nerves, which have also recently been found specifically useful for studying the action of insecticides. Crabs supply blue blood of a nature most suitable for demonstration of the reactions of respiratory pigments.

But perhaps marine animals are most valuable owing to their great variety. Out of the hundreds of different kinds there is

almost always to be found one which has some unusual characteristic not to be found anywhere else in the animal kingdom. When speaking of colour change, no animals show more remarkable powers than squids and cuttlefish. While the reactions of colour change in other animals are somewhat slow, taking minutes or even hours to be effective, in these animals the changes are instantaneous. Their mechanism of colour change is quite different. Instead of having pigment in unmoving branched cells, the colour cells themselves expand and contract. They are, in fact, little bags with muscle fibres radiating from their circumferences. This arrangement has made possible their control from the central nervous system far more efficient than the improvements we have noted in vertebrates. In almost all respects the squids and cuttlefish are most interesting animals. In them, and they belong to the mollusc family as do oysters, the eyes are most highly evolved and, curiously enough, almost on the same pattern as that of the human eye. The squid is also a powerful and efficient swimmer, progressing through the water by a method of jet propulsion. By sudden contractions of the body, water is forcibly ejected from a syphon under the head. This contraction is exceedingly rapid, and, in order that the muscular contraction may be almost simultaneous throughout the whole body, the squid, and its relative the cuttlefish, possess giant nerve fibres of dimensions rivalled in the whole animal kingdom only by those of one species of marine worm. Whereas vertebrate nerve fibres rarely exceed one-fiftieth of a millimetre in diameter, the giant fibres of the cuttlefish may be more than half a millimetre across. What a find for the nerve physiologist! The fundamental study of the electric phenomena which take place within and at the surface of a nerve fibre has been impeded by the difficulties of working on structures of such minute dimensions. Now we have fibres available twenty-five times larger, so large, in fact, that electrodes can most satisfactorily be placed within the fibre itself, and so large that the contents of the fibre itself can be extracted and subjected to examination.

It is this selfsame squid family which has given rise to many fabulous tales of sea serpents. In the deep waters of the ocean they grow to great dimensions; specimens have been washed ashore whose overall dimensions to the tips of their extended arms were upwards of 60 feet in length. On the arms are powerful suckers. Squid form the chief food for sperm whales. Many are the battles beneath the sea surface between these two leviathans of the deep, and the suckers of the squid leave their scars on the whale's flesh. Measurements of these scars indicate that squid even

larger than those few specimens washed ashore must exist. What else lives in the ocean abyss? It is only nine years since that most remarkable occurrence in the annals of biology, when the fossil fish *Latimeria*, thought to have been extinct for 60 million years, was caught alive in the Indian Ocean. Of all the problems in the sea, both pure and applied, I personally think those of such academic nature as this hold the greatest interest.

In this brief and all too sketchy outline of the balance and interaction between pure and applied researches on the sea, I may have appeared to favour, if anything, the latter. This is perhaps because I happen to be the Director of a laboratory the chief object of which is to further fundamental researches. It would not have been fair for me to deny applied research its due. But I hope that I have at any rate shown that each is necessary. In the words of Louis Pasteur: "In the field of science chance only favours the mind that is prepared." So applied science is favoured by the ground that has been prepared beforehand by pure research. Without this vast background, which we owe to those whose lives have been devoted to the quest for knowledge for its own sake, applied science would never have reached its present success—a background in its vastness and detail out of all proportion to applied knowledge. And it will never survive the future unless the pure knowledge on which its foundations are built continues to grow of its own free will.

WAVE MECHANICS IN PHYSICS, CHEMISTRY AND BIOLOGY *

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INTRODUCTION

EVERY Inaugural Lecture is an embarrassment—both to him whose job it is to state his ground, and to those who come wondering what kind of fish their net has gathered. In my own case it is made no easier by the fact that not only am I new, but so also is the Chair itself. In one sense we inaugurate the Chair of Theoretical Physics as well as the Professor on this occasion. Both of us, if I may so speak, are new, both unknown. How otherwise should I describe the receipt of my first bill, after arriving at the College? It was for a calculating machine, and it was made out to the “Theatrical Physics Department.”

My lecture is to be concerned with wave mechanics. This is one part of the greater subject of quantum theory, but I have chosen the smaller rather than the larger for two reasons; first, that it is the one branch of theoretical physics which has shown itself most fruitful in explaining, correlating and developing our knowledge both of physics and of chemistry in the last few years; and, second, that it has now come of age. For it was just over twenty-one years ago, in 1926, that Erwin Schrödinger issued in the *Annalen der Physik* his first account of the subject. Twenty-one years is long enough for a new subject, as for a new human being, to reach maturity. It is appropriate therefore that we should pass in quick review some of the things that wave mechanics has shown us; and then, if as a physicist I may be allowed a little crystal-gazing, I should like to hazard a guess at what we may expect in the next stage of its growth.

* Inaugural lecture at King's College, London, March 2, 1948.

ITS BEGINNING

Its beginning was fairly simple. "In this communication," wrote Schrödinger, "I wish to show, first for the simplest case of the non-relativistic and unperturbed hydrogen atom, that the usual rules of quantisation can be replaced by another postulate. . . ."

This was, of course, the idea, first enunciated by de Broglie, that a particle of momentum p had waves somehow associated with it, and their wavelength was simply h/p , where h was a universal constant, Planck's constant. The idea led to a wave equation to describe the waves. This is the Schrödinger wave equation which promptly replaced the good old Newtonian equations of motion. It is not for me to describe the equation; but rather to pass over all this and come back to Schrödinger's original paper. "The new conception can be generalised, and I believe it penetrates deeply into the true nature of the Quantum Rules."

Well, of course it was generalised; and with great success. It showed, with small additions to allow for relativistic effects, how the energy levels, and hence the spectrum, of atomic hydrogen could be predicted with astonishing accuracy, at least as accurately as the individual constants that appeared in its formulæ were known. It gave selection rules, so that we knew which electronic transitions were allowed; it calculated their intensities; it dealt with the effects of a hydrogen atom in a steady electric or magnetic field, effects generally described under the heading of electric and magnetic susceptibilities; it dealt with the interaction of varying fields and the atom, providing correct dispersion formulæ. For molecules, it explained the allowed rotation and vibration levels, and accounted easily and nicely for their selection rules, and their various degeneracies.

The whole world of physics seemed to be falling into its lap. No wonder that Dirac, in the Bakerian lecture of 1929, which followed the derivation of his relativistic form of the wave equation, could say: "The general theory of quantum mechanics is now almost complete, the imperfections that remain being in connection with the exact fitting in of theory with relativistic ideas. These . . . are of no importance in the consideration of atomic and molecular structure, and ordinary chemical reactions. . . . The underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known. . . ."

THE FULL-STOP

But after that we came to a kind of full-stop, as if we had over-reached ourselves. For hitherto we had dealt entirely with one-electron problems or problems which could be separated into parts, each involving one independent variable. The simplest two-electron problem was that of atomic helium, and here, although Heisenberg could give a qualitative account of ortho- and para-helium, and relate it to the symmetry of the wave function, it was only after several more years that a really satisfactory *a priori* calculation of the energy was obtained.

The simplest molecular problem was H_2 : and again, although it was as early as 1927 that Heitler and London attempted to solve this problem using the new form of perturbation theory introduced by Schrödinger, it became apparent that if one went to a second degree of approximation the previous agreement was not retained, for the second term was even larger than the first.

But worse was to follow very soon.

The simplest three-electron problem of chemical interest was the interaction of three hydrogen atoms, in the reaction



The best calculations to date are about 100 per cent. wrong in their estimate of the energy necessary before this reaction will take place.

We can see some of our difficulties in this way: consider the methane molecule CH_4 ; if we keep the nuclei fixed, and discuss the motion of the ten electrons in this molecule, we shall have to write down and solve a partial differential equation of 30 variables. It is easy enough to write it down; it is a vastly different matter to solve it, even if one uses all the symmetry that is available. In larger molecules, like benzene, the situation is obviously worse; quite evidently no completely accurate solutions of the wave equation will ever be made for such systems.

This is true in other ways; the simplest problem associated with a metal surface is the interaction between an isolated electric charge outside it and the body of the metal itself; classically this is represented by the interaction between the original charge and its image in the metal. But quantum-mechanically, this problem has never been solved! And that means that no one has ever been able to calculate *ab initio* any of the electrical properties of solids sufficiently precisely to dispense with the need for measurement.

In a situation like this, do you wonder that, when the British Council wrote last term asking for particulars of the work that we

PLATE I

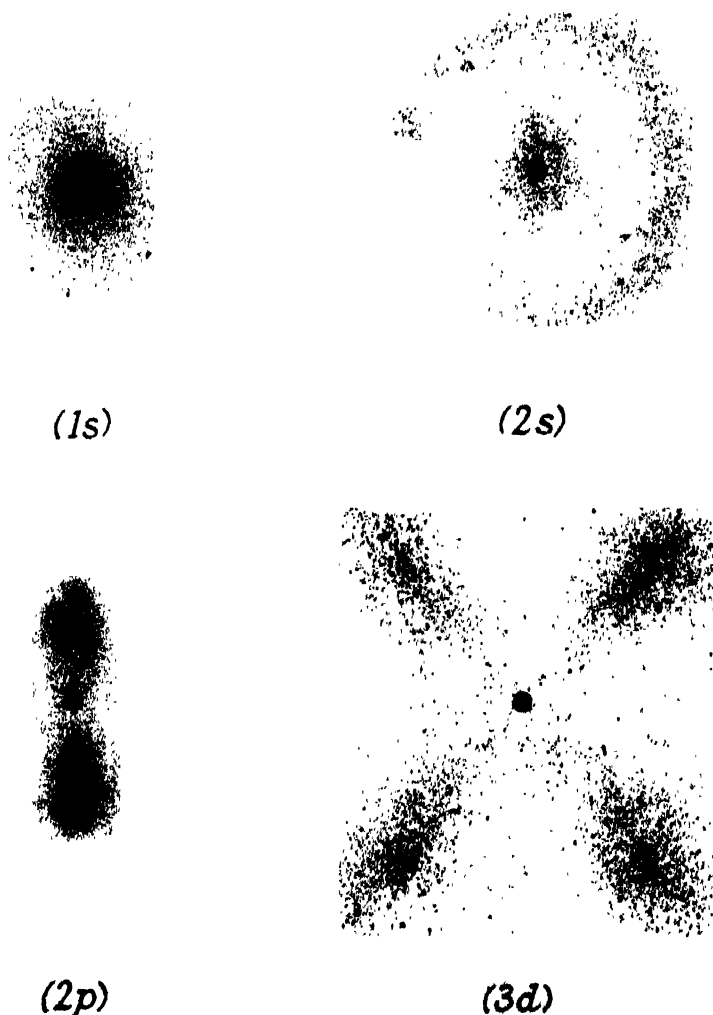


FIG. 1 - Density patterns for an electron in a Hydrogen atom. Each pattern represents a possible "orbit," in which the probability of finding the electron at any point is greatest where the pattern is most dense. Underneath are shown the spectroscopic names for each orbit. The central dot in each case denotes the position of the nucleus.

do in the College here, they sent their request to the department responsible for "waste mechanics"?

If I judge correctly, it was at this stage that a certain despondency fell on the subject; for men realised that there were very severe limits to the complete calculation of almost any single physical or chemical property. I believe that this despondency arose from a wrong conception of the true function of wave mechanics. In many people's minds, the initial successes made them expect that everything would be completely calculated; and when this was found to be clearly impossible, they felt that they had been let down. This was well epitomised in a letter I received some time ago from a fellow-conspirator who had found, as everyone who has tried the game knows only too well, that it was one matter to start calculating the colour of a compound (that is, the characteristic absorption of light which it shows), but it was quite another matter to get the right answer. "I came here," he said, "believing that wave mechanics was the key to all the future. Now I am disappointed and disillusioned."

I believe he was wrong; and that wave mechanics is the key—or perhaps *a* key—to the future of physics, chemistry and biology. However, when we open doors with it we do not find completely worked out, neat solutions to all our problems, but new ideas, new principles, new understanding of processes the details of which we shall probably never calculate to "so many" significant figures, and new relations between apparently unrelated properties. It is this new understanding which is the chief contribution of wave mechanics.

EXAMPLES OF THE NEW IDEAS

Let me show you what I mean in terms of a few particular examples; and perhaps, for this purpose, I may be allowed to choose illustrations from those fields where I have been more especially concerned myself.

1.—ATOMIC AND MOLECULAR STRUCTURE

First let us consider the changes that have come to our knowledge of the orbits of electrons in atoms and molecules—atoms first. In the early quantum theory of Bohr, the electrons moved round the nucleus in certain orbits similar to planetary orbits round the sun; but the distribution of these orbits in three dimensions, when one would have expected only two dimensions (motion in a plane), and the various ways in which the orbits precessed round a certain

polar axis were very obscure ; though not so obscure as the reason why only certain orbits were possible.

Now consider Fig. 1 (Plate I). It shows the new pictures of atomic orbits. No longer do we try the impossible job of following an individual electron in its orbit, but we determine the chance, or probability, of finding the electron in any region. In these diagrams, which show possible orbits, the probability is greatest where the diagram is most dense. Sometimes we do not realise what a step forward this was in our understanding. Gone are the two-dimensional orbits, gone are the Wilson-Sommerfeld-Bohr phase-integrals to provide quantum conditions ; these patterns, for all their apparent complexity, are found by simple solution of a differential equation such as any second-year mathematician would take almost in his stride. Now we can *feel* the orbits. What does it matter if, in lithium, for example, we do not know on just how large a scale we should draw these orbits, or just how much individual orbits interfere with each other through mutual electron repulsion ? If we know that Li is $(1s)^2(2s)$, i.e. has two electrons in a $1s$ orbit and one in a $2s$, then we know its character at a deeper level than before.

Or let us go to molecules. Fig. 2 shows the contours of equal probability for molecular hydrogen. To be precise, this is not entirely true : these contours are the best we can do by way of calculation ; for it may be shown that the wave equation is not now soluble in ordinary closed form as it was for the atom of hydrogen. But, although they are not quite right, they are nearly right ; and how much they tell us about the bond between the two nuclei ! How far we have progressed from the old Bohr picture of two electrons in a bond chasing one another round some sort of circle in the plane midway between the nuclei. How far we have progressed in understanding the original idea of G. N. Lewis in his famous 1916 claim that two electrons formed a bond, and they were shared between the nuclei. This sharing has become quite real ; I almost think we can *feel* it.

I ought to go on and talk about the way in which stereochemistry has come to be explained, but I will only mention one point in it. If we know that two electrons form a bond usually localised in the way shown in Fig. 2 ; and if we know that the oxygen atom has two valence electrons in orbits the patterns of which point rather strongly in two directions at right-angles to each other, then we have a simple explanation of the fact that in water the angle HOH is nearly 90° . What I want to stress is the naturalness of it, almost the inevitability of it. And I want you to contrast it, if you

remember these things, with the attempts made in the 1920's by people like Hund, at Leipzig, to explain the structure of water molecules by pre-wave mechanical methods. It could not be done; and now every chemistry undergraduate in his third year knows all about it.

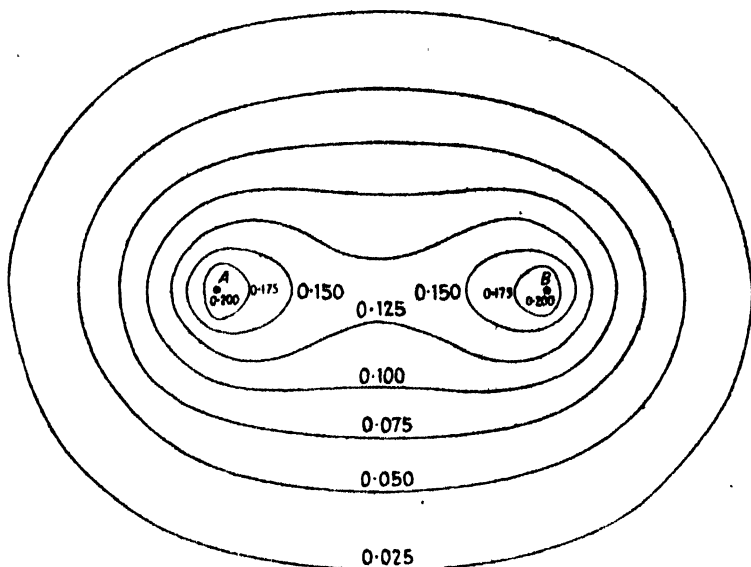


FIG. 2.—Density patterns for the electrons in the hydrogen molecule. Two electrons, with "orbits" similar to this, and with their spins anti-parallel, form the bond. These contours show that the electrons of a bond are concentrated quite closely in the region between the nuclei, and do not usually "spill over" much into the region occupied by adjacent bonds.

Or should I speak of the structure of benzene, that *cause célèbre* of organic chemistry? Do you remember how Kekulé, effectively using the idea of two electrons for a single bond and four for a double bond, supposed that the molecule could be drawn (omitting the hydrogen atoms for convenience) in either of the two patterns of Fig. 3; and how there was some sort of dynamic oscillation

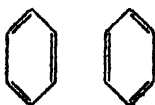


FIG. 3.—Kekulé's structures for benzene (omitting the hydrogen atoms for convenience). Kekulé supposed that there was dynamic oscillation between the two possible ways of pairing the electrons to form bonds. Modern wave mechanics says that the molecule is represented by a wave function ψ , which may be regarded as composed linearly out of separate wave functions, each representing a possible structure. For benzene there are altogether 5 covalent structures and many more ionic ones.

between the two? How much more naturally we put it now! Each of Kekulé's structures corresponds to a certain wave function; we say that the true wave function is a combination of these separate ones, which are rather like components; so the molecule has simultaneously characteristics of both structures (and of others that I need not mention). At the same time it is more than the sum of the two; somewhat in the same way that the colour brown may be obtained by combining red and green, but is yet more than the two separately.

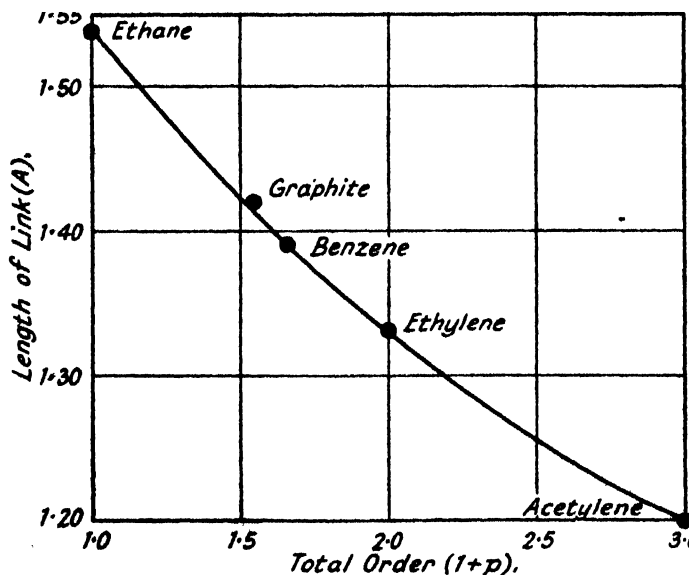


FIG. 4.—The curve connecting bond length and bond order for C—C bonds. If we can calculate the bond order, which is usually fractional, this curve allows us to predict the C—C length. Thus, in benzene the bond order is 1.67, so that the C—C length is 1.39 Å., in agreement with experiment. (The calculations of bond order for this curve are to be made using the so-called "method of molecular orbitals." Similar results are found by other methods.)

What I have been describing in this particular case is the general phenomenon of "resonance," the introduction of which has done so much to clarify our ideas of structural chemistry.

May I mention one single development, from many? As a result of the superposition of the structures in benzene, the bonds are all equal, and are neither single nor double, but have a bond order that is fractional, between one and two. This may be generalised, and we are able to calculate the fractional order of bonds in all sorts of molecules. If we can draw, as in Fig. 4, a

basic curve relating the order and the length (for single and double bonds have different characteristic lengths) we have a means of predicting the lengths of all sorts of bonds in all sorts of molecules. This idea of fractional bond order is proving very valuable; and its application in this way to the calculation of interatomic distances has provided a most important link with some of the recent very accurate X-ray work of the experimentalists.

What does it matter if our calculations of the energy of resonance are not quite correct, or if there are other factors which play some part in the whole scheme of molecular structure? Even as things are we can make pretty good predictions, but, which is more important, we know what is really happening, and what is the effective agent in determining bond length and bond shape.

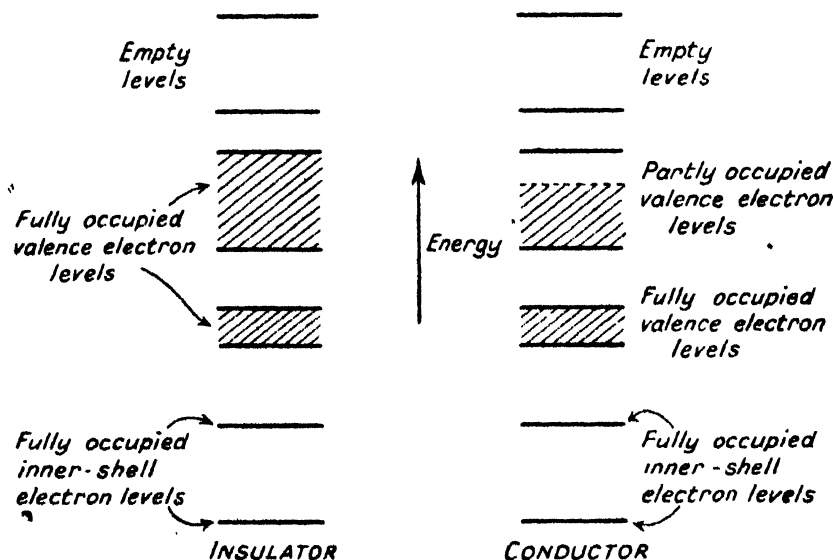


FIG. 5.—Energies of electrons in a metal. Wave mechanics tells us that an electron cannot have arbitrary energy, but only a value lying within certain bands. For the valence or conduction electrons, these bands may be quite wide. If all the energy levels in certain bands are filled by electrons, and there are no bands partly filled, the substance is an insulator. Otherwise it is a conductor, although it may be a good one or a bad one.

2.—ELECTRONS IN METALS

Now let us pass to another field; and I choose this time a purely physical one—the energies of electrons in metals. I believe it is quite doubtful whether, without the help of wave mechanics, we should ever have had any clear idea of electric behaviour in metals. Fig. 5 is a rather simplified version of some of our knowledge.

Electrons in metals cannot have arbitrary values of their energy, but these values must lie within certain bands; for the valence or conduction electrons, these bands may be quite wide, as the diagram shows; but even then there is a limit to the number of electrons that any single band can accommodate; if there are too many electrons available, then they must go to another, higher band; if there are too few, they will only partly fill the band. But here is the interesting point—if a band is not filled, but is partly empty, then an applied electric field can make the electrons flow in one direction preferentially, and we have conduction; the metal is a conductor; but if the band is completely full, then only

very high fields can make any electrons flow preferentially in one direction, and we have an insulator.

How simple! But it is doubtful if we should ever have understood it without the aid of wave mechanics. From this simple description there follows a study of photo-conductivity, fluorescence and a heap of other related effects. But remember that no one has ever completely calculated the band distribution in any single metal! Indeed, what does it really matter about these details, if only we know approximately how the bands should be drawn, and to what extent they are filled?

There is an interesting application in a graphite layer. Fig. 6 shows how the carbon atoms are arranged in a regular hexagonal array like a beehive; each

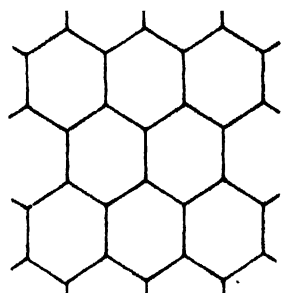


FIG. 6.—Part of a layer of carbon atoms in graphite. The atoms are arranged at the corners of a regular hexagonal lattice, but individual lattice planes are quite far apart from one another. Thus the C—C distance in a plane is 1.42 Å., but the distance between planes is about 3.35 Å.

individual plane of atoms is some long distance from its neighbours on either side. It is possible to make rough calculations of the bands for a graphite crystal. Fig. 7 shows the function $N(E)$ for the topmost band of valence electrons, where $N(E) dE$ is the number of energy states in the energy range from E to $E + dE$. At the absolute zero of temperature, the complete band is filled up to the mid-point; and we should have practically an insulator. But, as the temperature rises, some of the electrons have enough "temperature-energy" to carry them into the upper half of the band, and we have a partial conductivity. This conductivity should increase with the temperature, as indeed it does; it should also be much greater along the basal planes

of the crystal than across them, again in agreement with experiment.

May we not say that we understand the graphite crystal now ?

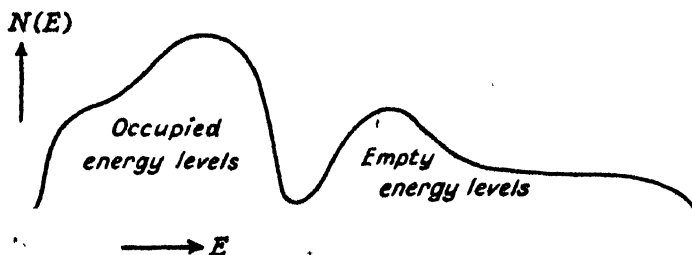


FIG. 7.—Allowed energy bands (approximate) for the conduction electrons in graphite. At the absolute zero of temperature, this band is exactly half-filled, but on account of the unusual, and almost distinct, character of the "half-band," the crystal is nearly an insulator. At higher temperatures, a certain number of electrons get into the upper "half-band," and increased conductivity results.

3.—COLOUR

I shall only briefly refer to the theory of colour ; why are some substances one colour, others a different colour, and some apparently colourless ? Evidently it is a matter of the wavelength of the light that they will characteristically absorb. If we can calculate this, we can predict the colour. Fig. 8 shows an interesting series of molecules, somewhat like a string of sausages of increasing length (as n increases) ; and these molecules, for values of n up to 6,



U.V. absorption of para-polyphenyls

n	2	3	4	5	6	7	∞
λ calc. (Å.)	2280	2710	[3000]	3200	3350	3440	3690
λ obs. (Å.)	2515	2800	3000	3100	3175	—	—

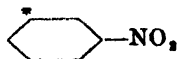
FIG. 8.—Characteristic absorption of light by the polyphenyl series of molecules. The numbers show how the wavelength of maximum absorption increases as the size of the molecule increases. The value of a certain constant is chosen to give agreement between theory and experiment for the case shown in brackets. But no other empirical data are used.

have been discussed experimentally by Gillam and Hey. They are the so-called polyphenyls. Now, calculations can be made of the energy levels of the electrons in this type of molecule ; they are rather difficult calculations, and in the end we find ourselves with a certain parameter the absolute value of which there is no reasonable hope of calculating *a priori*. The sceptical would say that we had failed to understand the characteristic shift of the colour to more and more red, i.e. longer wavelength with increasing n . But would he be right ? The numbers in Fig. 8 show that if we

decide, quite arbitrarily, to choose the value of this parameter so that we get the right answer for one of the series (the case $n = 4$ shown in brackets), then we get tolerably good answers for the others. The agreement is not perfect—a situation resulting from a variety of causes that I do not propose to discuss now. What seems to me to be important is that these calculations show that the model we were using was essentially the right one, and that, although I have not described it in detail, we can honestly feel that we understand what makes the colour go more and more to the red. This is vastly more significant than whether or not we can calculate the absorption correctly to the nearest Ångström, or even hundred Ångströms.

4.—CHEMISTRY

And now, having drifted imperceptibly from physics into chemistry, let me say something about the light which wave mechanics has thrown upon certain chemical reactions. When an unsaturated molecule such as nitrobenzene



is undergoing a chemical reaction, in which, let us say, a positively charged radical is approaching (perhaps another NO_2^+ group), it is fairly evident that, other things being equal, the reagent will approach most easily that part of the original molecule where the largest negative charge is found, and least easily to that part where there is a net positive charge. But the charge distribution in nitrobenzene can be calculated by wave mechanics, and in this way we can predict that the second nitro group goes, as in practice it almost entirely does go, to one of the meta positions which are marked with an asterisk. Now the important thing about all this is not that it saves the chemist from making his accurate experiments (for in fact there are many other factors involved, of which I have not spoken at all), but rather that we have an understanding, at the deepest possible level, of *why* these reactions take place, and what is actually going on at the time.

Phenanthridine.—This could be generalised to other molecules where the experiments are not so easy. For instance, the parent member of a large series of drugs is phenanthridine; but all of the



substitution reactions of this system are not yet worked out

experimentally. However, the charge distribution has been calculated, and these calculations are proving a useful guide to the experimenter.

Drugs.—This reference to drugs leads to another one; for in wave mechanics there is one of the keys to the future of biology. Dr. Taylor of King's College has recently shown that there are certain drugs, the curareiform family, which are effective anæsthetics in preventing the contraction of a muscle; and in them it appears that the electric charge on, or near, a certain nitrogen atom is significant. Now, we can alter the charge on the N atom by various substitutions; and the magnitude of the charge shift can be estimated. It is found that when this shift exceeds a critical value, the drug is effective; if it is below it, it is ineffective. Here surely wave mechanics is throwing new light on previously rather mysterious phenomena.

5.—LIFE

These large molecules lead me immediately to that larger problem of the nature of life itself. The question of just what constitutes life is a tremendously big one; in a sense it represents the ultimate goal of almost all our efforts. Quite clearly its solution must demand, and indeed is now receiving, the combined attention of physicists, chemists and biologists. We can put it this way. The largest molecule that is reasonably well understood contains some 30–50 atoms; if it were rectangular in shape, it would be represented by a little cube about 3 or 4 atoms each way. But the smallest biological system is by comparison much bigger. The egg albumin molecule contains some 10,000 atoms, and that would be represented by a cube some 20 atoms each way. The virus particle of foot-and-mouth disease would be only a little bigger. Between the small and the large cube there lies life—is the difference too big for us to jump? In some respects I think not. And at any rate there are several ways in which wave mechanics can contribute to making the jump more possible. For example, we can calculate the nature of the forces between units of this kind, forces which, although they have their origin in the electrons of the individual atoms, are yet more than summations of individual units. Or, again, we can discuss the propagation of electric influences along a molecule of this sort, or from one to another, as in the case of a series of protein chains, and thereby throw some light on the mechanism of sensation. Thirdly, it can help us to understand the relation between the geometry and other properties of the molecules themselves, linking together size, and shape, and energy.

Work is proceeding along all these lines : to show the sort of thing that emerges, here is a single example, though perhaps an important one. For some years it has been known that certain hydrocarbon

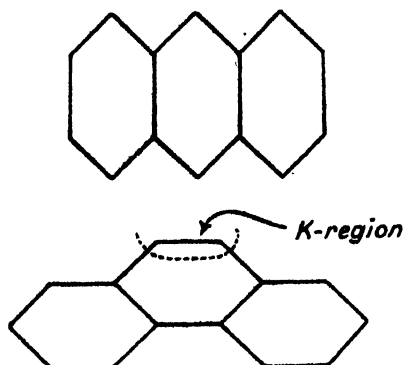


FIG. 9.—The K-region of certain molecules. It is found experimentally that molecules with a K-region are often able to induce tumours if the "electrical index" of this region exceeds a critical value. The upper molecule is anthracene, with no K-region; the lower molecule is phenanthrene, with its K-region marked.

molecules, such as benzanthracene, if injected into animals, are capable of producing tumours. The power to do this seems to be associated with the existence of a certain region, now called the K-region; if a molecule is of such shape as to have a K-region (Fig. 9), then it possesses a possible carcinogenic activity; otherwise not. The properties of the electrons in such regions may be summarised in what, for convenience, I will call the "electrical index." It appears that there is a certain threshold value, to be determined purely by calculation; and, almost without exception, if the index exceeds this

threshold, the molecule is found to be carcinogenic. Some tens of molecules have been studied, and predictions made; nearly always these are later verified experimentally.

Of course this does not solve the problem of cancer, nor of life. But it is a big step in the right direction.

CONCLUSION

If I have made myself clear, you will by now have discovered what I myself feel about all these things. Wave mechanics is not a device whereby a set of calculating machines replaces all laboratory experiment and apparatus. It is never meant to be so. It is meant to throw new light on things—to make us understand them—to correlate different bits of information, as, for example, the heat of formation of a compound and its u.v. spectrum—to show the essential character of what we observe and measure, but not to spare us the need for measuring them. That is why the experimenter need have no fear of the theoretician, for he is not so dark a horse as he is sometimes painted.

And that is why the greatest triumphs of wave mechanics are

likely to be in borderline fields ; for there not only different sides of one subject, but even different subjects, are seen to be one. That is why I look forward to a future full of excitement and the chase ; and of good and bad hypotheses, of disagreements and of disappointments, of hope, and all that makes a really living subject. And always in collegueship with experimenters. For it was Norman Lockyer, some fifty years ago, concluding the story of helium, who wrote : " The more we can study the different branches of science in their relation to each other, the better for the progress of all sciences."

THE GENETICS OF THE CILIATE PROTOZOA

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IN a recent review T. M. Sonneborn [1] summarises modern work on the genetics of the ciliate Protozoa. Since a knowledge of the cytological basis is essential for an understanding of the genetical findings, a summary of work in this field is included. The species on which most genetical work has been carried out is *Paramecium aurelia*, and consequently the greater part of Sonneborn's article is devoted to this species.

I. *PARAMECIUM AURELIA*

(a) *Mating Types*.—*Paramecium aurelia* consists of a number of sexually isolated stocks which will not interbreed: these are called varieties, and seven of them are known. In each variety there are two mating types; individuals of one mating type will not interbreed, but on mixing them with individuals of the other type of their own variety they at once agglutinate into clumps, from which they emerge conjugating in pairs. The mating types are known by Roman numerals, thus variety 1 contains mating types I and II, variety 2 contains mating types III and IV, and so on. Variety 7 is an exception, for here only one mating type, XIII, is known. Sexual isolation of the varieties is not quite complete, for varieties 1, 3, 5 and 7 show a slight degree of inter-variatal crossing. These four varieties are, therefore, more closely allied to each other than any one of them is to any of varieties 2, 4 and 6. The former are, therefore, classed together as group A and the latter as group B.

(b) *Genetical Differences*.—The understanding of mating types allows a genetical analysis of the differences between various stocks to be undertaken. In group A most of the work has been carried out on variety 1. Differences in antigenic properties have been found to show typical mendelian inheritance. The same is

true for a condition in which there is only one mating type instead of the normal two; this condition is caused by a single recessive gene pair, known as *mtI*.

The existence of such genetically determined characters makes possible the elucidation of certain features in conjugation and nuclear reorganisation processes which cannot be resolved by cytological methods. In conjugation W. F. Diller [2] claims that the classical account is incorrect in two particulars: (i) the gamete nuclei are not necessarily sister nuclei; (ii) exchange of nuclei between conjugants does not occur, but the synkaryon in each individual is formed by the fusion of its own two gamete nuclei. The process is, therefore, not cross-fertilisation, but double self-fertilisation or cytogamy. By suitable crosses it is possible to decide between the classical account and Diller's interpretation. If a normal dominant 2-mating type stock (+/+) is crossed with a homozygous recessive single mating type (*mtI/mtI*) the F_1 is found to consist only of the dominant normal phenotype. That this is the heterozygote *mtI*/+ can be shown by back-crossing it to the double recessive *mtI/mtI* when 50 per cent. single mating type and 50 per cent. normal 2-type are obtained. Breeding of the F_1 gives an F_2 in which there is a 3 : 1 ratio of 2-type to single mating type individuals. These results can only be explained if cross-fertilisation does occur, and if the two gamete nuclei of an individual are identical and are therefore sister nuclei. A few exceptional cases, in which after mating the conjugants are not genetically identical, are due to the occasional occurrence of cytogamy. The frequency of cytogamy can be altered by changes in temperature.

The occurrence or otherwise of endomixis has also been investigated genetically. As described by L. L. Woodruff and R. Erdman [3] endomixis is a process in which the macronucleus degenerates, the micronucleus divides a number of times, and finally the normal nuclear constitution is re-established from one of the products of division of the micro-nucleus without the intervention of any reduction division followed by nuclear fusion. Diller does not agree with this account, but claims that the process exactly parallels conjugation, except that only one individual is concerned. Two reduced sister gamete nuclei are formed, and these fuse with each other to give a synkaryon from which the macronucleus and micronuclei are reconstituted. The process is thus a sort of self-fertilisation or autogamy. If Diller is correct then after autogamy a heterozygote will always be converted to a homozygote, since its new nuclei are formed by the fusion of two identical gamete nuclei,

whereas this will not be so after endomixis. Sonneborn finds that in all instances investigated heterozygotes do become homozygous after uniparental nuclear reorganisation. For example, a heterozygous $mtI/+$ stock after such reorganisation is found to consist of 50 per cent. $+/+$ and 50 per cent. mtI/mtI individuals. There is thus clear genetical evidence that autogamy is the normally occurring process, while no such demonstration of the occurrence of endomixis is available.

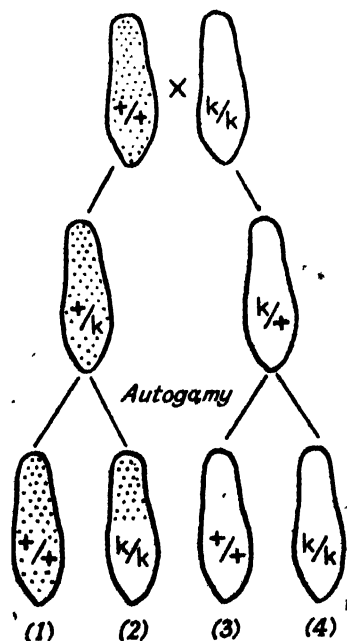


FIG. 1.—*Paramoecium aurelia*: cross of homozygous killer by homozygous non-killer, conjugation being followed by autogamy. k and $+$ indicate the presence of the non-killer and of the dominant killer genes respectively. Stippling indicates the presence of kappa substance in the cytoplasm.

Genetical analysis of group B varieties reveals conditions that are widely different from those that obtain in group A. In varieties 2 and 4 all the characters investigated have been shown to be due not to nuclear gene differences, but to cytoplasmic factors. Inheritance is therefore "maternal," i.e. determined by the cytoplasmic constitution of the conjugant, not by the nuclear genes it receives from its fellow in conjugation. Exceptions to this occur where the union of conjugants is prolonged and exchange of cytoplasm takes place.

The most extensive study has been made of the killer character. Killer animals make their culture fluid poisonous to non-killers, but are themselves unaffected by the poison. At first this character showed straightforward cytoplasmic inheritance. Later work indicated that genes also were involved, and the character has proved to be a most useful tool for the study of the relations between nucleus and

cytoplasm. A stock was found which was homozygous for a sensitive non-killer gene (k), recessive to the killer gene ($+$). The cytoplasmic factor determining the killer character is called kappa. If homozygous non-killer (k/k) is crossed with homozygous killer ($+/+$) the F_1 consists of heterozygous $+/k$ individuals (see Fig. 1). Half of these, since their cytoplasm contains kappa, are killers, and half being devoid of kappa are non-killers. If autogamy

is now allowed to occur, the four types shown in the last row of Fig. 1 result. Of these numbers 3 and 4, devoid of kappa, remain non-killer in spite of the fact that 3 is homozygous for the + gene. Number 1 remains killer, but after a few fissions 2 becomes sensitive. Thus both kappa in the cytoplasm and + in the nucleus are necessary for the maintenance of the killer character. Kappa without + is soon lost, while + alone cannot produce kappa. In other words we are dealing with a process, kappa production, which is gene controlled, but not gene initiated.

The correctness of this conclusion is demonstrated by further observations. If the + gene can be reintroduced into the animals of class 2 (Fig. 1) before more than a very few fissions have occurred, i.e. before all kappa is lost, then once again they give rise to killer clones. Moreover, if cytoplasmic interchange takes place as a result of prolonged conjugation between an individual containing kappa and a +/+ individual lacking kappa then the latter at once produces a killer clone.

The latter type of mating provides other interesting data. Pairs are selected for further breeding in which the union was only a trifle prolonged. In such strains some of the +/+ non-killers receive no kappa and remain non-killer, some receive enough kappa to give rise at once to killer clones, while others receive an intermediate "sub-liminal" amount. This is not sufficient to cause the immediate production of killers, but killer clones are produced when autogamy occurs; and kappa can be transferred from these animals to non-killers when prolonged conjugation takes place. In addition, some individuals may ultimately become killers after a large number of fissions (60-80) without the occurrence of any fertilisation process.

The killer character is also affected by temperature. Exposure to 38.6° C. for 3-3½ days kills variety 4. Exposure of +/+ killers to this temperature for shorter periods has interesting results. An exposure of 36 hours or more causes all to become permanent non-killers. An exposure of between 12 and 36 hours causes some individuals to become permanent non-killers, but others become non-killers only temporarily. These temporary non-killers are capable of becoming killers when autogamy occurs, exactly as with the individuals just discussed which received only a small amount of kappa by cytoplasmic transfer. Clearly the high temperature is destroying the kappa substance, and the permanent non-killer condition is only reached when the exposure is sufficient for complete destruction.

Killers are also known in variety 2, and the character is determined by a cytoplasmic factor. Whether this is or is not under

genic control is not known. In this variety if killers are caused to undergo very rapid fissions they ultimately become non-killers, going through a stage of temporary non-killer which is exactly comparable with the same condition in variety 4. Killers of variety 4 cannot by this technique be converted to non-killers.

Sonneborn's original explanation of all these data depended on a postulated union of kappa with the + gene in the macronucleus. This kappa was supposed to be held in such a way that it became free in the cytoplasm only when the macronucleus disintegrated. Sonneborn has now abandoned this type of explanation in favour of the "concentration hypothesis." According to this hypothesis all the facts given above receive a simple explanation if it is assumed that the kappa substance is not necessarily produced at the same rate as the rest of the organism, i.e. does not necessarily double in amount between successive fissions. If, therefore, a *Paramecium* possesses a sub-liminal quantity of kappa then any process which causes a relative increase in its rate of production will allow more kappa to accumulate and the animals will become killers; while any process which causes kappa production to lag behind fission will result ultimately in complete loss of kappa, and the stock will then have become permanently non-killer. Thus in variety 2 kappa production fails to keep pace with fission when the latter is very rapid. In variety 4, stocks possessing sub-liminal quantities of kappa become killers when autogamy occurs; the reason for this is that autogamy causes a great reduction in fission rate, thus allowing kappa production to "catch up." Catching up may also occur without autogamy after 60 to 80 fissions. Fission rate, in the absence of fertilisation, does not remain constant but gradually falls off with time, so that here too kappa production is increasing relative to fission rate.

Sonneborn sees this concept of differential rates of production of cytoplasmic factors as providing a possible interpretation of the phenomena of Dauermodifikationen. In Dauermodifikationen a change in the organisms is produced by the action of some external agent or alteration in culture conditions. After restoration to normal culture conditions the change persists for a time, but ultimately the stock reverts to normal. The reversion to normal is usually hastened by conjugation and by autogamy. Sonneborn suggests that the original treatment may have had an action on some cytoplasmic factor, comparable with the action of high temperature on the kappa substance, reducing it to a sub-liminal amount. This produces an alteration in the organism, and reversion to normal cannot occur until the cytoplasmic factor has re-established

itself at the normal level of concentration. Since Dauermodifikationen are known in group A stocks this explanation assumes that there, as well as in group B, cytoplasmic determiners exist.

Every hereditary character known in group B has been shown to be due to a cytoplasmic factor; but only for the killer has gene control of this factor been demonstrated. This demonstration was possible only because of the discovery of a stock possessing the mutant gene for non-killer. Other cytoplasmic factors may be controlled by genes, but, unless or until suitable mutants are found, such control cannot be demonstrated. If the killer situation is general, then, in group B, phenotype is normally determined by plasmagenes, while nuclear genes control the multiplication of the plasmagenes. Various hypotheses concerning the relationships between nuclear gene and plasmagene have been put forward. H. J. Muller [4] suggests that the + gene produces a substance π , a precursor of kappa, and in the cytoplasm π and kappa interact to produce more kappa, much as pepsinogen and pepsin interact. S. Spiegelman [5] suggests that the + gene produces a self-reproducing plasmagene which itself has no killer properties. This plasmagene unites with kappa to produce a compound self-reproducing plasmagene which mediates the production of an enzyme essential for the production of killer substance. Sonneborn himself does not put forward any very detailed hypothesis, but believes that the + gene and kappa may be chemically similar, and that the latter may have originated from the former during the course of evolution. These ideas are not all mutually exclusive, and a final explanation may embody points from several of them:

II. OTHER SPECIES OF *PARAMECIUM*

Genetical analysis is much less far advanced in other species of *Paramecium*. In *P. bursaria* mating types have been shown to exist, but the sexually isolated varieties contain not two but several mating types each. Each type can mate with any other type of its own variety. Hereditary differences are known in size, form and fission rate, but most work has been devoted to the study of the inheritance of mating type differences in variety 1. Genetical analysis is made more difficult in this species by the fact that the micronuclei are very variable in character, and the species may consist of a polyploid series.

In the vast majority, after conjugation the clones produced by the two ex-conjugants are alike in mating type, indicating that cross-fertilisation has occurred and that the identical synkaryons

of the two mates determine the mating type. A small number of exceptions may be the result of cytogamy. In the normal identical mating type approximately 40 per cent. of the conjugating pairs give rise to clones of the same mating type as one parent, 40 per cent. to clones of the same mating type as the other parent, while the remaining 20 per cent. give rise to clones different from either parent. This appears superficially similar to the inheritance of blood groups in man, but further observations make it clear that the matter is by no means so simple. H. S. Jennings [6] has observed that, although clones normally remain constant for mating type, selfing may suddenly occur in a culture, and is due to the presence of two mating types where formerly there was only one. This origin of a second mating type he calls "self-differentiation." Any one clone can produce by self-differentiation only two mating types, but two different clones of a single mating type do not always self-differentiate alike. For example, one clone of type A will produce only A and B, another A clone will give A and C, while a third will give A and D. Moreover, if the A and D arising from a pure A clone are allowed to conjugate they are capable of giving rise to A, B, C and D. No satisfactory explanation of these observations is as yet forthcoming.

III. *EUPLOTES PATELLA*

Work on the mating types of this species has been carried out by R. F. Kimball [7] and by E. L. Powers [8]. At least two varieties are known, but work has been confined to variety 1, which possesses six mating types. Conjugation occurs in any mixture of two mating types. The process of mating is different from that of *Paramecium*. On mixing two types there is no immediate reaction, then after about one and a half hours pairs are formed without any preliminary clumping. By using double animals resulting from incomplete fission as one type, Kimball was able to show that, although two mating types must be present for conjugation to occur, the pairs formed do not all consist of one member of each type. A mixture of double animals of one type with singles of another, as well as giving the expected double/single pairs, yielded also double/double and single/single pairs. This suggested that a contact stimulus might not be necessary for conjugation, but that some substance liberated into the culture medium might be responsible. Kimball therefore tried adding fluid from one type to cultures of a different type. He found that in certain instances heterotypic culture fluid induced selfing. Table 1 summarizes his results.

TABLE 1

THE INDUCTION OF SELFING IN *EUPLOTES PATELLA* BY THE ADDITION OF
HETEROTYPIC CULTURE FLUID

+ indicates selfing, - failure to obtain selfing, when culture fluid of the type shown in the first column is added to animals of the types given at the heads of the other columns. The arabic figures in brackets are the mating-inducing substances assumed to be present in the corresponding culture fluids.

Type from which Culture Fluid Originated.	Type Tested for Selfing after Addition of Fluid.					
	IV (1)	VI (2)	III (3)	I (1 and 2)	II (1 and 3)	V (2 and 3)
IV (1) . . .	-	+	+	-	-	+
VI (2) . . .	+	-	+	-	+	-
III (3) . . .	+	+	-	+	-	-
I (1 and 2) . . .	+	+	+	-	+	+
II (1 and 3) . . .	+	+	+	+	-	+
V (2 and 3) . . .	+	+	+	+	+	-

Kimball interpreted his findings as indicating that three mating-inducing substances exist, each of which will cause selfing only in such mating types as do not themselves produce the same substance. Thus type iv produces substance 1, and induced selfing in vi, iii and v. It has no effect on i and ii, and these must, therefore, also produce substance 1. Similarly vi produces substance 2, and so do i and v, while iii produces substance 3, as do ii and v. Unfortunately there are a few data which do not fit in with this scheme, and some modifications or complications of the simple picture will presumably be necessary to accommodate these exceptions.

The possibility of inducing selfing makes further genetical analysis simple. Types iv and vi on selfing give rise only to the parental mating types, and are therefore presumably homozygous. i on selfing gives iv, i and vi in a 1 : 2 : 1 ratio. i is therefore a heterozygote for two genes named mt^1 and mt^2 . The corresponding homozygotes are iv (mt^1/mt^1) and vi (mt^2/mt^2). ii on selfing gives iv, ii and iii in a 1 : 2 : 1 ratio, and is therefore heterozygous for two genes, one of which is mt^1 and the other is a third allele called mt^3 , while iii is homozygous mt^3/mt^3 . v on selfing gives vi, v and iii in a 1 : 2 : 1 ratio, and is therefore the heterozygote mt^2/mt^3 . All further crosses bear out this interpretation. A complete parallelism therefore exists between the genes determining the mating type and the postulated mating-inducing substances produced, as shown in Table 2. Further work on the chemistry of the inducing substances and their relations to the genes concerned may be expected to give interesting results.

The relations between cross-conjugation and selfing in mixtures

TABLE 2

RELATION BETWEEN POSTULATED MATING-INDUCING SUBSTANCES PRODUCED AND GENOTYPE IN THE MATING TYPES OF *EUPLOTES PATELLA*

Mating Type.	Substances Postulated.	Genotype.
I	1 and 2	mt^1/mt^2
II	1 and 3	mt^1/mt^3
III	3	mt^3/mt^3
IV	1	mt^1/mt^1
V	2 and 3	mt^2/mt^3
VI	2	mt^2/mt^2

of two mating types remain uncertain, for it is not always clear whether or not both processes are taking place in a mixture unless double animals are used as one of the types. Sonneborn postulates that cross-breeding will occur only when *each* type produces a conjugation-inducing substance not produced by the other. If this is true then cross-breeding should occur as shown in Table 3.

TABLE 3

SONNEBORN'S POSTULATED SCHEME OF CROSS-BREEDING IN *EUPLOTES PATELLA*

+ indicates that cross-breeding is expected, — that failure to cross is expected. The figures in brackets after each mating type are the mating-inducing substances assumed to be produced by that type.

Mating Type.	V (2 and 3)	III (3)	II (1 and 3)	IV (1)	I (1 and 2)	VI (2)
V (2 and 3)	—	—	+	+	+	—
III (3)		—	—	+	+	+
II (1 and 3)			—	—	+	+
IV (1)				—	—	+
I (1 and 2)					—	—
VI (2)						—

This gives nine possible crosses, of which five have been shown to occur, while there are no observations showing that the remaining four do not occur. There are also six crosses which should fail. Powers' data show that of these four in fact do fail, one cross has not been tried, and one exceptional cross of doubles of I by singles of VI has been made. Further work on the inducing substances may make the reason for this exception clear; and as a temporary working hypothesis Sonneborn's postulate is a useful summary of the results obtained to date, and indicates the desirability of undertaking such of the crosses as have not yet been attempted.

Sonneborn concludes his review by pointing out that work on the genetics of ciliates is as yet only in its infancy. The material

is particularly suited for studying problems of the relationship between gene and cytoplasm since in different genera diverse nuclear phenomena occur. A few examples may be cited: in *Vorticella* there is cytoplasmic fusion at conjugation; in *Dallasia* there are two processes, conjugation involving exchange of nuclei, and copulation involving fusion of gametes; while in some species of *Chilodonella* macronuclei as well as micronuclei are exchanged. Here is a virgin field providing "an endless source of favourable material awaiting enterprising students intent on attacking the problems of the nature of the gene and gene action."

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RECENT ADVANCES IN SCIENCE

ASTRONOMY. By A. HUNTER, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

A NEW MINOR PLANET.—Nearly 1600 asteroids are now known sufficiently well to have been given names and numbers. The list grows (or rather grew before the war interrupted much of this work) at the rate of about 30 or 40 a year, though many more than this are "discovered," only to be found identical with some previous object or lost again before a reliable orbit can be computed. Naturally enough, the average brightness at discovery decreases steadily: twenty years ago (if we confine ourselves to objects bright enough to be subsequently well observed) the average magnitude at discovery was about 13, whereas it is now nearer 15. The search for new asteroids is now a specialised business conducted by a handful of enthusiasts who use their big telescopes for little else. At the same time the interest of the work to non-specialists has correspondingly diminished. That is not to say, however, that its importance to astronomy is any less. The computation of asteroid orbits is still continued, not for any intrinsic interest in the orbits themselves but for the statistical value of the information to celestial mechanics in general, to perturbation theory in particular, and to theories of the origin of the solar system.

It is pardonable in these circumstances to take a closer look only at those minor planets that are peculiar in some way. Fifty years ago no known asteroid had an orbit which brought it closer to the sun than Mars. Then in 1898 Eros was discovered, revolving in a highly eccentric orbit which could at times bring it within 14×10^6 miles of the earth. (Since then two programmes of intensive international observation of this planet at its favourable oppositions in 1901 and 1931 have yielded progressively more accurate values of the size of the astronomical unit, the fundamental yardstick of the solar system.) Then in 1932 a new close neighbour, Amor, was discovered to have an orbit of high eccentricity with its perihelion between the earth and Mars. It can approach to within

about 10×10^6 miles of the earth. Also in 1932 was found an object which can come still nearer: Apollo got within $6\frac{1}{2} \times 10^6$ miles by virtue of an orbit (the first such to be found) which brings it at perihelion actually within the earth's orbit and which can under favourable circumstances cause it to approach within 2×10^6 miles. In 1936 and 1937 Adonis and Hermes were discovered, both with perihelia within the orbit of Venus and with actual distances of closest approach to the earth of about 1 and about $\frac{1}{2}$ million miles respectively.

The latest new discovery of this sort was made by Wirtanen at the Lick Observatory on March 7, 1948. From its position then, and when re-photographed on several occasions later in March, a preliminary orbit has been computed. Perihelion distance is rather less than that of the earth, but not so small as those of Apollo, Adonis and Hermes. Closest approach to the earth occurred in mid-April when it came within 16×10^6 miles; perihelion passage on May 21, when its brightness as seen from the earth was about 2 magnitudes fainter than at discovery (13th magnitude) both because of its greater distance and because of its phase, which was far from full. This new object did not make any spectacular dash across the sky as did Hermes in 1937, when it was moving at 5° an hour, but for that very reason its orbit is much better known, and unlike Adonis, Apollo and Hermes it will probably be picked up again at its next close approach. Its perihelion is about as far inside the earth's orbit as Amor's is outside, and Amor was successfully recovered in 1940 only a few degrees from its predicted position based on the observations made 8 years (3 revolutions) before.

It is not unnatural that the close approach of these minor planets should be looked upon with a certain amount of uneasiness. A collision between the earth and such an object as this new asteroid, which (calculating from its apparent magnitude and assuming the same low albedo as is found for the bigger minor planets) is probably about two miles across, would, of course, be a serious matter, indeed a catastrophe in a populated district. It is small comfort to reflect that such objects are rare—only four or five asteroids are known to come within the earth's orbit. They are only visible when close to the earth, and to make a close approach they must fulfil very special conditions: their orbits must be so oriented that they cross the plane of the ecliptic when they are at about an astronomical unit from the sun. Thus the chance of discovering one is small, and the few we know may be representative of hundreds which pass close enough to be appreciably affected by the earth's gravitational field. But the real danger, of course, is not here: it is in the

likelihood that for every flying mountain like Wirtanen's asteroid, capable of obliterating London or New York in the unlikely event of a direct hit, there are thousands of smaller objects which may have a not impossible chance of doing appreciable damage in inhabited country or (through a tidal wave) near populated coasts. Is our present civilisation perhaps lucky that the relatively recent impacts of these bodies have been in deserts or uninhabited forests : in Arizona about 5000 years ago, in Siberia in 1908, and on the Russian Pacific coast last year ? The intruder causing this last occurrence was observed visually (though it was daylight at the time) within a radius of 120 miles of the point of impact, and the concussion was audible 60 miles away. The fall occurred about 30 miles from the village of Kharkovka in the Ussuri-taiga area of the Maritime Territory on 12th February, 1947 ; and the point is marked by over 30 craters from 50 to 80 ft. across and up to 30 ft. deep. Trees in the vicinity have been levelled, radially from the point of fall, by the blast. The main body, which is believed to weigh several dozen tons and to consist mainly of metallic iron of specific gravity 7, is being recovered by a special expedition sent out by the U.S.S.R. Academy of Sciences.

It is perhaps stretching the nomenclature too far to call a body of this size an asteroid rather than a large meteorite, but if, as many lines of evidence suggest, the enormous lunar craters are the result of meteoritic bombardment, left evident by the lack of atmospheric erosion, it may be that the earth, too, is still subject to collisions with small asteroids, or large meteorites—it becomes less and less possible to draw any valid distinction between the two classes of body as time goes on.

THE HOT SOLAR CORONA.—Five years ago one of the outstanding problems confronting the solar physicist concerned the support of the chromosphere and corona. Assuming that the temperature of the solar atmosphere decreases outwards from the observed photospheric value of about 6000° K., it had proved quite impossible to account for the great extent of the solar atmosphere without providing some compensating force partially counteracting gravity more effectively than gas pressure alone could do. Efforts were therefore made to bring radiation pressure, turbulence and other effects to the aid of gas pressure, but with limited success. Then came a series of observations throwing doubt on the idea that the temperature does in fact decrease outwards. Eclipse observations of line widths in the chromosphere confirm deductions from the excitation level existing there in pointing to a temperature of the order of 35,000° K. for the lower atmosphere. Farther out, the widths of

the coronal emission lines and their extremely high excitation potentials (SCIENCE PROGRESS, 34, 556, 1946) are consistent only with a temperature of about half a million degrees for the inner corona; and it is unlikely that this drops appreciably till the outer coronal regions are reached, for the Fraunhofer lines are "washed out," by thermal broadening, from the solar light scattered by electrons in the middle corona. Still more recently, observations of "solar noise" (SCIENCE PROGRESS, 34, 751, 1946) even from the "quiet" sun require the coronal temperature to be about 10^6 degrees if the electromagnetic radiation at metre wave-lengths is thermal in origin.

Now if these temperatures are substituted in the ordinary hydrostatic equation, the gas pressure alone can produce the low density gradients observed for the chromosphere and corona. This is highly satisfactory in that it now enables us to dispense with any appeal to *ad hoc* effects to reinforce gas pressure; but, of course, it just pushes the problem back a stage, and we must now explain how these high temperatures are maintained. This is perhaps the most debated question in solar physics today, and it gives rise to answers of the most varied character. The most recent is due to M. Schwarzschild (*Astrophys. J.*, 107, 1, 1948) who adopts the view that the source of the energy is the convective motion of the solar granulation, and that it is transported to the corona by acoustic waves.

To maintain a steady state in the corona, the energy supplied must compensate the heat loss suffered. Now at first sight it might be supposed that this could be directly estimated from the integrated brightness of the corona, which has been measured with reasonably accordant results at eclipse after eclipse in the past half-century. But most of this brightness is due to electron scattering of photospheric light, a process which is, of course, not relevant to the energy balance in the corona. Schwarzschild accepts the view that the main loss is due to emission from electrons accelerated in the field of protons—the so-called "free-free transitions." This process gives a rate of energy dissipation of 6×10^{25} ergs/sec. for the whole corona. He next discusses whether the kinetic energy of the rising granules to be seen in the photosphere can provide for this loss. From a variety of observational data on the diameter, velocity and lifetime of the average granule, from their total number at any instant, and from the properties of the convection zone in which their motion takes place, he estimates that they carry to the solar surface 10^{30} ergs/sec., which is ample for his purpose.

But the problem is not yet solved: he has to get this energy through the photosphere, leaving that unchanged, and deliver it to the coronal material at its observed high temperature. The mechanism he proposes consists of acoustic waves of the nature of noise * arising from perturbations of the photosphere immediately overlying the convective zone and transmitted by the granules into the layer above. Assuming that the particle velocity in the waves is at most as large as the speed of ascent of the granules, and using the ordinary expression for the sound velocity in the photosphere, Schwarzschild finds an upper limit of 10^{30} ergs/sec. for the energy transport of the stream of noise. This numerical agreement he regards as fortuitous, because a number of factors will tend to reduce the energy content of the noise: the material velocity may be much lower than the granulation velocity; each granule may be effective as a source of waves for only part of its lifetime; and much of the noise may escape horizontally or (especially at the lower frequencies) may be reflected back by the steep density gradient. So long as these factors do not reduce the energy content by more than 10^4 , however, the noise will start out with sufficient energy to supply the heat losses of the corona.

It is important to be sure that no serious dissipation of this energy can occur before the noise reaches the corona, first because, if it does, enough may not be left to maintain the corona at its high temperature, and second because the intermediate layers must not be heated by its passage. The dissipation depends on the ratio of the mean particle velocity ω to the wave velocity V : as ω approaches V the noise degenerates into a shock wave and dissipation becomes excessive. An examination of the probable value of ω/V throughout the sun's atmosphere shows that in the photosphere the ratio is negligibly small and the noise can therefore produce no appreciable heating. The gradient of density ρ is therefore that predictable from the ordinary hydrostatic equation. Since there is little dissipation, the energy transport $\rho \omega^3 V$ must remain constant in spite of the upward decrease of ρ . Thus, since

* The use of this term in this connection emphasises the inappropriateness of its use to describe *electromagnetic* radiation emanating from the sun at wave-lengths of the order of metres. It is no doubt because radio waves a metre or so in length are short to the radio physicist but long to the astrophysicist that when the two collaborated in selecting a short name for the phenomenon, the present totally irrelevant term "solar noise" emerged. Now that astronomers really want to talk about acoustic waves of mixed wave-lengths they find the obvious term pre-empted for another purpose. However, the radio meaning is already too firmly established to be shaken, but the word is here used in its acoustic sense.

V is not dependent on ρ , ω must increase upward until, at a point defined as the base of the chromosphere, where the temperature is still not far from photospheric, ω/V reaches a value (Schwarzschild selects $2/7$) above which appreciable dissipation starts. In this isothermal region between the top of the convective zone and the base of the chromosphere, ω increases 20-fold and ρ correspondingly decreases by a factor of 400. From the hydrostatic equation this occurs in a depth of 800 km., which is quite enough to comprise all the photosphere. Thus, as required, the noise proceeds undiminished in intensity through an unaffected photosphere into the chromosphere and corona.

There the dissipation is governed as before by the relation of ω to the critical value of the wave velocity V . At any point the temperature will be that corresponding to this critical value of V : if the temperature were above this, the rate of dissipation would fall and the temperature would drop; if the temperature were below the critical value, much of the noise energy would be transformed into heat and the temperature would rise. Using this fact and the observed 10^6 degrees for the maximum temperature of the corona, Schwarzschild finds that the noise dissipation increases the temperature to $35,000^\circ$ K. at a height of 700 km. from the photosphere and to 10^6 degrees at a height of 35,000 km. This steep temperature gradient is of the order required by observation. The author can therefore claim that convection currents observed as the solar granulation can produce more than enough energy to maintain the corona at its observed temperature; that a stream of acoustic waves can transport a sufficiently large proportion of the energy so provided from the convective zone through the photosphere (which the noise leaves dynamically and thermodynamically unchanged at 6000° K.) to the corona; and that there degradation into heat energy will proceed rapidly and will produce a rise of temperature as steep as is actually observed.

The subject of maintaining the coronal temperature is at present in the stage where diametrically opposite hypotheses can hold the field simultaneously, and it would not be fair to other theories to omit to state that in the view of some solar physicists the source of coronal energy lies outside the sun altogether. Bondi, Hoyle and Lyttleton (*Mon. Not. Roy. Astr. Soc.*, 107, 184, 1947), for example, find that a moderate rate of accretion of interstellar material with high energy per unit mass will produce round the sun a convective atmosphere at a temperature of about 10^6 degrees which they identify with the corona. It is, of course, by the interplay of such opposing ideas that progress will come.

A NEW PLANETARY SATELLITE.—A recent report from Dr. Gerard P. Kuiper announces the discovery of a new satellite to Uranus. First detected on a photograph taken with the 82-in. reflector of the McDonald Observatory in Texas, it is an object of magnitude 17 which could not be seen visually at the Cassegrain focus but was readily photographed in two or three minutes. Its distance from the planet is about 0.64 that of Ariel, the innermost of the four satellites previously known. Its period of revolution, though not yet determined observationally, will be about 30 hours at that distance.

Its size is and will remain quite uncertain, direct micrometric observations being impossible with such a small object. One might hazard a guess from its magnitude—perhaps a diameter of 150 miles. The discovery leaves the satellite system of Uranus unique in one respect, namely, the close packing of the moons. The remotest satellite is distant only 11 diameters from its primary, whereas for Saturn the corresponding figure is 108 and for Jupiter 166. It will be interesting, when the orbit of this new satellite is well enough known, to see if it falls in with the other unique feature of the system, namely, the high inclination (98°) of the satellite orbits to the plane of the planet's orbit. Uranus itself is not peculiar in this respect (it moves within a degree of the ecliptic), but in no other satellite system do the moons revolve backwards in orbits inclined nearly at right-angles to the main orbit. As might be expected from the planet's ellipticity, its rotation speed, as determined spectroscopically (no disk markings are sufficiently distinctive to permit a direct determination), is high, corresponding to a day of only $10\frac{1}{2}$ hours. The rotation is in the same sense as the revolution of the hitherto known satellites, and the absence of orbital perturbations by the equatorial bulge makes it probable that the planet's equatorial plane coincides with the orbital plane of the satellite system. Unless further observation of the new satellite modifies this deduction, the equator of Uranus must lie almost perpendicular to its orbital plane, and in consequence the seasonal variations on the planet will be enormous. No other planet's equator is inclined to its orbit by more than 29° (the earth's is at $23\frac{1}{2}^\circ$ to the ecliptic), and in no other satellite system is the plane of a satellite's orbit inclined at more than 29° to the orbit of its primary, except in the rather peculiar cases of the outermost moons of Jupiter, the most distant satellite of Saturn and the single satellite of Neptune, all of which show retrograde motion in orbits of inclination up to 40° .

If astronomy ever reaches a state where only two or three theories of the origin of the solar system hold the field, each explain-

ing all the major observed facts (a state of affairs which is conspicuously not the case at present), it may well be that the crucial test between them will be ability to explain how this satellite system came to have its present extraordinary properties.

STELLAR MAGNETIC FIELDS.—Further work in continuance of that already described in these pages (*SCIENCE PROGRESS*, 35, 658, 1947) has led H. W. Babcock to a discovery which raises doubts about the fundamental connection between rotation and magnetism suggested independently by himself and by Blackett. It will be remembered that observations of the magnetic fields of certain suitable stars appeared to show that the observed proportionality between magnetic moment and angular momentum which was already known for the earth and the sun held also for the stars.

Babcock has been using his same observational technique employing a compound circular analyser on the star BD — 18° 3789, an early-type star whose spectrum lines show periodic changes in intensity. Such spectrum variables are not uncommon: a well-known case is that of α Canum Venaticorum, a star in which the rare-earth lines fall into two groups which show marked reciprocal intensity changes. In BD — 18° 3789 the lines of ionised europium exhibit periodic changes in strength in opposite phase to similar variations in the lines of ionised chromium. The period is about 9.3 days, but the spectrum changes are not, so far as is yet known, accompanied by any change in the star's luminosity or colour.

Babcock's first plates showed (*Publ. Astr. Soc. Pacific*, 59, 260, 1947) that when the *Eu* II lines are strong, the integrated Zeeman effect shown by the sharp lines in the star's spectrum corresponds to a polar magnetic field of 5500 gauss, the strongest yet found in any star. Further plates also taken when *Eu* II was prominent indicate that the field varies, reaching a maximum of 7000 to 8000 gauss roughly in phase with the intensity of the *Eu* II lines. When the *Cr* II lines become strong, however, the sign of the field actually reverses, and a Zeeman effect is observed characteristic of a field still of the order of 6000 gauss but indicating opposite magnetic polarity to that at *Eu* II maximum. Practically all the measurable lines, including the strong K line of *Ca* II and lines of neutral and ionised iron, show the reversal. At intermediate phase, when both *Eu* II and *Cr* II lines are visible, some lines show one polarity and some the other, but the average effect appears to correspond to a field near zero.

It would be premature to conclude from this remarkable observation that the Blackett-Babcock theory can have no basis in fact: a more complete report on this star and on other similar ones must

be awaited. But if further observations confirm this short-period reversal of polarity, the cat will be among the pigeons. It is bad enough to have to explain the existence of a strong magnetic field in a star at all, without having to account further for its reversal in a matter of days.

PHYSICS. By F. A. VICK, O.B.E., Ph.D., F.Inst.P., The University, Manchester.

THE USE OF CRYSTALS FOR THE DETECTION OF IONISING RADIATIONS AND PARTICLES.—In an earlier article in this series (*SCIENCE PROGRESS*, 35, 668, Oct. 1947) electron energies in semiconductors were discussed. It was shown that the electrical conductivity of such crystalline solids at normal temperatures depends on the existence in the lattice of impurities or defects of some kind. Without these, *i.e.* in a perfect crystal, electrons are normally bound to the atoms or ions of the lattice, and must be given additional energies of the order of several electron volts before being able to move from atom to atom in the solid under the influence of an applied electric field. This additional energy corresponds to the ionisation potential of a free atom or molecule in a gas. The solid is thus normally an insulator, and a very high temperature or very high electrostatic field is required before it can conduct.

In principle, the energy required to raise such an electron from the "full band" of energies to the "conduction band" (*see* Fig. 3, *loc. cit.*) to enable the solid to conduct may be obtained in other ways, *e.g.* by absorption of radiation of a high enough frequency. If, for example, a pulse of radiation is absorbed it will liberate to the conduction band a few electrons which may be swept by a suitable applied field to a positive electrode to produce a corresponding pulse of current. [Liberation of an electron to the conduction band leaves behind a "positive hole" in the normally full band, which will also migrate under the influence of the applied field (*loc. cit.*, p. 671).] This process is analogous to the action of the Geiger-Müller Counter which has for so long been one of the most important tools of the nuclear physicist and in the study of cosmic rays.

Apparently, the first person to study the possible use of crystals as counters was P. J. van Heerden (*The Crystal Counter*, Dissertation, Utrecht, 1945). His experiments were most successful with crystals of silver chloride, though he found it necessary to cool them to liquid air temperatures to reduce the spontaneous residual conductivity present at room temperatures. Platinum electrodes were evaporated on to opposite faces of the crystal, and a field

applied between them. The pulses of current through the crystal when bombarded by α -, β - or γ -rays were detected after suitable amplification. In spite of the disadvantage of liquid air cooling, the possibility of a counter of small size, high density, moderate operating voltage and higher counting rate has stimulated further experimental and theoretical work, particularly in U.S.A.

R. Hofstadter, J. C. D. Milton and S. L. Ridgway (*Phys. Rev.*, **72**, 977, Nov. 1947) have extended van Heerden's work with silver chloride. After opposite faces had been sputtered with platinum, the crystal was annealed at 400° C. for several hours, and then cooled to room temperature over a period of several days. It was then mounted in contact with silver foil at a high potential (value not stated) resting on a crystalline quartz holder. The crystal was cooled slowly to liquid nitrogen temperature by conduction through the quartz. A clip and foil made contact with the top face, and were connected to a pulse amplifier. The crystal was irradiated with γ -rays from RaC or Th(C + C''). The individual pulses were examined on an oscillograph screen. In some cases, the strain pattern of the crystal was observed through crossed Nicol prisms, since regions of strain may give rise to electron traps and so reduce the range of the electrons in the conduction band (see SCIENCE PROGRESS, **34**, 562, 1946). The largest pulses were observed for the most uniform crystals, a crystal which had not been annealed giving no observable pulses. The heights of the pulses varied up to a maximum of about 1.3 millivolts and the mobilities of the electrons in the crystal were calculated to be 100–400 cm./sec. per unit field, which was smaller than expected from the theory of Froehlich and Mott (*Proc. Roy. Soc.*, **A171**, 496, 1939) for a perfect crystal. The energy required to form a free electron (or ion pair) was estimated to be about 13–16 electron volts in favourable cases.

To obtain a large "signal-to-noise ratio" for the observed pulses, it is obviously important to reduce the residual conductivity arising from impurities or strains in the crystal lattice, and at the same time to increase the range and mobility of the electrons released to the conduction band. J. R. Haynes has described (*Rev. Sci. Instr.*, **19**, 51, Jan. 1948) experiments to this end. He began with large cylinders of silver chloride, $3\frac{1}{4}$ inches diameter and several inches long, in which large single crystals could be detected by examination in polarised light or by etching the surface with photographic hypo. The crystals were cut by a circular milling saw, the saw marks removed by a wet sanding machine, and an additional 0.03 cm. removed from each surface by hand grinding using wet aluminium oxide paper on plate glass. Then the surfaces of the crystals were

etched away by agitating them in hypo solution to remove embedded aluminium oxide particles, the crystals were washed in running water, rinsed in distilled water and dried with filter paper. The crystals were annealed very carefully at 400°C . for two hours, and the temperature then reduced by about 18° per hour. The crystals were then found to give greatly increased electron range and mobility. Silver electrodes may be deposited on the crystals by the use of a developer consisting of 70 gm. of hydroquinone and 250 gm. of sodium carbonate in 120 c.c. of water.

The inconvenience of cooling silver chloride to liquid air temperature has naturally led to the search for crystals which can be used at room temperatures. Van Heerden (*loc. cit.*) tried one diamond of gem quality, but detected no pulses. D. E. Wooldridge, A. J. Ahearn and J. A. Burton (*Phys. Rev.*, **71**, 913, June 1947) were, however, more successful. They used chips sawed from natural diamonds in eliminating flaws, etc., and investigated the pulses arising from bombardment by α -particles from radium, with the crystal at room temperature. Two different electrode systems were used: (a) A pair evaporated on the same surface of the diamond and separated by a gap of about 0.003 cm. The conductivity is then mainly along the surface layers. Good pulses were obtained with only five volts across the gap. (b) Evaporated electrodes on opposite faces of a crystal about 0.5 mm. thick. Good pulses were obtained with a P.D. of about 100 volts, and saturation with a field of about 2000 volts per cm. With each of these electrode systems, pulses of comparable size were obtained with the applied field in either direction. Also, pulses were observed when the field was reduced to zero. These "space-charge" pulses were opposite in direction to those occurring with an applied P.D., and soon died away. No pulses were observed with some diamonds, however treated.

Further work on diamonds has been described by L. F. Curtiss and B. W. Brown (*Phys. Rev.*, **72**, 643, Oct. 1947), using γ -rays. About 100 industrial diamonds of roughly $\frac{1}{4}$ carat size were tried, all in their natural states. Two gave excellent response to γ -rays, and others showed some response. All these were single crystals free from obvious flaws, which substantiate the remarks made above on the necessity for perfect crystals. The pulse size was variable, about ten times noise level, indicating that the electrons originate at random throughout the effective volume of the crystal. This and later work shows that diamonds which exhibit crystal counting properties are relatively rare. As has been pointed out by H. Friedman, L. S. Birks and H. P. Gauvin (*Phys. Rev.*, **73**, 196,

Jan. 1948), the existence of two types of diamond, one much rarer than the other, was described by Robertson, Fox and Martin (*Phil. Trans.*, **A463**, 232, 1934). The rarer Type II is characterised by transparency in the U.V. down to 2250 Å.U., and in the I.R. at 8 μ , while Type I is opaque below 3000 Å.U. and also at 8 μ . From the work now described it appears that the counting characteristic is another exclusive property of the Type II diamond, the Type I diamonds tried being unusable for counting. These observations are further discussed by R. Hofstadter (*Phys. Rev.*, **73**, 630, March 1948) in the light of recent work by C. V. Raman and his colleagues (*Proc. Indian Acad. Sci.*, 1944 and 1946). Apparently, Type I diamonds have a mosaic and Type II a laminated structure, in which case the electron range parallel to the laminations might be expected to be longer than in the mosaic structure, since electron traps are likely at the boundaries of the microcrystals forming the mosaic. Further work is needed to test this view.

In another paper (*Phys. Rev.*, **72**, 1120, Dec. 1947) Hofstadter describes tests on thallium halide crystals, which have photoconducting properties similar to those of the silver salts. The crystals have to be annealed and cooled as for silver chloride, and when this was done pulses were detected. Such crystals should be efficient γ -ray detectors since they are comparatively dense. It is not at present, however, easy to obtain pure crystals of thallium salts.

Silver chloride crystals operating at liquid nitrogen temperatures are now used for the detection of high energy protons produced by recoil and exchange reactions in the neutron beam generated by the 184-inch cyclotron at Berkeley (L. F. Wouters and R. S. Christian, *Phys. Rev.*, **72**, 1127, Dec. 1947), the pulses being up to 20 times the input-stage noise level of 50 microvolts. The chief difficulty encountered is the gradual decrease of pulse amplitude as counting proceeds, due to space charge in the crystal. This space charge is found to persist for periods as long as 48 hours. The behaviour on reducing the applied field to zero is similar to that noted above for the diamond, pulses of reverse polarity being observed until the space charge disappears. The procedure adopted is to reverse the applied voltage after each 1000 counts or so.

Conductivity pulses similar to those in the diamond have been observed recently in natural single crystals of zinc sulphide when bombarded with α -particles from polonium (A. J. Ahearn, *Phys. Rev.*, **73**, 524, March 1948). Pulses appear even when the impurities are appreciable, in contrast to the observations with the diamond and silver chloride.

There is thus emerging a new tool of potential importance to nuclear physics, which also provides a further stimulus to the experimental and theoretical study of non-metals. As an example of the latter, an interesting paper on "The Mobility of Electrons in Pure Non-Polar Insulators," by F. Seitz, has just been received (*Phys. Rev.*, **73**, 549, March 15th, 1948).

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WIND STRUCTURE AND ATMOSPHERIC TURBULENCE

1. INTRODUCTION

THIS article, the second half of which will be held over until next January's issue of SCIENCE PROGRESS, reviews progress made in atmospheric turbulence and wind structure since about 1939 and is a record of, in the main, rather minor advances over practically the whole front. Deeper penetration in certain experimental and theoretical directions waits on the exploitation of techniques now under development but not yet adequately applied to the meteorological problem. In particular, the statistical theory of isotropic turbulence invented by Taylor in 1935 and developed later by Taylor, von Karman, Batchelor and others has not yet been seriously introduced into meteorology, though application is envisaged. The new statistical theory will not, therefore, be discussed in this article, and the reader requiring guidance may be referred to excellent recent reviews by Dryden (1945) and Cumming (1946).

That part of the article printed in the current number deals with instrumental advances and additions to observational knowledge. The later part will deal with advances in the theory of atmospheric turbulence.

2. ANEMOMETRY

The experimental problem of wind measurement has two aspects, (a) the accurate determination of profiles of *mean* wind velocity in a boundary layer, *e.g.* the boundary layer arising in the lower atmosphere from friction at the ground, but generalisable to any region of the atmosphere exhibiting a marked change of wind with distance (not necessarily vertical), and (b) the measurement of the turbulent fluctuations of wind velocity and the correlations which may be established between the fluctuations, either of different components at a fixed point (related to shear stress), or of individual components at different instants or positions (temporal

or spatial correlations). Evidently the design of apparatus for the purposes (a) or (b) will differ markedly.

Improved apparatus for the measurement of velocity profiles near the ground has been described by Sheppard (1940), who uses a light, sensitive, portable, cup anemometer for direct reading, and by Deacon (1948), who has developed the former to a precision recording instrument.

Anemometers using rotors, cups or vanes are subject to positive errors in a fluctuating wind, and the error with some instruments may be several per cent. in natural winds. Scrase and Sheppard (1944) have shown that the error of cup anemometers is much reduced by using beaded, conical cups instead of the more usual hemispherical cups. Small systematic errors, such as these, are of no great consequence in the determination of velocity profiles, but it is very important to minimise random error or variable systematic error if the effects of thermal stratification on the profile are to be adequately established.

No very notable advance in the technique of measurement of the turbulent fluctuations of wind has been reported in the period under review, but Loeser (1944) has examined the desirability of measuring various functions of the eddy velocity (gustiness, variously defined, is in question), and he describes apparatus for the measurement of gust accelerations $\Delta u/\Delta t$, where Δu is the change in wind speed in an interval of time Δt , the resolving power of the apparatus being $\Delta t = 0.1$ sec. But it is doubtful whether the anemometer used was capable of responding adequately to fluctuations of that order of periodicity. The instrument indicates the quantity $\sum_{-b}^{+b'} \omega_b$, where ω_b is the frequency of gust acceleration b , and b' is the maximum gust acceleration (25 cm. sec.⁻²) to which the instrument will respond.

Sheppard (1941) has given an historical and critical review of anemometry dealing both with mean wind and eddy velocity measurement, but mainly in relation to observations near the earth's surface. Johnson (1946) has described the radio and radar methods now in use for the measurement of the wind, independent of weather, in the free atmosphere up to 20 km. or so. Techniques for the measurement of variance of wind at any level in the free atmosphere have been developed recently, but await publication.

3. OBSERVATIONAL DATA

The importance of turbulence in meteorology, as in other branches of physics, is in the diffusion which it produces. Of fundamental

meteorological importance are the diffusion of momentum, heat and water vapour, though for the study of the turbulent diffusion mechanism, observations may be made on *any* convenient diffusible entity, *e.g.* dust, smoke or a foreign gas; indeed, there are many practical problems facing the meteorologist in which the diffusion of the latter entities is concerned. As with the wind velocity itself (*cf.* (a) and (b) in § 2 above) the observational approach may be either through the distribution of the mean value of the entity considered, or through its turbulent fluctuations, preferably in association with corresponding observations on wind itself.

VELOCITY PROFILES AND DRAG.—Dealing first with observations on mean as opposed to instantaneous values, and of these first with the wind speed (momentum per unit mass of air) and the interchange of momentum, Sheppard (1947*a*) has made direct measurements of the drag at the earth's surface with the accompanying velocity profiles. It was already known that in near-neutral statical equilibrium the velocity increased accurately as the logarithm of the height above the surface over the first few metres, though there was sometimes a difficulty in allocating an appropriate datum for height over a very rough surface. This logarithmic variation was in agreement with observations over flat plates and not too far from the walls of tubes or channels. But the exact correspondence of the profiles had not been established until Sheppard's observations showed that, for the lower atmosphere as for the laboratory,

$$u = \frac{(\tau_0/\rho)^{1/2}}{k} \log_e z/z_0 \dots z > z_0 \quad (3.1)$$

where u is the mean wind speed at height z ,

τ_0 is the surface drag or shearing stress,

ρ is the density of the air (or other fluid medium in the laboratory),

k is von Karman's constant (non-dimensional),

and z_0 is a length parameter of the profile determined by the roughness of the underlying surface and usually of the order of 1/10 of the height of the roughness elements but depending on their geometrical form and spacing,

with k equal to about 0.4. The value of k actually measured in these experiments, *viz.* 0.45, was rather larger than that obtained in pipes and channels, but the small difference was almost certainly due, experimental error apart, to the observations having in fact been made in slightly unstable conditions. Taking the shearing

stress to be independent of height over the first 2 m., the observations also provided a value of the mixing length l defined by

$$(\tau_0/\rho)^{\frac{1}{2}} = l(du/dz)$$

and it was found that l increased rather more rapidly than the first power of the height, a further probable consequence of the instability, with $l = 0.45z$ near the origin. The significance of these observations resides primarily in the complete correspondence established between the properties of the mean motion on the entirely dissimilar scales of the laboratory and the atmosphere, and they show that the form of the velocity profile, *for a given surface drag*, is independent of the intensity and scale of the turbulence, both of which are much greater at a few metres above the earth than in the thin turbulent boundary layers of the laboratory (at equal distances from the boundary in laboratory and field one would, of course, expect equality in intensity and scale of the turbulence). The observations also enable the drag of any fixed rough surface in near-neutral statical equilibrium to be calculated from observations of mean wind speed u_1 , u_2 at two heights z_1 , z_2 , since from (1)

$$(\tau_0/\rho)^{\frac{1}{2}} = \frac{k(u_2 - u_1)}{\log_e z_2 - \log_e z_1}$$

One of the peculiar interests of atmospheric turbulence, apart from its scale, is the effect of stability or thermal stratification, so that the variation in the form of the velocity profile with change in the lapse rate of temperature is of great importance. In this respect the most notable published profiles are those of Thornthwaite and Kaser (1943), taken over a flat field in Ohio at up to 12 levels between 0.5 ft. and 28 ft. The u , $\log z$ curves for successive hours throughout the day show a marked progression of form, being concave to the u -axis between sunset and sunrise, that is during the period of temperature inversion, linear shortly after sunrise and before sunset when conditions are approximately dry-adiabatic and convex to the u -axis during the central daylight hours of super-adiabatic lapse rate. Thornthwaite and Kaser do not analyse their profiles in relation to the lapse rate, which was not apparently measured over the range of anemometer levels, and which, like wind gradient, is a function of height, but Halstead (1943) has shown that the curvature of their profiles is intimately related with the temperature difference which was recorded between 2 ft. and 8 ft. Firesah (1940) observed a similar dependence of curvature of the u , $\log z$ curves upon lapse rate in observations between 10 cm. and 200 cm. over a short grass surface. The relevant parameter determining the curvature of profiles is, however, probably the ratio

of the lapse rate of potential temperature to the square of the wind gradient (Richardson's parameter, apart from constants); rather than the lapse rate itself, as indicated by Deacon—see Sheppard (1947b). Deacon has in fact made beautiful recordings of the wind profile up to 13 m. over downland with the apparatus referred to in § 2, and has established a relation between profile curvature and Richardson's Number, which is likely to be very helpful to an understanding of the effects of stability on turbulence, and which must be taken into account in any satisfying theoretical development (see § 4 to be published later). Deacon's full results are, however, awaiting publication.

VELOCITY PROFILE AND DRAG OVER THE SEA.—The velocity profile over the sea is of special interest since, although the effects of stability are less marked than over land, the geometrical roughness of the underlying boundary is here a function of the mean wind at some reference height, and there is the additional difference from the land of a wind velocity which, though small, does not vanish at the surface itself. These apparently more complicated conditions at the sea surface are of special interest in relation to (a) the intensity of turbulence over the sea—small relative to that over land in similar lapse rates, (b) the production of waves, and (c) the value of the frictional drag—also probably small relative to that over most land surfaces. It is, unfortunately, very difficult to make measurements of the wind profile over the sea with adequate accuracy, and early measurements are suspect on account of imperfect exposures, etc. Bruch (1940) has, however, substantially improved our state of knowledge in this respect with observations over the Baltic and an inland lake, though between 16 cm. and 212 cm. only. Water and air temperature profiles were also recorded.

Model (1942) in an analysis of Bruch's observations suggests that the best representation of the wind profile over the sea is, in our notation,

$$u = \frac{(\tau_0/\rho)^{\frac{1}{2}}}{k} \log_e \frac{(z-d)}{z_0} \quad z > 5 \text{ cm.} \quad (3.2)$$

where d is a zero-point displacement of 4 cm. from the mean level of the sea surface, and z_0 the roughness parameter is 0.03 cm. only, independent of the actual geometrical roughness. This profile leads to an equation for drag

$$\tau_0 = \frac{1}{2} C_D \rho u_1^2$$

where C_D , the drag coefficient, has the very small value relative to that for solid surfaces of 0.0024 for u_1 measured at $z_1 = 126$ cm. In

spite of the usually large geometrical roughness of the sea surface, the small values of z_0 and C_D are not unacceptable in view of the wind-induced motion of the water surface. Nor, for the same reason, is the result of z_0 constant very surprising, though it would presumably demand that the sea surface should always be in very nearly complete equilibrium with the wind above, a condition probably violated in the presence of swell. Westwater (1943) has examined the veer of the surface wind direction from the direction of the wind at the top of the friction layer over the North Sea, Mediterranean Sea and North Atlantic Ocean and, from the small angles of veer found relative to those observed over land, and from the different form of the frequency distribution curves, arrives at conclusions on the nature of the friction process at the sea surface qualitatively in accord with Model's. But Model's analysis is not free from objection, since he assumes the existence of a laminar sub-layer which is inconsistent with the square law of drag given above, and with the form of his profile equation. Further, Bruch's observations were affected by stability, as shown by his temperature profiles, but to what extent is not clear owing to his method of analysis, and it may be that Model's representation of Bruch's profiles is affected by this hidden factor.

PROFILES OF TEMPERATURE AND HUMIDITY.—Turning now to other entities than momentum, a good deal of work has been carried out recently on the distribution of temperature and humidity, not merely near the surface but over a substantial part of or the whole layer commonly considered to be mainly dominated by turbulent interchange, say the bottom kilometre or so of the atmosphere. The need for these observations arose from the markedly variable effect which the lower atmosphere produces on the propagation through it of radio waves shorter than about 5 m. in wave-length when the ray-path is nearly horizontal, the propagation being determined by the profile of refractive index of the lower atmosphere. That profile is in turn dependent on the profiles of pressure (practically constant in form), temperature and humidity, and a fairly complete account of knowledge on the latter two profiles, over sea and land, as it existed in published and partly unpublished form at the end of 1945, has been given by Sheppard (1947c) in a 1946 London symposium on radio meteorology. To the bibliography given there may now be added papers by Craig (1946) and Emmons (1947) relating to the distribution of temperature and humidity over the sea in the lowest 1000 ft. or so of air which had in the main recently left a warmer land surface, by Fransilla (1940) who measured profiles of wind speed, temperature and humidity between

12 cm. and 350 cm. over an inland sea, by Sverdrup (1946) who gives values of temperature and vapour pressure over the ocean between 6 m. and 32 m. in winds up to $16.5 \text{ m. sec.}^{-1}$, and by Nyberg (1939) who made detailed observations of temperature between 1 mm. and 25 mm. over a smooth snow surface during strong inversions. An experimental investigation by Fransilla (1936) on the heat balance at the earth's (grass-covered) surface is also to be noted, having been omitted from Sheppard's (1947c) bibliography. Fransilla made observations of radiation, earth-temperatures and wind, as well as of air-temperature and humidity profiles between 1 cm. and 1.6 m. and has discussed his observations in relation to synoptic conditions.

Over the bottom 50 m. or so the profiles of potential temperature and humidity mixing ratio (or vapour pressure itself in such a small range of height) are logarithmic, as for wind, provided the lapse rate of potential temperature is very small, as it usually is over the sea. In the more stable or unstable conditions commonly experienced over the land and occasionally over the sea (*cf.* Bruch), however, the profiles show the same behaviour as that noted above for wind. Evidently the turbulent transfer mechanism, in this layer at least, is similarly affected by the stability or instability, whatever the entity transferred. That is not to say that the turbulent transfer coefficients for momentum, heat and matter are numerically equal (though varying with the stability) but the observations are pointing that way. See also § 4 on this matter.

The profiles of the bottom 50 m. or so are quickly adjusted to variations in the boundary conditions, *e.g.* of surface temperature or vapour pressure, so that advection effects are not normally evident in this layer except in a very general sense, such as a stable lapse rate being set up in air travelling over a cold surface with large thermal inertia in the material below. The *form* of the profile itself is, rather, determined by conditions at the point in question. Over the greater depth of the earth's boundary layer, on the other hand, conditions are very liable to be influenced to an extent varying with height by the past history of the air, and conditions are, therefore, more complicated, though of great concern in many meteorological problems, *e.g.* the formation of turbulent layer cloud. Craig and Emmons have each investigated in detail a considerable number of soundings to determine the modification of the lowest 1000 ft. or so of air passing from land to mainly cooler sea, with trajectories over the sea of the order of 10 miles (Craig) and 100 miles (Emmons). The complexities of the profiles referred to above are much in evidence, but aided by analysis, using the

Taylor diagram of vapour pressure versus air temperature, a certain orderliness is beginning to appear. The modifying influence of cooler water spreads upward relatively rapidly over the first 50 miles of path, but is slight thereafter, the depth of the affected layer being about 600 ft. for 250 miles of path when the initial temperature difference is from 10° F. to 20° F. with "surface" winds less than about 15 mi./hr. With the initial temperature difference less than about 5° F. and stronger surface winds, the height of influence may reach 1000 ft. in about 100 miles of path. The transformation

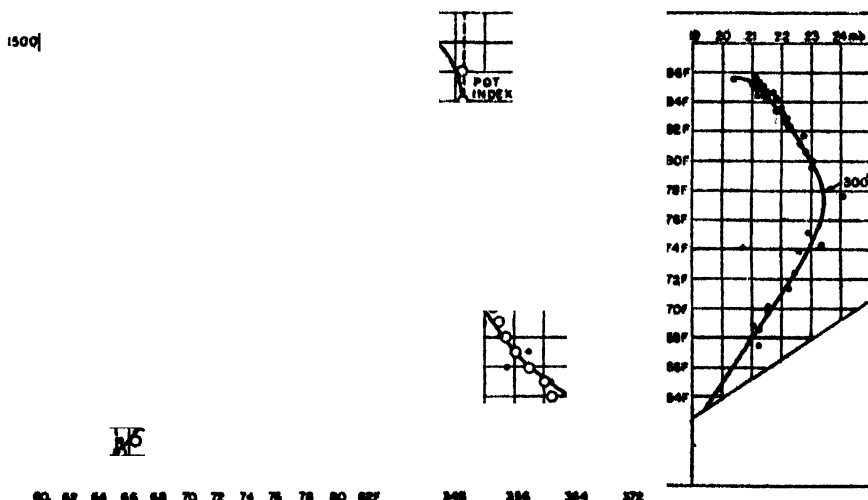


FIG. 1.—Sounding in air off Long Island, 16 June 1945.

Over water trajectory for air in upper half of sounding, 125 miles.

'Surface' wind, 3 Beaufort; 1000 ft. wind, 20 knots. Sky clear.

O = values observed on first ascent, • = on second ascent, 17 min. later. Arrows indicate values at sea surface.

Right-hand diagram (Taylor diagram) has co-ordinates potential temperature and potential vapour pressure, referred to sea-level pressure. Dashed lines in other three diagrams refer to constant potential values.

The third diagram from the left shows values of the potential refractive index n_p of the air for short radio waves in units $(n_p - 1) \cdot 10^6$.

Modification by relatively cool water extends to 300 ft.

Note agreement between extrapolated Taylor diagram curve and observed sea-surface temperature.

After EMMONS (1947).

of the air is, however, occasionally much affected by "shearing stratification," using Craig and Emmons' nomenclature, by which is meant the thermal stratification resulting from air at different levels having different trajectories and source temperatures, due to the change of wind speed and direction with height in the friction layer, and to diurnal variation of temperature overland. In spite of such complexities the temperature, vapour pressure values in the modified layer generally lie on a straight line in the Taylor diagram, and the inferred water temperature, as given by the cut

of the extrapolated line with the salt-water curve, agrees well with the observed value. (An example from Emmons is given in Fig. 1.) Emmons infers that the turbulent diffusivity of heat and matter are identical.

Observations of the kind described and discussed by Craig and Emmons require a large effort in manpower and material for their collection, and it is very important that such work, carried out in several places during World War II to clarify radio-propagation problems, should be very carefully analysed and discussed in order to extract the full meteorological content of the observations. Only by such procedures is it likely that the complex problems of the formation and dissipation of stratus cloud and advection fog will be unravelled.

DATA ON DIFFUSION.—Turning to a different class of observations of mean properties, O. G. Sutton (1947) has placed on record observations made over a period of years on the behaviour of gas and smoke clouds from point and line sources at ground level in near-adiabatic conditions over downland. They may be summarised as follows:

- (i) The concentration at any point from a continuous point or line source is proportional to :
 - (a) the strength of source (hot or cold sources excepted),
 - (b) the reciprocal of the wind velocity, to a good approximation.
- (ii) The axial concentration from a continuous point source decreases with distance (x) downward from the source as $x^{-1.2}$, and the ground concentration from a continuous line source (infinite across wind) as $x^{-0.2}$. In regard to scale, the maximum concentrations at 100 m. from a source of 1 g. sec.⁻¹ (point source) or 1 g. sec.⁻¹ m.⁻¹ (line source normal to wind) in a wind of 5 m. sec.⁻¹ are 2 mg. m.⁻³ and 31 mg. m.⁻³ respectively.
- (iii) The time-mean (3 min. or greater) half-width of a cloud from a point source at 100 m. distance is 17.5 m., and the height of a point or line source cloud 10 m., both practically independent of wind velocity. (*N.B.*—Half-width and height are defined as distances at which the concentration falls to one-tenth of the maximum value at the given x .) The difference between half-width and height is, of course, to be ascribed to the horizontal crosswind turbulent velocity being greater than the vertical turbulent velocity in the lowest 10 m. or so.

Calder (1948) has supplemented these data which provide valuable material for the test of any theoretical treatment of the problem of turbulent diffusion in the lower atmosphere. But it must be realised that they relate to a particular type of underlying surface, so that, while the formal relations given may be expected to be widely valid, the absolute values are likely to be influenced by the particular surface. Over the sea, for example, the diffusion will normally be less intense, causing smaller cloud widths and heights and higher maximum concentrations.

Of a similar nature are data collected by Gregory (1945) on the dispersion (distribution and deposition) of air-borne spores. The data are rarely as precise as could be wished, but they confirm within their limits the figures given above for gas and smoke. They have the added interest of providing information on material which, because of size, is continuously falling under gravity at a rate comparable with the vertical eddying velocity.

TURBULENT FLUCTUATIONS.—Few observations on the turbulent fluctuations of wind and other quantities have been published during the period under review, and it is the lack of these which, while remaining, is likely to prove a serious hindrance to further understanding of turbulent transfer processes. Sheppard (1947c) summarised information to 1945 on the fluctuations of temperature and humidity, but omitted Nyberg (1939) and Fransilla (1936). Loeser has given a few curves of the frequency distribution of gust accelerations in the mainly "instrumental" paper already referred to, while in an earlier paper (Loeser, 1940) he reports the root mean square velocity fluctuation as varying between 0 and 2 m. sec.⁻¹ at levels from 200 m. to 2500 m., using observations from pilot balloon ascents repeated at 10-minute intervals.

Nyberg has found that the temperature fluctuations during inversions increase as the surface (smooth snow) is approached down to 1 cm. distance, and are still considerable at 1 mm. distance. Fransilla also found a maximum variance at 1 cm. during the night, but at 5 cm. during the day, over grass about 5 cm. long, the amount of the variance at any level being strongly correlated with the numerical value of the temperature gradient. Priestley and Swinbank (1947) have given a few particulars on the type and magnitude of the temperature fluctuations between 4 ft. and 6000 ft. in different synoptic situations. They report a fourfold change in root mean square variation from 0.5° C. to 2.0° C. at 4 ft. in the rear of a gradually-clearing, cold-front sky in the afternoon, 0.5° C. variation at 3700 ft. on a clear summer afternoon with light wind, and almost complete absence of variation between 2000 ft.

and 6000 ft. on an occasion of advective cooling with strong wind and overcast sky. For values of the more significant quantities of the type $\overline{w's'}$ where w' is the vertical eddying velocity and s' the fluctuation from the mean of the entity whose transfer is to be studied, or for values of the spatial and temporal correlations between turbulent velocity components, the meteorologist still waits.

(To be concluded.)

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GENERAL AND PHYSICAL CHEMISTRY. By J. W. SMITH, D.Sc.,
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SOLVENT EFFECTS IN DIPOLE MOMENT MEASUREMENTS.—The usual method for the determination of dipole moments is based upon the Clausius-Mosotti equation

$$P = \frac{(\epsilon - 1) M}{(\epsilon + 2) d} = \frac{4}{3} \pi N \alpha \quad (1)$$

where ϵ , d , and M are the dielectric constant, density, and molecular weight, respectively, of the compound, N is the Avogadro number, and α is the polarisability of the molecule, *i.e.* the induced electric moment produced in each molecule of the compound by a field of unit field-strength. The derivation of this equation is based on the assumption that the molecule is a sphere of radius a , when α has the significance of being equal to a^3 . The term $\frac{4}{3} \pi N \alpha$ has therefore the fundamental significance of being the actual volume occupied by the molecules in 1 g.-mol. of the substance in question.

Hence it is to be anticipated that the product $\frac{\epsilon - 1}{\epsilon + 2} \cdot \frac{M}{d}$, which is

known as the Molecular Polarisation, will be independent of the physical state of the substance, and indeed for symmetrical molecules it has been found to be very nearly the same for the solid, liquid, and gaseous states. Also, for such symmetrical molecules ϵ should be equal to n^2 , where n is the refractive index measured at the same wavelength. In such cases P can be conveniently regarded as comprising two factors, P_E and P_A , arising from the displacement of electrons and nuclei, respectively, by the electric field. At radio frequencies such as are now normally used for dielectric constant measurements both these displacements attain equilibrium values, but, as nuclei cannot undergo significant displacement during the period of alternation of electric fields of optical frequency, the only contributory term is P_E which can be identified

with the molecular refraction R given by $\frac{n^2 - 1}{n^2 + 2} \cdot \frac{M}{d}$. In most cases

P_A is a small term amounting to about 5 per cent. of P_E .

For unsymmetrical molecules, however, ϵ is much greater than n^2 , and for such cases Debye showed that α is enhanced by the term $\mu^2/3kT$, where k is the Boltzmann constant, T the absolute temperature, and μ the permanent electric moment or dipole moment of the molecule. This adds a third polarisation term P_μ ,

the orientation polarisation due to the presence of a permanent dipole, so the equation in this case becomes

$$P = P_E + P_A + P_\mu = \frac{(\epsilon - 1)}{(\epsilon + 2)} \cdot \frac{M}{d} = \frac{4}{3} \pi N \left(\alpha + \frac{\mu^2}{3kT} \right) \quad (2)$$

The derivation of this relationship, however, involves the assumption that the molecules are sufficiently far apart to prevent any interaction between them. This restricts the relationship to gases and strictly speaking to these only at infinitely low pressure. Although the values of μ for a large number of relatively volatile compounds have been determined from their dielectric constants in the gas phase, either by determining the rate of variation of P with $1/T$, which leads directly to the value of $4 N \mu^2 / 9k$, from which μ can be deduced, or by assuming that the $P_E + P_A$ terms are given by R or $1.05R$, this method is extremely restrictive. In 1925, however, Debye pointed out that on the analogy of the kinetic behaviour of a solute in a dilute solution, the equation derived for the molecular polarisation of a substance in the ideal gas state should be applicable to a solute in infinitely dilute solution in a non-polar solvent, provided there is no specific mutual interaction. The form of the equation applicable in such cases should be

$$P_1 f_1 + P_2 f_2 = \frac{(\epsilon - 1)}{(\epsilon + 2)} \cdot \frac{(M_1 f_1 + M_2 f_2)}{d} \quad (3)$$

where the subscripts 1 and 2 refer to the solvent and solute, respectively, and f_1 and f_2 are the mol. fractions of the components in the solution.

The procedure adopted has been to determine P_1 from dielectric constant and density measurements on the pure solvent and to assume that its value is the same in the solutions. Hence, from measurements on the solutions the value of P_2 can be derived for solutions of varying f_2 . As P_2 generally decreases with increasing f_2 , the values of P_2 are plotted against f_2 and the curve extrapolated back, the intercept at $f_2 = 0$ being taken as the molecular polarisation of the solute ($P_{2\infty}$) in the absence of forces of interaction between the dipolar molecules. From this μ can be calculated.

In quite early stages it was observed, however, that the value of $P_{2\infty}$ and hence the value of μ deduced from it, varied with the solvent in which the measurements were made, and in cases where comparison was possible the values differed to some extent from those observed in the gaseous state. The values in solution were sometimes higher ("positive solvent effect") but generally lower than in the gas ("negative solvent effect"). A number of sys-

tematic attempts to correlate the results obtained with different solvents have since been published. Simultaneously attempts were made to relate the change in P_s for a solute with its concentration. This change, and also the fact that the value of P for a dipolar compound is generally lower for the liquid than for the gaseous state, had generally been ascribed to association between the dipolar molecules. However, the change with concentration is different in different solvents, and the manner in which it changes indicates that it cannot be due to association only, since the results for nitrobenzene, for example, would suggest, contrary to expectation, a greater degree of association in solvents of higher dielectric constant (Malsch, *Physikal. Z.*, 1932, **33**, 383; Davy and Sidgwick, *J. Chem. Soc.*, 1933, 281). Indeed Bury and Jenkins (*ibid.*, 1934, 688) pointed out that to accord with Sidgwick's views association should lead to an increase instead of a decrease in polarisation such as has been observed in the case of a number of liquids, such as alcohols, which are known to associate.

Naturally the first step was to devise empirical relations to express the variation of μ or of $P_{s\infty}$ with the dielectric constant of the solvent, and to correlate such values with those for the gaseous state. The first of these was that of Müller (*Physikal. Z.*, 1933, **34**, 689), which can be expressed

$$(P_s - R)_{\text{soln.}} / (P_s - R)_{\text{vapour}} = 1 - k(\epsilon - 1)^2 \quad (4)$$

where the constant k has the value 0.075 ± 0.005 . This implies that as a first approximation $\mu_{\text{soln.}} / \mu_{\text{vapour}}$ is a linear function of $(\epsilon - 1)^2$ and that $\mu_{\text{soln.}}$ is always less than μ_{vapour} . Müller suggested that the intense electric field of a dipolar molecule produces partial saturation of the dielectric in its vicinity, thereby decreasing the polarisability of the solvent. Frank has pointed out, however, that this does not explain why the dielectric constant of the solvent should be the principal parameter determining its effect, or why this saturation does not affect the refractivities, which are almost additive for solutions (*Proc. Roy. Soc.*, 1935, **A152**, 171). The relationship actually holds quite well for a number of compounds but fails completely in other cases, including, of course, all compounds for which the moment in solution is approximately equal to or greater than the value for the gas.

Jenkins (*Nature*, 1934, **133**, 106; *J. Chem. Soc.*, 1934, 480), on the other hand, suggested that $P_{s\infty}$ is a linear function of $1/\epsilon$.

$$P_{s\infty} = A \pm B/\epsilon \quad (5)$$

This expression seems to apply over a rather wider ϵ range than Müller's relation, and can be used to represent both positive and

negative solvent effects, but extrapolation to $\epsilon = 1$ generally leads to values much above the observed P_s value for the gas phase.

A rather different empirical relation was that of Le Fèvre (*J. Chem. Soc.*, 1935, 773)

$$P'_\mu/P''_\mu = k(\epsilon'' + 2)/(\epsilon' + 2) \quad . \quad . \quad (6)$$

where P'_μ and P''_μ are the orientation polarisations of a solute in media of dielectric constant ϵ' and ϵ'' , respectively. k is nearly unity for some substances, but is uncertain in other cases, and no provision is made for a positive solvent effect.

During the same period attention was also directed to the variation of P_s for various concentrations of solute with the dielectric constant of the solution at each concentration. The earliest and most successful approach to this subject was that of Sugden (*Nature*, 1934, 133, 415), who noticed that for a number of solutes including nitrobenzene, benzonitrile, chlorobenzene, and heptyl bromide, P_s decreased almost linearly with the volume polarisation [$\Pi = (\epsilon - 1)/(\epsilon + 2)$] of the solution. In a number of cases studied the slope of the $P_s - \Pi$ line was equal to P_μ for the solute, but extrapolation of this line to $\Pi = 0$ (i.e. $\epsilon = 0$) did not lead to the value of P_s for the gaseous state. Hence Sugden wrote his relation

$$P_s = \alpha + P_{s0} - P_\mu(\epsilon - 1)/(\epsilon + 2) \quad . \quad . \quad (7)$$

where P_{s0} is the total polarisation deduced from vapour measurements, and α is a small constant which varies both with the solute and solvent, although in the case of nitrobenzene its value is almost the same for a number of solvents, so that the $P_s - \Pi$ curves for these solvents superimpose upon one another. It follows, therefore, that when this occurs the values of P_{s0} also vary linearly with the values of Π for the pure solvent.

Measurements by Fairbrother (*J. Chem. Soc.*, 1934, 1846) on nitrobenzene in *p*-xylene and decane appeared to confirm this relationship, but Jenkins and Sutton (*ibid.*, 1935, 609) found that in some cases the values of P_μ deduced from it differed appreciably from those observed in the gas phase.

A very similar relation, and, indeed, one which can be derived from it, was deduced independently by van Arkel and Snoek (*Trans. Faraday Soc.*, 1934, 30, 707). Assuming that the ideal mixture law held in all the solutions they express their relationship in terms of volume polarisations, and it can be written

$$\Pi_s = \Pi_{\text{opt.}} + \Pi_0/(1 + cV_s\Pi_0) \quad . \quad . \quad (8)$$

where Π_0 is the orientation polarisation of the solute at infinite

dilution, V , the volume fraction of solute present, Π_{opt} , the optical volume polarisation of the solute, and c is a constant.

These approaches to the matter were completely empirical, but various attempts to treat the subject theoretically have also been made during the past 15 years. By consideration of the field around a dipolar molecule, Weigle (*Helv. Phys. Acta*, 1933, 6, 68) concluded that moments acting in the same direction as the moment of the dipole and therefore modifying the latter will be produced by the orientation of anisotropic solvent molecules in the dipole field and by the polarisation of the medium by the dipolar solute molecule. Under certain assumptions these effects are given by the first and second terms, respectively, of the expression

$$\Delta\mu = \frac{A\mu^2}{a^3} \cdot \frac{(\alpha' - \alpha'')}{kT} + B\mu \cdot \frac{(\alpha' + 2\alpha'')}{3} \quad (9)$$

where μ is the moment of the solute molecule, assuming it to be spherical, a its radius, and α' and α'' are the polarisabilities along the two principal axes of the solvent molecule, which is assumed to be an ellipsoid of rotation. A and B are factors which depend on the shape of the solute molecule. As the first term involves $\alpha' - \alpha''$ it is small and does not normally account for values of $\Delta\mu$ greater than about 1 per cent. of μ . It is negative for a spherical molecule. On the other hand, for a spherical molecule the second term is zero, but under other conditions its magnitude and sign depend on the shape of the molecule and it can be much greater than the first term.

Glasstone has pointed out (*Ann. Rep. Chem. Soc.*, 1936, 33, 121) that if $\alpha' - \alpha''$ is small this relation reduces to the form $\Delta\mu = B\mu\alpha_1$, where α_1 is the mean polarisability of the solvent molecules, and for a non-polar substance this is given by $\frac{(\epsilon - 1)}{(\epsilon + 2)} \cdot \frac{M}{\alpha} / \frac{4\pi N}{3}$. Hence $\mu_{\text{soln.}}/\mu_{\text{vapour}} = 1 + C(\epsilon - 1)/(\epsilon + 2)$, an expression which follows from the Sugden relation.

The theory was developed by Higasi, who pointed out (*Bull. Inst. Phys. Chem. Res. Tokyo*, 1934, 13, 1167) that the second term in Equation (9) predicts negative and positive solvent effects for molecules elongated along the dipole axis and perpendicular to this axis, respectively. In confirmation of this inference the dipole moments of molecules with negative Kerr constants generally have dipole moments in solution greater than are observed for the vapour state, indicating their greater optical polarisability in the direction at right-angles to the axis of the dipole.

In another development of Weigle's treatment, Frank (*Proc. Roy.*

Soc., 1935, A152, 171) deduces that the value of μ measured in a solvent of dielectric constant ϵ differs from the gas value by an amount given by

$$\Delta\mu = (A' + A'')_{\mu} \cdot \frac{(\epsilon - 1)}{\epsilon} \quad (10)$$

where A' is a constant characteristic of the geometrical shape of the solute molecule and for which he developed a method of calculation. The constant A'' depends on the external boundary conditions, but its value was regarded as being small. Actual plots of the values of μ in various solvents against $1/\epsilon$ do fall approximately on straight lines, as had been previously indicated by Jenkins, but the best straight lines extrapolate to values of μ at $1/\epsilon = 1$ which are much greater than the observed values for the gas phase. Also the values obtained in carbon disulphide and in polar solvents fall below the line found for non-polar solvents of lower dielectric constant. The weakness of such relations as this is that non-polar solvents the values of $1/\epsilon$ range only from about 0.53 to 0.38, and hence over such a narrow band the variation may appear linear, though more careful examination shows that in the majority of cases the linearity is far from perfect.

Higasi's further development (*Sci. Pap. Inst. Phys. Chem. Res. Tokyo*, 1936, 28, 284) was to apply the relationship $\Delta\mu = \alpha_1 E$, where α_1 is the polarisability of the solvent molecule and E the field strength. From this he obtained $\Delta\mu = \mu A(\epsilon - 1)/(\epsilon + 2)$, or

$$\mu_{\text{soln.}}/\mu_{\text{vapour}} = 1 + A(\epsilon - 1)/(\epsilon + 2) \quad (11)$$

which is identical with the expression derived by Glasstone directly from Weigle's treatment. The sign and value of A depend on the shape of the solute molecule; if spherical it is zero, but if it can be represented as an ellipsoid of rotation with radius a' along its axis of symmetry and radius a'' at right-angles to this axis A has a finite value. If the dipole is at the centre of the molecule and directed along its axis of symmetry there are two cases according as a' is greater or less than a'' . For the first case A is given by

$$A = -\frac{1}{k^2 - 1} \left(1 - \frac{k}{\sqrt{k^2 - 1}} \ln(k + \sqrt{k^2 - 1}) - \frac{1}{2} \right) \quad (12)$$

where $k = a'/a''$. As in this case k is greater than unity, A is always negative, so that for molecules of this type the moment in solution is less than in the vapour state. For the second case

$$A = -\frac{1}{k^2 - 1} \left(1 - \frac{k}{\sqrt{1 - k^2}} \sin^{-1} \sqrt{1 - k^2} - \frac{1}{2} \right) \quad (13)$$

and as k is less than unity A and hence the solvent effect are both positive.

When the dipole is not at the centre of the molecules, the problem is more complex as A is given by a combination of functions of the type of (12) and (13), and hence the solvent effect may be either positive or negative. The main difficulty in applying the theory arises in the calculation of the dimensions of the molecule, which must be considered as an ellipsoid of rotation, but in some cases this can be derived approximately from a knowledge of the atomic radii and stereochemical considerations or from the value of the Kerr constant.

Quite a different approach to the matter was made by Debye (*Physikal. Z.*, 1935, 36, 100) who suggested that the local field acting on a molecule as a result of the presence of neighbouring molecules can be regarded as directed along an axis which, owing to the quasi-crystalline structure of liquids, moves slowly compared with the thermal motion of liquids and independently of the external field applied during the measurements. The local field therefore gives rise to another energy term, in addition to the thermal agitation, which tends to minimise the orientation of dipolar molecules by the externally applied field. The value of P_μ therefore becomes the product of $4\pi N\mu^2/9kT$ and a factor dependent on E/kT , where E is the coupling energy between the solvent and solute molecules preventing rapid rotation. Such an interpretation of the solvent effect, however, has the weakness that it predicts that the dipole moment in solution will always be less than in the gas phase, whilst it gives no reason for the undoubted fact that solvent effects are greatest in solvents of higher dielectric constant.

A fundamental development which for some time received insufficient consideration was that of Raman and Krishnan (*Proc. Roy. Soc.*, 1928, A117, 589) who pointed out that most actual molecules are not optically isotropic, i.e. equally polarisable in all directions, and hence equations (1) and (2) are not strictly applicable. For the case of a pure liquid consisting of anisotropic molecules they derived a relation which can be written in the form

$$P = \frac{\epsilon - 1}{\epsilon + 2} \cdot \frac{M}{d} = \frac{4\pi N}{3} \left(\frac{\Sigma \alpha}{3} + \frac{\mu^2}{3kT} \right) + \frac{\epsilon - 1}{\epsilon + 2} N \left(\Psi + \frac{\Theta}{3kT} \right). \quad (14)$$

where $\Sigma \alpha$ is the sum of the polarisabilities $\alpha' + \alpha'' + \alpha'''$ along the three axes, and Ψ and Θ , dependent on the geometry of the molecule, involve the effect of anisotropy on the induced and orientation polarisations respectively. The significance of this theory in connection with the calculation of dipole moments, and the similarity

of form between this equation and that of Sugden (equation (7)), was first pointed out by Goss (*J. Chem. Soc.*, 1933, 1341; 1934, 696) whose empirical developments will be discussed later.

The theory was extended to liquid mixtures by Subbaramaiya (*Proc. Indian Acad. Sci.*, 1934, **A1**, 355) who deduced that as a first approximation

$$P = \frac{\varepsilon - 1}{\varepsilon + 2} \cdot \frac{M_1 f_1 + M_2 f_2}{d} = f_1 \cdot \frac{4\pi N}{3} \left(\frac{\Sigma \alpha_1}{3} + \frac{\mu_1^2}{3kT} \right) + f_1 \cdot \frac{\varepsilon - 1}{\varepsilon + 2} \cdot N \left(\psi_1 + \frac{\Theta_1}{3kT} \right) \\ + f_2 \cdot \frac{4\pi N}{3} \left(\frac{\Sigma \alpha_2}{3} + \frac{\mu_2^2}{3kT} \right) + f_2 \cdot \frac{\varepsilon - 1}{\varepsilon + 2} \cdot N \left(\psi_2 + \frac{\Theta_2}{3kT} \right) \quad (15)$$

Rau has pointed out that when one of the components is non-polar the expression can be simplified (*ibid.*, 1935, **A1**, 498) by omission of the μ_1 and Θ_1 terms. Writing Π for $(\varepsilon - 1)/(\varepsilon + 2)$, etc., this leads to the following relation for the true value of P_2

$$P_2 = \frac{1}{f_2} \left(\frac{\Pi M}{d} - f_1 \cdot \frac{\Pi_1 M_1}{d_1} \right) - N \Psi_1 \frac{f_1}{f_2} (\Pi - \Pi_1) - \Pi N \left(\psi_2 + \frac{\Theta_2}{3kT} \right),$$

and hence P_2 is related to the apparent value $P_{2\infty}$ at infinite dilution by the expression

$$P_2 = P_{2\infty} - N \Psi_1 \frac{3x\varepsilon_1}{(\varepsilon_1 + 2)^2} - \Pi_1 N \left(\psi_2 + \frac{\Theta_2}{3kT} \right) \quad (16)$$

where x is the value of $d\varepsilon/df_2$ at $f_2 = 0$.

Rau suggested that if his expression were written in the form

$$P_2 = \frac{1}{f_2} \left(\frac{\Pi M}{d} - \frac{f_1 \Pi_1 M_1}{d} - N \Psi_1 \Pi_1 - f_1 \Pi_1 \right) - \Pi N (\psi_2 - \psi_1) - \Pi \frac{N \Theta}{3kT}$$

the first term is closely equivalent to the value of P_2 calculated in the usual manner, since $N \Psi_1 (\Pi - f_1 \Pi_1)$ will only be a small correction term. Similarly $\psi_2 - \psi_1$ will also be a small factor and the second term containing it will be small corresponding to the α of Sugden's relation, whilst Θ is of the order of $4\pi\mu^2/3$ and hence the last term corresponds to P_μ . As the other empirical relationships are all closely related to that of Sugden this theory therefore gives a theoretical basis for these also.

Further applications of the Raman and Krishnan theory have been made by Goss, generally in relation to solutions in carbon tetrachloride, which was chosen as an ideal solvent on account of its isotropic character. For the system carbon tetrachloride—nitrobenzene, chosen because it covers a wide dielectric constant range, it was found (*J. Chem. Soc.*, 1935, 502) that the simple formula of the Sugden type required modification, and Goss suggested that

a close approximation to the experimental data is given by the expression

$$P_2 = P_{2 \text{ vapour}} - B(\Pi + \Pi^2) \quad (17)$$

Examination of the actual experimental data for this system reveals, however, that up to values of Π of about 0.7 the value of P_2 is much more linear with Π than is indicated by this relation, but at higher values of Π it changes more or less abruptly towards the value for the pure liquid. This relation, also, was found not to be generally applicable to mixtures of other liquids, and Goss suggested that this was due to the fact that P_2 is normally calculated on the assumption that P_1 is constant, which will generally not be the case (*ibid.*, 1937, 1915).

Hence Goss has applied the intercept method first suggested in connection with this problem by Orr and Butler (*Nature*, 1932, 130, 930). If the values of P are plotted against f_2 , and a tangent is drawn to the curve at any point corresponding to $f_2 = x$, then the intercepts on axes corresponding to $f_2 = 0$ and $f_2 = 1$ are taken as giving the true values of P_1 and P_2 , respectively, at $f_2 = x$. By this method he has deduced P_1 and P_2 data for the solutions of a number of solutes in carbon tetrachloride, and he finds that the values of the effective molecular polarisations P_e of each component can be expressed in the form

$$P_e = P_E + P_A + Z\Pi^4 + Y/\epsilon \quad (18)$$

Y and Z are evidently the values of $P_e - (P_E + P_A)$ when $1/\epsilon$ is 0 and 1, respectively, i.e. when ϵ is ∞ and 1. The term $Z\Pi^4$ is taken as an additional term in the distortion polarisation and Y/ϵ as the orientation polarisation. When $\epsilon = \epsilon_1$ Goss considers that the second term of equation (18) will be negligible, and hence Y/ϵ_1 is approximately equal to $P_\infty - (P_E + P_A)$.

Goss also finds that within experimental error

$$(Y/P_\mu - 1)/[\sqrt[4]{(Z + P_E + P_A)/(P_E + P_A)}] - 1 = V \quad (19)$$

where V , the intercept ratio, has an average value given initially as 3.25 but later amended to about 3. From this it follows that

$$P_0 = Y/3 \sqrt[4]{\frac{Z + P_E + P_A}{P_E + P_A}} - 2. \quad \text{As } Y/P_\mu \text{ is nearly the product}$$

of ϵ_1 and the ratio $(P_\infty - P_E - P_A)/P_\mu$, which Higasi had found to be connected with the relationship between the direction of the dipole and the magnitude of the molecular axes. $Y/\epsilon_1 P_\mu$ and $Z/(P_E + P_A)$ are regarded as expressing the extent to which the orientation and distortion polarisation, respectively, are modified by induction, the latter being attributed to the effects of the reaction

field and the former to the moments induced in the polar molecules by other permanent dipoles. By utilising this expression Goss has deduced values for P_μ from data on carbon tetrachloride solutions which lead to values of μ closely in accord with values observed for the gas.

Later Goss (*J. Chem. Soc.*, 1940, 752) pointed out that $Y/\epsilon P_\mu$ is a linear function of the ratio $K = 2b'/(b'' + b''')$, where b' is the dimension of the polar axis of the ellipsoid molecule and b'' and b''' are the other axes, and that K is given by $K = 4 - 2.85Y/\epsilon_1 P_\mu$ and hence from the intercept relation

$$K = 6.55 - 3.82 \sqrt{(Z + P_E + P_A)/(P_E + P_A)}.$$

Attractive though this approach is, and in spite of the apparent success which it has had in permitting the derivation from solution data of moments almost identical with the values observed for the gas, it is, in the opinion of the writer, based upon insecure foundations. If the intercept method is correct, then Goss's results indicate that not only the values of P_2 for various solutes but also the value of P_1 for the carbon tetrachloride solvent varies with the ϵ of the solution. This is the very circumstance under which the value of P_2 at infinite dilution, obtained by the usual extrapolation method, is incorrect. This was shown in the relation of Rau (equation 16) but is frequently ignored, and, in fact, it is often stated quite explicitly that the change of P_1 with ϵ is eliminated when data are extrapolated to infinite dilution. This may be shown very simply as follows. Values of P_2 at finite concentrations are derived on the basis of the relation $P = P_1 f_1 + P_2 f_2$, where P_1 is taken as the value for the pure solvent. Hence $P_2 = (P - P_1)/f_2$. Now if both P_1 and P_2 really vary with ϵ , and hence with f_2 , and have actual values P_1^0 and P_2^0 at $f_2 = 0$, then at a finite concentration f_2 we have $P_1 = P_1^0 + \int_0^{f_2} \frac{dP_1}{df_2} \cdot df_2$; $P_2 = P_2^0 + \int_0^{f_2} \frac{dP_2}{df_2} \cdot df_2$, and therefore $P_2^0 + \int_0^{f_2} \frac{dP_2}{df_2} = P_1^0 + \int_0^{f_2} \frac{dP_1}{df_2} + (P - P_1^0 - \int_0^{f_2} \frac{dP_1}{df_2} \cdot df_2)/f_2$. As f_2 approaches zero this becomes $P_2^0 = P_1^0 + \lim_{f_2 \rightarrow 0} \frac{P - P_1^0}{f_2} - dP_1/df_2$. Hence the apparent value of $P_{2\infty}$ derived in the usual way is not P_2^0 but $P_2^0 + dP_1/df_2$.

Similar arguments also apply with regard to the intercept method. If $P = P_1 + f_2(P_2 - P_1)$, then

$$dP/df_2 = P_2 - P_1 + f_2(dP_2/df_2) + f_1(dP_1/df_2),$$

and hence the intercept at $f_2 = 0$ is

$$\begin{aligned} P - f_2(dP/df_2) &= P - (P_2 - P_1)f_2 - f_2\{f_2(dP_2/df_2) + f_1(dP_1/df_2)\} \\ &= P_1 - f_2\{f_2(dP_2/df_2) + f_1(dP_1/df_2)\} \end{aligned}$$

Similarly the intercept at $f_2 = 1$ is $P_2 + f_1\{f_2(dP_2/df_2) + f_1(dP_1/df_2)\}$. Hence the intercept method only gives correct values for P_1 and P_2 if $f_2(dP_2/df_2) + f_1(dP_1/df_2) = 0$. At $f_2 = 0$ the value of P_1 is, of course, correct, but the value of P_2 so derived is really $P_2 + dP_1/df_2$, as derived for the usual extrapolation method.

The advantage of using carbon tetrachloride is that it is an isotropic solvent, and so, according to the Raman and Krishnan theory, its polarisation should not vary with ϵ . Hence extrapolated values of $P_{1\infty}$ in this solvent should be correct, but the intercept method, assuming variation of both P_1 and P_2 with f_2 , is incorrect. For non-isotropic solvents it must be assumed that neither procedure is correct.

In quite a different direction various attempts have been made to modify the Debye treatment. In Debye's own development, referred to previously, the Lorenz expression for the local field acting on the molecule, *viz.* $F = E + 4\pi I$, where I is the induced moment per unit volume, was retained in principle, but was modified to introduce the hindering effect exerted by each molecule on the rotation of its neighbours relative to itself. Other developments along these lines were also suggested by Fowler (*Proc. Roy. Soc.*, 1935, **A149**, 1), Kubo (*Sci. Pap. Inst. Phys. Chem. Res. Tokyo*, 1935, **27**, 295), and Bauer (*Cahiers de Physique*, 1944, **20**, 1; **21**, 37; 1945, **27**, 33; *cf. Trans. Faraday Soc.*, 1946, **42A**, 12).

On the other hand, whilst the Lorenz field holds well for non-polar materials it fails in many respects for polar liquids, for instance it predicts an electric Curie Point for many substances, *i.e.* a temperature at which the dielectric constant becomes infinitely great. Such an effect has never been observed. Consequently other treatments have been developed which have taken the matter back to more drastic modifications in the fundamental theory. Thus Guggenheim (*Nature*, 1936, **137**, 459) suggested that the conditions under which μ is normally measured in solution, *i.e.* at high dilution in non-polar solvents, are those under which it is possible to avoid the Clausius-Mosotti equation. By considering a spherical solute molecule and regarding the solvent as a continuous medium he approached the problem from the same basis as the Debye-Hückel theory, obtaining the relationship

$$\frac{(\epsilon - \epsilon_1) - (n^2 - n_1^2)}{4\pi} = \frac{4\pi\mu^2}{3kT}$$

where ν is the number of solute molecules per c.c. of solution, μ being defined as the total electric moment between the plates of a large parallel plate condenser filled with solvent and containing a single solute molecule with its polar axes normal to the plates.

His model indicates that μ varies from one solvent to another according to the relation $\mu(\epsilon_1 + \epsilon_2/2) = \text{const.}$, where $(\epsilon_2 - 1)/4\pi$ is the volume polarisability of the solute sphere. On applying his relationships Guggenheim found that neither relation was obeyed exactly.

Onsager, on the other hand, by taking into consideration the reaction field as distinct from the cavity field treated by Lorenz and Debye, has deduced that for pure liquids

$$\frac{(\epsilon - n^2)(2\epsilon + n^2)}{\epsilon(n^2 + 2)^2} = 4\pi N\mu_0^2/9kT$$

where μ_0 is the moment in the vapour state, whilst for dilute solutions of a polar solute in a non-polar solvent

$$n^2 = \frac{4\pi N_2}{3kT} \mu^2 \left[\frac{n_1^2(n_2^2 + 2)}{2n_1^2 + n_2^2} \right]$$

where ν_2 is the number of solute molecules in unit volume. Further, the relationship between the moment in solution and in the vapour state is given by

$$\mu_{\text{soln.}} = \frac{(2n_1^2 + 1)(n_2^2 + 2)}{3(2n_1^2 + n_2^2)} \mu_{\text{vapour.}}$$

Like the modified theory of Debye, Guggenheim and Onsager's treatments, both of which apply to spherical molecules only, suggest that the solvent effect should be negative, whereas other theories all suggest that it should be zero.

Amongst other developments, a theory claimed to be formally exact and applying over a larger range than the Onsager relation was evolved by Kirkwood (*J. Chem. Phys.*, 1939, 7, 911); based upon classical statistical mechanics. The theory takes account of both the electrostatic hindering of rotation, and also hindering due to short-range intermolecular forces. This theory leads to the following relation for the dielectric constant of a liquid

$$\frac{(2\epsilon + 1)(\epsilon - 1)}{9\epsilon} = \frac{4\pi N}{3v} [\alpha + \mu\bar{\mu}/3kT]$$

where $\bar{\mu}$ is the sum of the molecular dipole moment and the moment induced as the result of hindered rotation by a representative molecule of fixed orientation in the spherical region around it of radius large relative to the range of intermolecular forces. This is very similar to Onsager's equation but it cannot be used to

calculate μ without further assumptions because $\bar{\mu}$ is an unknown function of temperature. However, the theory has been applied using the rough quasi-crystalline models of local liquid structure, and for the cases of water and alcohols Oster and Kirkwood (*J. Chem. Physics*, 1943, **11**, 175) have derived relationships which lead to dielectric constants of the actual order observed.

The Debye and Onsager approaches have been compared critically by several workers. Fröhlich and Sack (*Proc. Roy. Soc.*, 1944, **A182**, 388; cf. *Trans. Faraday Soc.*, 1946, **42A**, 3) conclude that both are open to criticism in that Debye's assumption of the Lorenz field implies that the dipole rotates so fast that the polarisation cannot follow, and hence the moment can be replaced by its average value, whereas Onsager makes the extreme assumption in the reverse direction that the reaction field follows the dipole. They have developed a treatment of which the Onsager expression is a first approximation. A second approximation leads to results intermediate between those of Debye and Onsager. The complete relation, however, converges only if $\mu F/kT$ is less than unity, where F is the equilibrium reaction field which would exist if the dipole were fixed. Hence they infer that Onsager's theory should hold only if ϵ is less than 9.

An approach from the basis of recalculation of the internal field has also been made by Böttcher (*Physica*, 1942, **9**, 937, 945; *Rec. trav. chim.*, 1943, **62**, 325, 503; 1945, **64**, 47; 1946, **65**, 14, 19, 39, 50).

Frank has pointed out (*Trans. Faraday Soc.*, 1946, **42A**, 20) that equation (1) is well proved for non-polar molecules, but when dipoles are present the factors which have to be considered may be summarised

- (a) the field applied for measurement;
- (b) the field due to the permanent moment of the central molecule which polarises the surrounding medium and thereby produces a reaction field on the central molecule;
- (c) the resultant field arising from neighbouring polar molecules which will not be zero even when the central atom is non-polar;
- (d) non-electrostatic forces restricting molecular rotation.

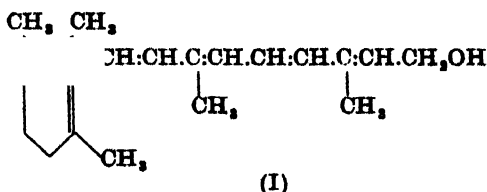
The defect of Debye's theory is that he disregarded (b), whilst Onsager disregarded (d) and also the dielectric saturation, which is the essence of Debye's theory.

The position is still not clarified. No present theory is complete and this is not to be wondered at considering the complexity of the

problem. This circumstance, however, detracts little from the fact that dielectric behaviour has proved and will continue to prove a powerful weapon in studying chemical problems. On the other hand, it does indicate that care must be taken not to push such considerations too far, as was done, it would now appear, a few years ago when such quantities as valency angles were deduced from the results of dipole moments measured in solution.

ORGANIC CHEMISTRY. By A. W. JOHNSON, Ph.D., A.R.C.S., The University, Cambridge.

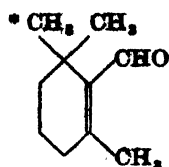
THE SYNTHESIS OF VITAMIN A AND DERIVED PRODUCTS.—The structure of vitamin A (I), first proposed by Karrer, Morf and Schöpp (*Helv. chim. Acta*, 1931, **14**, 1431), was definitely confirmed by the synthesis of perhydrovitamin A (Karrer and Morf, *ibid.*, 1933, **16**, 625) which was shown to be identical with the hydrogenated natural vitamin.



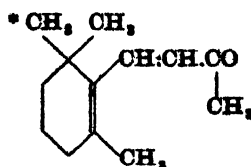
The synthesis of the vitamin itself has presented many difficulties, but during the last two years there have been three independent detailed accounts describing the preparation of vitamin A and a fourth announcement (no details as yet) claiming a further synthesis. In addition, methods are now available for the synthesis of the corresponding esters and ethers, as well as vitamin A aldehyde, acid and acid esters. Previous reviews of synthetic work in this branch of the polyene field include Heilbron, Jones and Bacharach, "Vitamins and Hormones," Vol. II, p. 155 (New York, 1944); Milas, *ibid.*, Vol. V, p. 1 (New York, 1947); Jones, *Ann. Rep. Chem. Soc.*, 1941, **38**, 180; Sobotka and Bloch, *Chem. Reviews*, 1944, **34**, 435.*

There is a fairly wide choice of starting materials for any projected synthesis of vitamin A for citral, β -cyclocitral (II), Ψ -ionone and β -ionone (III) are all comparatively readily available and it will be apparent that β -ionone contains 13 of the requisite 20 carbon atoms, together with a reactive centre for building up the remaining 7 carbon atoms of the chain.

* Added in proof: An extensive review of the synthesis of vitamin A and related products has recently been given by Sir Ian Heilbron in his Pedler lecture to the Chemical Society (*J.*, 1948, 396).



(II)

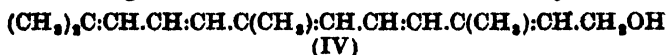


(III)

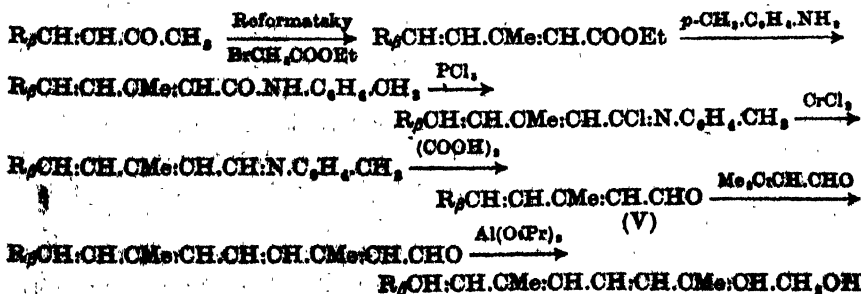
* The 2:2:6-trimethyl-6-cyclohexenyl rings will subsequently be designated R_β , i.e. β -ionone becomes $R_\beta\text{CH:CH.CO}$



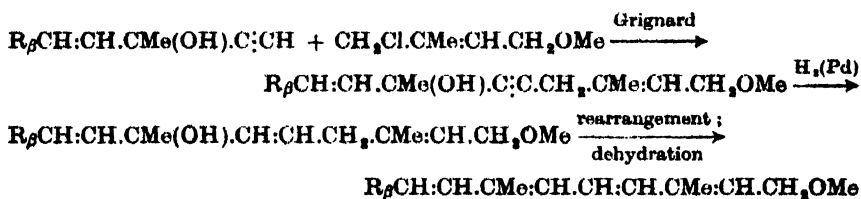
Syntheses based on β -Cyclocitral.—The earliest claim to have synthesised vitamin A was that of Fuson and Christ (*Science*, 1936, 84, 294) by condensation of two equivalents of β -methylcrotonaldehyde with β -cyclocitral and subsequent reduction, but the product was characterised only by absorption spectra data. Heilbron and Jones (*Chem. and Ind.*, 1936, 55, 813) showed that the properties described would also be expected of farnesinol (iv) formed by the reduction of the product of self-condensation of three moles. of β -methylcrotonaldehyde, and indeed β -cyclocitral has not been induced to undergo aldol condensations with aldehydes.



Syntheses based on β -Ionone.—In the following year, Kuhn and Morris (*Ber.*, 1937, 70, 853; G.P., 696,084, *Chem. Abs.*, 1941, 35, 6394; U.S.P., 2,233,375; 2,239,491, *Chem. Abs.*, 1941, 35, 3774, 4918) announced the synthesis of a biologically active material estimated to contain 7.5 per cent. of the vitamin, although, in view of the more recent isolation of pure crystalline vitamin A and consequent revision of standards of biological activity, the vitamin content of Kuhn's product was probably considerably less than 7.5 per cent. The steps involved were the conversion of β -ionone to a C_{12} aldehyde, β -ionylideneacetaldehyde (v), and thence by condensation with β -methylcrotonaldehyde to vitamin A aldehyde which gave vitamin A by reduction with aluminium isopropoxide:



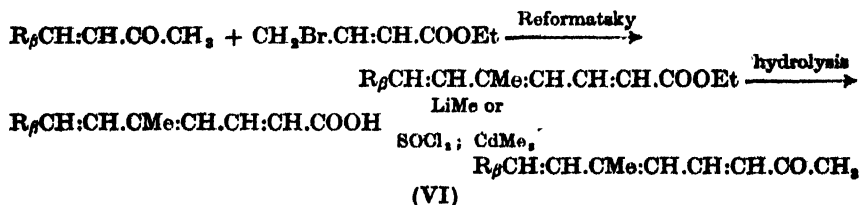
condensation of 6-methoxy-4-methyl-2:4-hexadienyl bromide, $\text{CH}_2\text{Br}:\text{CH}:\text{CH}:\text{CMe}:\text{CH}:\text{CH}_2\text{OMe}$, and β -ionone in the presence of lithium, but in the absence of biological results and absorption spectrum or analysis of the product, this claim can be accepted only with considerable reserve. Milas and McAlevy (*J. Amer. Chem. Soc.*, 1935, **57**, 580) had previously described the Grignard condensation of α -ionone and 4-methyl-2:4-hexadienyl bromide, $\text{CH}_2\text{Br}:\text{CH}:\text{CH}:\text{CMe}:\text{CH}:\text{CH}_2$, but the structure of the final product was not definitely established. More recently Oroschnik (*J. Amer. Chem. Soc.*, 1945, **67**, 1627) has claimed the synthesis of vitamin A methyl ether by the following route starting from the ethynylcarbinol of β -ionone (cf. Oroschnik, U.S.P., 2,425,201; *Chem. Abs.*, 1948, **42**, 584; Davies, Heilbron, Jones and Lowe, *J.C.S.*, 1935, 584; Gould and Thompson, *J. Amer. Chem. Soc.*, 1935, **57**, 340; Karrer and Benz, *Helv. chim. Acta*, 1948, **31**, 390):



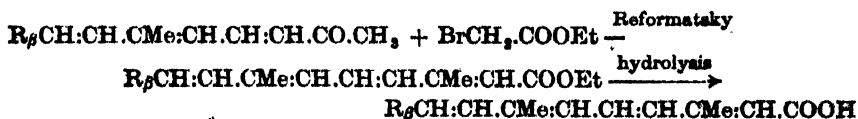
The final product was separated into two fractions by vacuum distillation, but no biological results were quoted for either fraction and no degradative evidence was adduced to support their identity. The absorption spectra of the products indicated that at best they were still very impure and the failure of other workers (Isler, Huber, Ronco and Kofler, Jubilee Volume to Emil Barrell, Basel, 1946, p. 31) to repeat the synthesis renders further confirmation of the claim desirable (see also Cymerman, Heilbron, Jones and Laeey, *J.C.S.*, 1946, 500).

In the last two years, however, successful syntheses of vitamin A and the corresponding aldehyde, acid and esters have been elaborated by the Dutch workers, Arens and van Dorp (*Nature*, 1946, **157**, 190; 1947, **160**, 189; *Rec. Trav. chim.*, 1946, **65**, 338) using β -ionone as the starting material. A Reformatsky reaction of β -ionone with γ -bromocrotonic ester, followed by hydrolysis and dehydration gave a C_{17} ester in some 15 per cent. yield (cf. a German patent application by Ziegler referred to in *Rec. Trav. chim.*, 1946, **65**, 338) and the corresponding C_{17} acid, m.p. 160° (155.5 – 157.5°) was converted to a C_{18} methyl ketone either by the direct action of lithium methyl or by the action of cadmium dimethyl (Heilbron,

Jones and O'Sullivan, *Nature*, 1946, **157**, 485; *J.C.S.*, 1946, 866) or zinc methyl iodide (Karrer, Jucker and Schick, *Helv. chim. Acta*, 1946, **29**, 704) on the C_{17} acid chloride. The C_{18} ketone (VI) was a pale yellow oil, characterised by absorption spectrum, $\log \Sigma_{\max}$. 3350 Å. = 4.58 and semicarbazone m.p. 191° (188.5–189.5°), $\log \Sigma_{\max}$. 3490 Å. = 4.69, and was the keystone of all of the above syntheses.



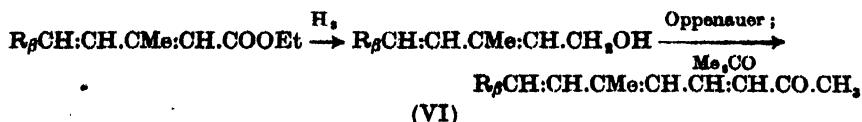
A further Reformatsky reaction of (VI) with bromoacetic ester gave vitamin A acid ester which was hydrolysed to the corresponding acid which formed yellow crystals, m.p. 181.5° ($\log \Sigma_{\max}$. 3470 Å. = 4.64). When dissolved in pea-nut oil, this acid had an activity of about one-tenth that of vitamin A, although an aqueous solution of the sodium salt had about half of the activity of vitamin A.



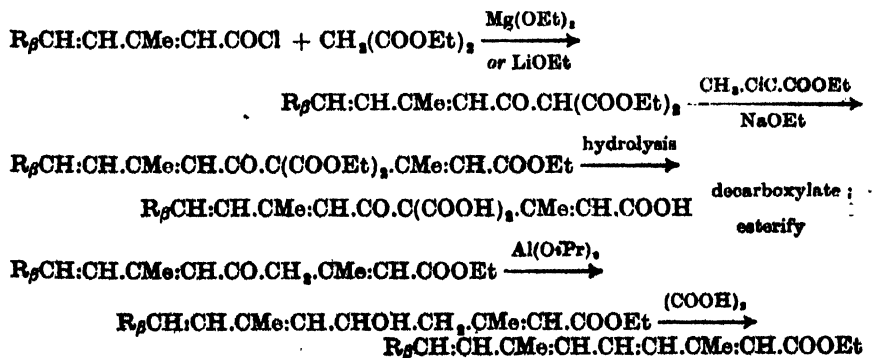
Further treatment of vitamin A acid with lithium methyl gave a C_{21} ketone which was also biologically active (Arens and van Dorp, *Nature*, 1946, **157**, 190). The preparation of vitamin A acid has been successfully repeated by Heilbron, Jones and O'Sullivan (*loc. cit.*) and by Karrer, Jucker and Schick (*loc. cit.*). Reformatsky reactions could not be effected between methyl ω -bromosorbate and simple ketones (Heilbron, Jones and O'Sullivan, *loc. cit.*; Karrer and Schwyzer, *Helv. chim. Acta*, 1946, **29**, 1191); such a reaction with β -ionone would have given the methyl ester of vitamin A acid directly.

An alternative synthesis of the C_{18} ketone (VI) has been published by Milas and Harrington (*J. Amer. Chem. Soc.*, 1947, **69**, 2247). β -Ionylideneacetic ester, formed by a Reformatsky reaction of β -ionone and bromoacetic ester (Karrer *et al.*, *Helv. chim. Acta*, 1932, **15**, 878; 1933, **16**, 557; see also Sobotka, Bloch *et al.*, *J. Amer. Chem. Soc.*, 1943, **65**, 1961; 1945, **67**, 403; *Chem. Reviews*,

1944, 34, 435; Young, Andrews *et al.*, *J. Amer. Chem. Soc.*, 1944, 66, 520, 2130) was reduced with lithium aluminium hydride (Finbolt *et al.*, *ibid.*, 1947, 69, 1199; Nystrom and Brown, *ibid.*, 1947, 69, 1197) and gave β -ionylidene-ethyl alcohol (*cf.* Gould and Thompson, *loc. cit.*) which by Oppenauer oxidation and simultaneous condensation with acetone (Batty, Heilbron *et al.*, *J.C.S.*, 1938, 175) gave the C_{18} ketone (vi), $\log \Sigma_{\max}$, 3330 Å. = 4.2. In view of the low intensity of this absorption band, it is unfortunate that the ketone was not further characterised by the formation of the crystalline semicarbazone.

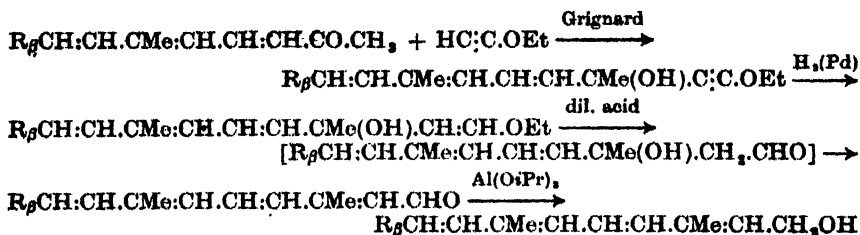


An entirely different method of synthesis of the ester of vitamin A acid has been claimed by Milas ("Vitamins and Hormones," *loc. cit.*; U.S.P., 2,369,158; *Chem. Abs.*, 1945, 39, 5044) although the final product was impure and no attempt was made to obtain the crystalline vitamin A acid from it. For this reason the method will be given only in outline and further comment deferred until full experimental details are available:

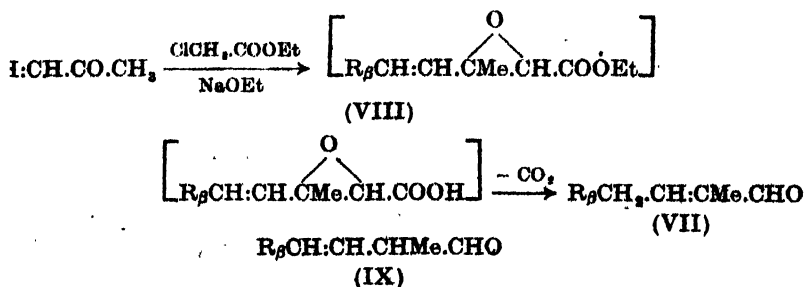


van Dorp and Arens (*Nature*, 1947, 160, 189) have converted the C_{18} ketone (vi) to vitamin A itself by the following ingenious synthesis. A Grignard reaction of (vi) with ethoxyacetylene gave a monoether of an acetylenic diol; which was semi-hydrogenated to the corresponding vinyl ether which, in common with the simple vinyl ethers, rearranged on treatment with dilute acids to a hydroxy-aldehyde which readily dehydrated to vitamin A aldehyde. A Pinner reduction with aluminium isopropoxide then gave vitamin

A itself which had about 35 per cent. of the activity of the crystalline natural vitamin :



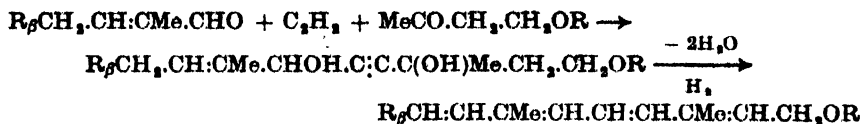
Syntheses based on the C₁₄ aldehyde (VII).—Since 1942 much attention has been given to the use of the C₁₄ aldehyde (VII) as a starting-point for syntheses of vitamin A and its analogues. The aldehyde is prepared from β -ionone by a Darzens synthesis with chloroacetic ester to give a glycide ester (VIII), which is hydrolysed to the corresponding acid and decarboxylated. A detailed investigation of the preparation and structure of the aldehyde was given by Heilbron, Johnson, Jones and Spinks (*J.C.S.*, 1942, 727) who showed that the original product of Ishikawa and Matsuura (*Chem. Zentr.*, 1937, II, 3452) was probably derived from α -ionone. The preparation of the C₁₄ aldehyde (see also Cymerman, Heilbron, Jones and Lacey, *J.C.S.*, 1946, 500) has recently been considerably improved by Lindlar (Swiss P. appl. 18,443/1946; quoted by Isler, Huber, Ronco and Kofler, *Helv. chim. Acta*, 1947, 30, 1911) who describes a one-stage process giving an 80 per cent. overall yield of the aldehyde from β -ionone :



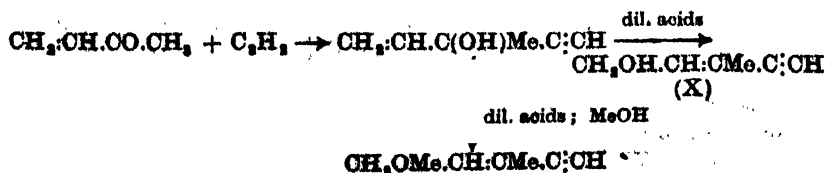
Heilbron, Johnson, Jones and Spinks (*loc. cit.*) produced good evidence to show that the structure of the C₁₄ aldehyde is (VII) and not (IX), on the basis of absorption spectrum and by analogy with the product from α -ionone which is also $\alpha\beta$ -unsaturated. This conclusion is challenged by Milas ("Vitamins and Hormones," *loc. cit.*) on the basis of ozonolysis results when geronic acid is obtained rather than 3:3-dimethyl-2:7-octanedione. The oxidation

of methyl ketones to carboxylic acids during ozonolyses is well known, however, and Milas quotes no yield of the geronic acid obtained and likewise ignores the analogy of the α -ionone product. Milas also stresses the difficulty of obtaining the β - C_{14} aldehyde free from α -, but it had previously been pointed out (Heilbron, Jones *et al.*, *J.C.S.*, 1942, 727; 1946, 500) that all starting materials must be purified by regeneration through a crystalline derivative in order to obtain pure isomers.

The following scheme was envisaged by Heilbron and his co-workers (*J.C.S.*, 1942, 727) for syntheses of vitamin A and derivatives from the C_{14} aldehyde:

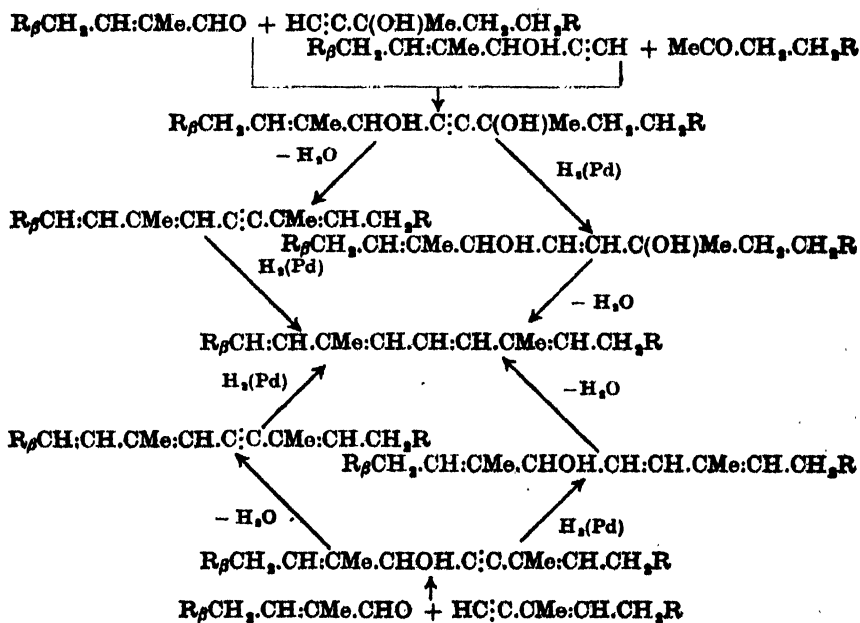


They further stated that "it is obvious from the above that many alternative methods of effecting the completed project present themselves. Thus the acetylenic glycol could be reached either (a) by the condensation of the ketonic portion with the ethynylcarbinol of the C_{14} aldehyde or alternatively (b) by the condensation of the C_{14} aldehyde with the acetylene carbinol derived from an appropriate ketone. Further, the determination of the most suitable stage for the removal of water from the carbinols or glycols and for the semi-hydrogenation of the acetylenic bond leads to a multiplicity of permutations and combinations of the basic theme." They described the formation of the ethynylcarbinol of (VII) and also the condensation of (VII) with acetone to give a C_{17} ketone. In a later paper (Cymerman, Heilbron, Johnson and Jones, *J.C.S.*, 1944, 141), it was stated that particular attention was to be paid to the reaction of the C_{14} aldehyde with 3-methyl-2-penten-4-inol, $HC:C.CMc.CH.CH_2OH$. The successful synthesis of the latter (Cymerman, Heilbron and Jones, *J.C.S.*, 1945, 90) was a result of, firstly, the successful reaction of acetylene with $\alpha\beta$ -unsaturated ketones (see also Hennion and Lieb, *J. Amer. Chem. Soc.*, 1944, 66, 1289) and, secondly, a general study of the anionotropic rearrangement of the ethynylcarbinols of $\alpha\beta$ -unsaturated carbonyl compounds, *e.g.*:



Owing to the relative inaccessibility of (x), preliminary experiments were carried out on the reaction of the C_{14} aldehyde with 1-hexene and 2-methoxy-3-hexen-5-ine, $HC:C.CH:CH.CH(OMe).CH_3$ (Cymerman, Heilbron, Jones and Lacey, *loc. cit.*). Attempted dehydration of the adducts gave anomalous results and the experiments were temporarily discontinued.

In the meantime, however, an extensive series of patents by Milas were published (U.S.P., 2,369,156-2,369,168; 2,382,085; 2,382,086; 2,415,834; *Chem. Abs.*, 1945, 39, 5043-6; 1946, 40, 681; 1947, 41, 3483; *Science*, 1946, 103, 581; "Vitamins and Hormones," *loc. cit.*), in which the syntheses of a number of biologically active materials were claimed by similar routes:

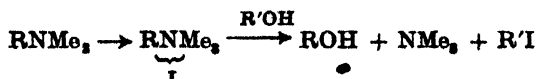


$R = CH_2OEt, OMe, OEt, OCHMe_2, OCM_2, OOC.CH_2, OOC.C_2H_5, NMe_2$

Full experimental details of this work are not yet available and biological data are still lacking for the most highly purified samples of the ethers and esters, and a detailed discussion of the methods should therefore await the publication of these results, at present in the press.* Several novel methods were employed and, although many of the structures appear to be assigned solely on the basis

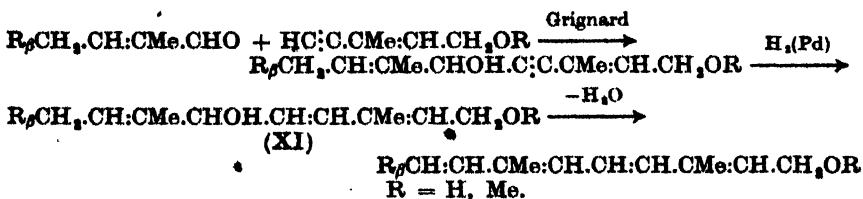
* Added in proof: Several papers on the synthesis of products related to vitamin A by Milas and his co-workers have now been published (*J. Amer. Chem. Soc.*, 1948, 70, 1292, 1584, 1591, 1597).

of absorption spectra, the fact that the crude concentrates were appreciably active indicates that derivatives of the vitamin were obtained. Vitamin A itself was obtained, although impure, from the corresponding dimethylamino compound, by conversion to a quaternary ammonium salt, and then heating in the presence of an alcohol :



The most active concentrate of vitamin A so obtained had $\log \Sigma_{\text{max}} 3240 \text{ \AA.} = 4.61$ (the crystalline vitamin A of Baxter and Robeson, *J. Amer. Chem. Soc.*, 1942, **64**, 2411, had $\log \Sigma_{\text{max}} 3280 \text{ \AA.} = 4.70$) and the most active samples of the methyl ether had $\log \Sigma_{\text{max}} 3230 \text{ \AA.} = 4.67$ (the crystalline methyl ether of Hanze *et al.*, *ibid.*, 1946, **68**, 1389, had $\log \Sigma_{\text{max}} 3280 \text{ \AA.} = 4.70$).

Complete syntheses of vitamin A and its methyl ether from the C_{14} aldehyde have recently been published by Isler, Huber, Ronco and Kofler (*Experientia*, 1946, **2**, 31; Jubilee Volume to Emil Barrell, Basel, 1946, p. 31; *Helv. chim. Acta*, 1947, **30**, 1911) and follow very similar lines to the methods already described, *viz.*



For the synthesis of vitamin A itself, the authors converted the ethylenic glycol (XI) to the monoacetate before effecting the dehydration with iodine in boiling ligroin in the presence of *dl*- α -tocopherol as a stabiliser, and the product was then purified by chromatography, hydrolysed to vitamin A itself which was then converted to the crystalline anthraquinone-2-carboxylate (*cf.* Robeson and Baxter, *J. Amer. Chem. Soc.*, 1947, **69**, 140) or the β -naphthoate (*cf. idem.*, *ibid.*, 1942, **64**, 2410) and then, after regeneration, was obtained as pale yellow prisms, m.p. 60–62°, not depressed on admixture with the natural product, m.p. 62–64°.

Finally, mention should be made of another synthesis of crystalline vitamin A (Cawley, Robeson *et al.*, *Ind. Eng. Chem. (News)*, 1947, **25**, 2884) but no details have yet been revealed.

Thus ends a chapter in synthetic organic chemistry which remained a challenge for nearly twenty years and to which so many eminent chemists have contributed.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

GEOCHEMISTRY.—E. S. Larsen has reviewed fifty years' progress in geochemistry (chapter in *Geology*, 1888–1938, Geol. Soc. Amer. Anniversary Volume, 1941, 393–413). The subjects dealt with in this valuable survey include chemistry of the earth's crust, syntheses of rocks and minerals, economic geology, mineralogy, petrogenesis, metamorphism, volcanic emanations, weathering and soils, and radioactivity.

Dr. S. I. Tomkeieff has written a valuable review of recent "Geochemistry in the U.S.S.R." (*Nature*, 154, 1944, 814), mainly on the work of Vernadsky and Fersman. In spite of much solid work on biological concentrations of elements by Vernadsky, and on pegmatites, etc., by Fersman, Tomkeieff concludes: "It is difficult to avoid the impression that geochemistry in the U.S.S.R. tends to the fanciful, and the criticism so implied is not altogether unjustified. It must, however, be noted that Russian geochemists have done and are doing much spadework, and that ideas and theories, which may have seemed extravagant, have proved valuable both in the co-ordination of knowledge already acquired, and in the search for knowledge as yet unrevealed."

T. F. W. Barth deals with "The Distribution of Oxygen in the Lithosphere" (*Journ. Geol.*, 56, 1948, 41–9), from the novel point of view that "the igneous rocks and the whole lithosphere are to be regarded as essentially a packing of oxygen ions." Oxygen makes up more than 90 per cent. by volume of the total lithosphere, and shows its greatest concentration in the outer shell. "The accumulation of this huge volume of oxygen is made possible by the cations which occupy the interstices and, with their electrical charges, keep the whole structure together although their volume is comparatively insignificant." Thus the lithosphere might appropriately be called the "oxysphere." The regular decrease of oxygen with depth in the crust represents an approximation to thermodynamic equilibrium. If and when highly oxidised rocks are carried down to great depths, oxygen will be squeezed out of the mineral lattices and returned to the surface. Hence the deeper parts of the globe cannot become oxidised.

The discovery of an easy and accurate method for the determination of fluorine, and the consequent re-determination of fluorine in rocks, has led to the result that fluorine, instead of being a very minor constituent, is now known to be present in about the same amount as chlorine (E. S. Shepherd, "Note on the Fluorine Content of Rocks and Ocean-bottom Samples," *Amer. Journ. Sci.*, 236,

1940, 117-28). A tentative average value of 0.04 per cent. is suggested, but there are much greater regional concentrations.

Dr. Shepherd indicates that the geochemical cycle of fluorine is still obscure, but T. F. W. Barth in his paper on the subject (*Journ. Geol.*, LV, 1947, 420-6) has attempted to dispel the obscurity. His results are as follows: "The average concentration of F in the earth's crust is about 800 gm. per ton. . . . Much F enters the magmatic gas phase, which has an acid reaction. Great amounts of F are therefore exhaled by volcanic eruptions. If the magmatic gas phase has opportunity to react with the wall rock, it turns alkalic, and F is extracted (as phosphate?) before it reaches the surface; alkaline hot springs carry no magmatic F. Of the F dissolved by the weathering processes, only 0.2 per cent. is found in the ocean."

A memoir "On the Geochemistry of Tantalum," by K. Rankama (*Bull. Comm. Géol. Finlande*, No. 133, 1944, 78 pp.), begins with the description of a new method for the pre-enrichment of tantalum in minerals and rocks, involving the use of phenylarsonic acid. After the concentration the determination of the tantalum is carried out by X-ray spectrographic analysis. Tables of tantalum content in minerals, ores, rocks and meteorites, are then given. The main results are that tantalum occurs as a cation in its minerals proper, and that it is doubtful whether it occurs native or as a simple oxide. In minerals it can replace tetravalent titanium, zirconium, tin, tungsten, and sometimes trivalent chromium and manganese. Investigation of the tantalum content of igneous rocks shows that the element is concentrated towards the final stages of crystallisation. It is quite a mobile element, notably participating in pneumatolytic processes. This shows that tantalum is a strongly lithophile element; according to Rankama it reveals no siderophile or chalcophile tendencies at all.

S. I. Tomkeieff's very topical study of "The Geochemistry of Uranium" (*SCIENCE PROGRESS*, 34, 1946, 696-712) summarises the facts of distribution of uranium in the earth's crust, the associations of chemical elements in which it occurs, the paragenesis of its minerals, the genetic history of its deposits, and its geochemical cycle. The average content of uranium in the outer shell of the earth is found to be about 1 gr. per ton and, like radium, it is concentrated in the outermost granitic geosphere. This concentration, as compared with the average content of uranium in the earth as a whole, is of a comparatively high order, although still very small as compared with its concentrations in pegmatites, hydrothermal veins, and in certain types of sedimentary deposit.

A paper by C. W. Correns "Beiträge zur Geochemie des Eisens und Mangans" (*Nachr. Akad. Wiss. Göttingen, M.-N. Kl.*, 1941, 219-30) deals with the iron and manganese content of foraminifera and the distribution of these elements in recent marine sediments. The concentration of manganese in the shells of foraminifera is apparently about 100,000 times greater than in sea water. It is believed that the algæ on which the foraminifera live are powerful collectors of manganese, iron and other heavy elements. Hence the concentrations of these elements in the foraminifera.

T. F. W. Barth discusses "Radium and the Petrology of Certain Granites of Finland" (*Amer. Journ. Sci.*, XXXV-A, 1938, 231-46) on the basis of determinations by C. S. Piggot (*ibid.*, 227-30). The results indicate that radium is associated more with the biotite of Finnish granites than with the other mineral constituents, and that in each age-group of granites the total amount of radium increases with the amount of biotite. There is no correlation between radium and FeO or K_2O . It is probable therefore that radium is to be correlated with the mineralogy rather than with the chemistry of granites. More data are required adequately to explain the distribution of radium between the various groups of granites.

A paper by E. S. Larsen and N. B. Keevil deals with "The Distribution of Helium and Radioactivity in Rocks.—III. Radioactivity and Petrology of Some California Intrusives [*sic*]" (*Amer. Journ. Sci.*, 240, 1942, 204-15). The intrusions were of granitic composition, and the determinations were made on minerals separated from three fresh specimens. The distribution of radioactivity was found to be non-uniform, the bulk of the radioactive elements having been separated with the early accessory minerals, zircon, apatite, sphene, which were 100 to 1000 times as radioactive as the common minerals. A decreasing order of helium retentivity of the constituent minerals was found to be hornblende, biotite, quartz, apatite, feldspar, sphene and zircon. Both field relations and corrected helium determinations indicate a Jurassic rather than a Cretaceous age for the earliest members of the batholithic complex of South California.

In 1933 C. S. Piggot summarised the published data on the radium content of ocean-floor sediments, together with that of samples taken by the *Carnegie* (*Amer. Journ. Sci.*, XXV, 1933, 229-38). The important fact was established that there was an extraordinarily high concentration of radium in these sediments, especially in the abyssal red clay, and mostly in the first few centimetres of depth. This work has been continued and described in

a number of papers, several of which have been published under the general title "Radioactivity of Ocean Sediments." Apparatus has been devised to secure deep cores of ocean-floor sediments (C. S. Piggot, "Apparatus to secure Core-samples from the Ocean-bottom," *Bull. Geol. Soc. Amer.*, **47**, 1936, 675-84), and cores 10 feet long have been obtained.

C. S. Piggot and W. D. Urry, "The Radium Content of an Ocean-bottom Core" (*Journ. Wash. Acad. Sci.*, **29**, 1939, 405-10). This core, 2.85 m. long, was collected in the Atlantic off the Newfoundland Banks, and was selected for preliminary examination because of its extremely uniform composition. Determination of the radium content at various depths in the core shows: (A) no decrease of the radium content with depth; (B) a radium content corresponding approximately to that of the general run of granites.

C. S. Piggot and W. D. Urry, "Radioactive Relations in Ocean Water and Bottom Sediments" (*Amer. Journ. Sci.*, **239**, 1941, 81-91). Radioactive atoms in the oceans have been derived from the continents along with others. As they change with time their transitory presence and amount in the water and in oceanic sediments is of peculiar geochemical interest. "There is much less radium in the ocean water and much more in the bottom sediment than is appropriate to the uranium present in each place. . . . Some mechanism exists which removes radium, and its immediate parent ionium, from the water and leaves most of the uranium behind. The ionium produces more radium, and the excess of these two elements, unsupported by uranium, eventually disappears, leaving an equilibrium based on the small uranium content—which is of the same order of magnitude as is found in ordinary sedimentary rocks. It is now apparent that the high radium content of the ocean sediments is transitory and of no great geophysical significance."

C. S. Piggot and W. D. Urry, "The Radium Content of Sediments of the Cayman Trough" (*Amer. Journ. Sci.*, **240**, 1942, 1-12). This research revealed the interrelations of the radioactive elements far better than the earlier ones. The radioactive elements are not in equilibrium in the uppermost layers of ocean-floor sediments, and in this connection only uranium, ionium and radium are important. "A history of these three elements is reflected in the variation of the radium content of ocean sediments with the depth below the ocean floor. Measurements of this variation demonstrate that the concentrations of uranium, ionium and radium at any given time are established by the usual laws of radioactivity governing the growth and decay of radio-elements in a system that is not

in radioactive equilibrium. The experimental results must be adjusted to the conditions that pertained to the undisturbed sediments. This requires a knowledge of the history of the specimens from the time that the sediment was taken from the core-sampler to the time at which the specimens were analysed. The relation between radium content and depth in a given ocean sediment promises a method of determining the rate of accumulation of the deposit at that place."

W. D. Urry and C. S. Piggot, "Concentrations of the Radio-elements and their Significance in Red Clay" (*Amer. Journ. Sci.*, **240**, 1942, 93-103). The relationship between the radio-elements in abyssal red clay is similar to that for the calcareous sediments of the ocean. "The Red Clay, represented here by a core 246 cm. long, is distinguished from the calcareous sediments by a very marked decrease in the radium content just below the surface of the ocean bottom, and the attainment of the final equilibrium between the three radio-elements in the bottom quarter of the core, which signifies a very slow deposition compared with that of the calcareous deposits. . . . The high surface concentrations of radium and ionium, particularly in Red Clay, are therefore only transient phenomena, produced by some unknown mechanism which concentrates these elements relative to the uranium content, during the deposition of the sediment."

The results of the above researches are stated, generalised and applied, in a paper by C. S. Piggot and W. D. Urry on "Time Relations in Ocean Sediments" (*Bull. Geol. Soc. Amer.*, **53**, 1942, 1187-210).

In a paper on "Isotope Ratios: A Clue to the Ages of Certain Marine Sediments," F. E. Wickman (*Journ. Geol.*, **56**, 1948, 61-6) shows that the ratio between a radiogenic and non-radiogenic isotope of an element can be used as an index of the age of marine chemical sediments, if the content of the mother-element can be neglected. The method is specially suitable for strontium in limestones and anhydrites and, less accurately, for lead (Pb_{208}). By a variation of the method uranium can be used for the same purpose.

A valuable collection of geochemical data has been made by H. Brown and C. Patterson in papers on "The Composition of Meteoritic Matter: I. The Composition of the Silicate Phase of Stony Meteorites" (*Journ. Geol.*, LV, 1947, 381-404); "II. The Composition of Iron Meteorites and of the Metal Phase of Stony Meteorites" (*ibid.*, 508-10). A statistical study has been made of analyses of the silicate phases of 107 selected stony meteorites;

and of the average composition, with respect to iron, nickel and cobalt, of iron meteorites and of the metallic phases of stony meteorites (320 analyses). The average composition of iron meteorites is : Fe, 90.8 per cent. ; Ni, 8.6 per cent. ; Co, 0.6 per cent. The metallic portions of stony meteorites have slightly higher values for Ni and Co, which are explained as the result of a relationship whereby the concentrations of Ni and Co in the metallic phase increase as the total metal-phase content of the meteorite decreases.

E. G. Zies discusses "The Concentration of the Less Familiar Elements through Igneous and Related Activity" (*Amer. Journ. Sci.*, XXXV-A, 1938, 385-404). He shows that the rarer elements may be concentrated by various processes of magmatic differentiation, including pegmatite formation ; by the action of acid vapours which form volatile compounds with many elements ; and by diffusion, whereby rare elements can be segregated within the crystal lattices of some common minerals because their ionic radii are almost identical with those of the mineral constituents.

The science of geochemistry has recently suffered the loss of three of its greatest personalities : V. I. Vernadsky, A. E. Fersman and lastly V. M. Goldschmidt, whose death in 1948 was directly traceable to privations endured during the Nazi occupation of Norway. Almost his last paper was one on "The Geochemical Background of Minor-element Distribution" (*Soil Science*, 60, 1945, 1-7). The distribution and availability of the chemical elements, including the minor or trace elements, is fundamentally controlled by the chemical evolution of our planet during geological and pre-geological time. The paper, which is too closely written for brief summary, deals with cosmic frequency and evolution, primordial differentiation, fractional crystallisation of silicate magmas (ionic radii), isomorphous substitution in minerals of igneous rocks, weathering and the cycle of sediments (ionic potential), oxidation and reduction as factors in the distribution of certain minor elements.

The discovery by the late V. M. Goldschmidt and his colleagues that germanium and other rare elements are concentrated in certain coal ashes by the biological processes in trees, has led to a practical method of prospecting for metals in forested areas. K. Rankama has discussed the subject in a paper "On the Use of the Trace Elements in some Problems of Practical Geology" (*C.R. Soc. Géol. Finlande*, No. XIV, 1940, 90-106). The solutions containing rare elements are drawn up from the soil and are evaporated mainly from the leaves of the trees. The rare elements are thus concentrated in the leaves, and eventually in the ground litter or humus ;

or they may be re-dissolved and dispersed according to their chemical properties.

Hence unusual concentrations of metals in plant ashes may mark localities in which concealed ore-deposits occur. This method has been tested for forested areas in Finland, for plants growing on old dumps from metalliferous mines, and in various other ways, with positive results. Rankama has established the enrichment of nickel in plant ashes from two nickel-mining districts in Finland. It is stated that the method is well adapted for the prospecting of elements, such as Au, Pt, Sn, Mo and W, which are not easily detected by ordinary geological or geophysical methods. It is also claimed to be a rapid method applicable to the prospecting of a large number of elements at the same time.

L. R. Wager and R. L. Mitchell have made "Preliminary Observations on the Distribution of Trace Elements in the Rocks of the Skaergaard Intrusion, Greenland" (*Min. Mag.*, 26, 1943, 283-96). They find that the marginal gabbro taken to represent the original magma shows fairly good agreement in respect of trace elements with an average for gabbros prepared by Noll, with the exception that SrO is ten times as abundant as in the average. As the acid gneisses which surround the intrusion are also richer in SrO and BaO than the average granite, it is suggested that the outer layers of the crust in East Greenland are richer in SrO and BaO than the average.

The rocks of the Skaergaard intrusion were produced by fractional crystallisation from a normal olivine-gabbro magma. The investigation shows that Cr and Ni tend to be concentrated in the earlier differentiates, V and Co in the early middle differentiates, Cu and Li in the later middle differentiates, while Mo, Zr, Th, La and Rb are concentrated in the acid granophyre, the latest differentiate. Sr occurs in considerable amount except in gabbro-picrite, the first differentiate; while BaO shows a continuous and marked increase in amount throughout the whole range of the differentiation series.

In a paper on "The Distribution of Trace Elements in a Scottish Permo-Carboniferous Teschenite and its Lugaritic Differentiate," E. M. Patterson (*Geol. Mag.*, LXXXII, 1945, 230-4) has analysed a biotite-teschenite and a lugaritic differentiate from a picrite-teschenite composite sill at Saltcoats, Ayrshire. The teschenite is fairly rich in Sr and Ba. In the lugarite, however, the trace elements are present in greatly differing proportions. Sr and Ba, for example, increase markedly in the lugarite; Li and Rb increase along with the potassium, but Cr and Sc diminish greatly in the differentiate. The results are correlated with Goldschmidt's work

on the conditions governing the distribution of trace elements in crystallising magmas.

S. R. Nockolds and R. L. Mitchell have published an elaborate and comprehensive account of "The Geochemistry of some Caledonian Plutonic Rocks: A Study in the Relationship between the Major and Trace Elements of Igneous Rocks and their Minerals" (*Trans. Roy. Soc. Edin.*, LXI, Pt. II, 1948, 533-75), which is one of the earliest papers on what promises to be a valuable new line of research in chemical and distributional petrology. Plotting some 90 analyses in a silicon variation diagram it is shown that the points towards the high-silicon end lie on curves of continuous variation which are interpreted as representing the liquid line of descent starting with a common type of pyroxene-mica-diorite magma. On the other hand, the points at the low-silicon end are highly scattered, and are taken to represent rocks whose composition is probably due to crystal-sorting in greater or lesser degree, *i.e.* of olivine, pyroxenes, hornblende, or plagioclase feldspar.

Concerning the trace elements three problems arise: (1) their distribution in a series of related rocks and their relations to the major elements; (2) their distribution among the constituent minerals of an individual rock; (3) the distribution of the major and trace elements in a given mineral species occurring in different rocks. Each of these problems is treated in regard to the Caledonian igneous region of Scotland. There is the usual concentration of Cr, Ni and Co in the non-feldspathic accumulative types, and of other elements in the other accumulative rocks and in those differentiated along the line of liquid descent. It is not possible to go into detail in this short note, but the major conclusion of the authors is that "The trace elements are found to behave in a manner similar to that of the major elements in the sense that, when plotted against silicon, they show scattering when the major elements show scattering (accumulative rocks), and fall on continuous curves when the major elements do the same (liquid line of descent)."

BOTANY. By Professor W. H. PEARSELL, D.Sc., F.R.S., University College, London.

CYTOGENETICS AND HEREDITY.—The speed with which a promising line of biological investigation is nowadays followed up is exemplified by what has happened since the discovery of the special effects of colchicine upon cell division. This was first recorded by L. J. Havas in *Nature*, 139, 371, 1937, from work done in the laboratory of A. P. Dustin at Brussels. A colchicine bibliography, covering

the work done in the intervening ten years, has now been compiled by O. J. Eigsti, with a supplement by P. Dustin, Jr., in *Lloydia*, 10, 65-114, 1947. (It may in fact be obtained separately from the Lloyd Library, 300 W. Court St., Cincinnati 2, Ohio, for 50 cents.) This represents mainly the European and American literature and the authors remark that probably some titles have been omitted; for Russia in particular records are scarce. Even so, nearly 1400 titles are given, and there is a useful chronology of the earliest publications and particular reference to certain papers summarising the early history.

One of the principal events in this branch of the subject during the past year has been the appearance of the new journal, *Heredity*, edited by C. D. Darlington and R. A. Fisher. The first number offers a rich promise of things to come, not only in the original papers included, but also in those which are to appear in the near future. Useful summaries are given in Part I of the genetical researches carried out in the principal British centres between 1939 and 1943, these being contributed by prominent workers in each centre. A. Buzzati-Traverso (Pallanza) contributes a parallel bibliography of genetical papers published in Italy and Germany during the same period of years.

Of the purely botanical papers, two only will be mentioned here. O. Frankel (*Heredity*, 1, 109, 1947) contributes a suggestive analysis of the theory of plant breeding for "yield," a point of view which includes a very large proportion of agricultural work. In essence, his analysis contrasts the differences between (i) the selection for resistance to factors which limit yield, which are more or less observable in single plants and their immediate progenies, and (ii) the selection among typical quantitative factors which are mainly involved in determining yields. The suggestion is made that the practical advances possible in this field require further examination and involve the use of numerous races and of large populations.

The second of the purely botanical papers is of a very different type. In it, S. C. Harland (*Heredity*, 1, 121, 1947) deals with the alteration of frequency of a gene (B) for waxy bloom in *Ricinus communis* in relation to climate. This behaves in breeding experiments as part of a simple allelic pair, B—b. In Lima, Peru, plants containing this gene do not reproduce during the "winter" months, although the normal type continues to produce seed regularly. Thus in Peruvian coastal stations, plants with bloom make up only 0.15 per cent. of the population. Further inland, cloud and fog diminish and sunshine is more frequent as elevation rises.

These changes, particularly that in sunshine, remove the inhibiting factors in winter seed production so that the proportion of plants with bloom rises and at 7764 feet it was observed that the whole population possessed bloom.

Some British work has also appeared in which the attempt has been made to consider hereditary and ecological factors. M. Skalińska (*J. Linn. Soc. London*, 53, 159, 1947) has studied the wide range of forms in *Valeriana officinalis* L. from this angle. This species in Britain is found to include not only a wide range of phenotypic and genotypic plasticity in nature, but also a tetraploid ($2n = 28$) and an octoploid ($2n = 56$) form. Purely morphological criteria are inadequate to distinguish between the two polyploids, but it is found that the tetraploid, presumably the older and continental type, is limited in its distribution to the drier parts of southern England and to parts of the Midlands. The octoploids, on the other hand, favour moister soil and occur all over Britain. Thus the view is taken that the results favour Hagerup's idea that the distribution of a species may extend as a result of polyploidy.

H. G. Baker has studied a highly different example in some detail in the *Biological Flora* (*J. Ecol.*, 35, 271, 1947) in relation to *Melandrium dioicum* and *M. album*. These have broadly similar chromosome complements ($2n = 24$), 22 autosomes and 2 sex chromosomes. *M. dioicum* is a diminishing species, a woodland form that is disappearing through deforestation. *M. album* has almost entirely replaced it in East Anglia. This is a plant of more open habitats hybridising freely with *M. dioicum*, and particularly characteristic of southern Britain. *M. dioicum* is now more frequent in Scotland and northern England. A summary of criteria for distinguishing the two species and their hybrids is given. Some of the characters, such as leaf shape and thickness and flower colour, are affected by environmental changes. Others are not, and these include seed weight and colour, capsule shape and pedicel length.

VIRUSES.—There seems always to have been a strong tendency for botanists to look at plant viruses in one of two ways. On the one hand are those who have regarded viruses wholly as organisms and, on the other, the more limited number, perhaps, whose work has emphasised more strongly the chemical properties and effects of viruses. These have tended to consider a virus wholly as a chemical substance. While neither point of view excludes the other, the difference of the two outlooks upon the mode of investigation in this field has been very pronounced.

The development of improved techniques has gradually moved the area of conflict into a somewhat different direction. It is

perhaps obvious that the nature of the virus as studied *in vitro* is not necessarily the same as that in which it exists in the cell. N. W. Pirie (*Cold Spring Harbour Symp. Quant. Biol.*, 9, 184, 1947) has pointed out that the most obvious change during the isolation of tobacco mosaic virus preparations is the aggregation observed during purification or even on standing. It is probable that part of the virus in the infected cell is aggregated, but that this proportion is increased by most of the processes used in purification.

This conclusion is based in the first instance on the work of F. C. Bawden and N. W. Pirie (*Brit. J. Exper. Path.*, 26, 294, 1945) mainly by serological and ultra-centrifuge methods. E. M. Crook and F. M. L. Sheffield (*Brit. J. Exper. Path.*, 27, 328, 1946) have since made a detailed study of the aggregation of tobacco mosaic virus during preparation and purification, using the methods of isolation employed by Bawden and Pirie in conjunction with the electron microscope. Their results confirm the conclusions earlier obtained by those methods. In brief, preparations of this virus show a range of particle length varying from 15 μ to 300 μ —the diameter of the rods being about 15 μ . The length of the rods depends on the method of preparation and on its duration. The authors point out that all the micrograms of this virus published show similar variations in size, but in the past these variations have largely been ignored because, perhaps, of the preconceived idea that viruses are macro-molecules and thus are expected to be of constant size.

In Pirie's view, the virus inside or outside the cell has thus a range of properties whose boundaries can by no means be sharply defined. The importance of this variability has also been emphasised, of course, by other workers, some of whom, like J. W. Beard and his colleagues (*Proc. Med. Inst. Chicago*, 15, 294, 1945), conclude from such observations on variability that viruses are small organisms going through cycles of growth and division. These conclusions may ignore the possibility that substances like the starches, glycogens, haemocyanins and possibly even haemoglobins may exist in a variety of states and particle sizes. The basic problem is thus not so much whether viruses can most usefully be called molecules or organisms as whether anything is to be gained by calling particles of the order of size 10–100 μ macro-molecules. In Pirie's view, no measurement that has so far been made on a virus would detect variations of molecular weight of less than 100,000 units, and generally the possible variation might be much more. Even further, the assumption that all the particles in a preparation are identical is foolish. This is a familiar difficulty

even in protein chemistry, where the possibility of variation in structure around what might be called "outlying" parts of the molecule is envisaged and probably exists. If it is recognised that there may well be an essential structure and an unessential structure in these large molecules, the danger of assuming a rigid structure in still larger units is thus obvious.

ECOLOGY AND LAND UTILISATION.—An interesting application of plant ecology has been made by H. B. Gilliland (*S. Afric. Geog. Journ.*, April 1947, 43) in dealing with the problems that arise in devising an administration suitable for the government of nomadic tribes in Somaliland. The normal method employed in British tropical and eastern dependencies, that of appointing District Commissioners to administer political districts with clearly defined topographical boundaries, proves quite unsuitable in regions populated almost wholly by nomadic races. The attempt to devise a more suitable form of administration involves firstly a study of the mode of life of the native peoples and then a survey of the factors influencing their movements. In Somaliland these factors are mainly the character of the vegetation, which determines the grazing facilities and also the distribution of trees and shrubs yielding exportable gums. Thus a typical eastern Somali tribe will usually move towards the permanent wells during the drought season, moving away and following the grazing during the rains. The apparent drainage channels indicate that the climate was formerly wetter and accumulations of water below the surface are often temporary. The former stream channels become blocked by re-distributed soil at their junctions with larger drainage channels and these areas (*dohos*) are sites of temporary grazing.

The tribal districts tend to centre around units of permanent water and related *doho* systems and the annual migrations thus usually have boundaries, though these bear no relation to the suggested political regions. A natural system of administration should be based on the migration boundaries, but it must also take into account the need for vegetation and soil conservation. Sheep and to a less extent goats have markedly deleterious effects in both the latter respects and the most valuable grazing animal is the camel, more cosmopolitan in its tastes, browsing on thorny shrubs, hardy for water, and not destructive of the soil surface, neither destroying grassy cover nor cutting through the existing surface as do sheep. If the economy of the native tribes continues to centre around camels, the existing tribal migrations would maintain themselves; a change in the type of grazing animal would lead to a change (and a reduction) in the migration boundaries.

It is of interest to contrast with this type of utilisation problem those that may be involved in the utilisation of freshwater lakes. Widespread interest has recently been taken in the possibility of increasing the gross productivity of such bodies of water. Usually the aim is to obtain larger crops of fish, and the suggested method is to employ manurial treatments in which nitrogenous or phosphatic substances are added, or else organic manures. A pertinent aspect of this subject that has been generally overlooked is that there is a large literature dealing with the fertilisation of lakes by purified sewage effluents. These contain the very substances, phosphoric, nitrogenous and organic, that are suggested as the most suitable manures. The known history of lakes affected by these materials is thus useful as a guide to the probable results of attempts to increase productivity by manurial additions. This subject has recently been reviewed by A. D. Hasler (*Ecology*, **28**, 383, 1947) who gives a long catalogue of recorded examples. The effects have almost always been entirely unsatisfactory. They result in abundant growths of unsuitable algæ, particularly the blue-green, which yield unsightly scums and smells, often involving preventative treatment and adding greatly to the cost of filtering where water is used for industrial purposes or human consumption. The effect on fish has been equally unfavourable, at best a reduction in the numbers of the desirable kinds and increased production of less desirable types such as coarse fish. As in most cases the changes observed seem generally to be of an irreversible kind as well as undesirable, it seems that a good deal of further investigation is required of the effects of the manurial treatment on the algal cycles, which show the most striking effects, and appear to inaugurate the wider biological changes.

ZOOLOGY. By DAVID BARKER, M.A., D.Phil., The University, Oxford.

THE INNERVATION OF SKELETAL MUSCLE.—The progress made in the earlier decades of this century towards resolving some of the outstanding problems relating to the innervation of vertebrate skeletal muscle was excellently reviewed by Hinsey in 1934 (*Physiol. Rev.*, **14**, 514). There had been considerable controversy as to whether individual muscle fibres ever possessed more than one motor nerve-ending (i.e. endplate) and as to whether there was some kind of accessory efferent innervation, in addition to that provided by somatic motor fibres, which was concerned solely with the maintenance of muscular tone. With regard to the first of these issues, the balance of evidence at that time led Hinsey to

conclude that "the occurrence of more than one endplate . . . in a single muscle fibre is a rare phenomenon," and with regard to the second there appeared to be every reason to suppose that "the somatic motor fibres represent the efferent pathway to skeletal muscle fibres for all types of their contraction." During the last few years, however, evidence has accumulated which demands a reconsideration of these conclusions, and also of certain other aspects of muscle innervation.

The Multiple Innervation of Single Muscle Fibres.—Katz and Kuffler (*J. Neurophysiol.*, 1941, 4, 209) have recently presented strong physiological evidence which indicates that most of the muscle fibres in the sartorius of the Australian tree-toad (*Hyla aurea*) are innervated by two or three motor nerve-endings. The majority of the fibres composing the sartorius muscle run through nearly its entire length and the motor endings are mainly concentrated within two zones so as to leave a small area at either end of the muscle completely devoid of terminations. Katz and Kuffler placed their recording leads on either one or the other nerve-free end of the muscle and electrically stimulated the nerve supplying it. In both cases they expected to obtain a prolonged and complicated muscle action potential composed of individual waves which originated in the various myoneural junctions and arrived in succession at the leads. However, in both instances they obtained, not a complex potential wave, but a *single spike* similar in all respects to that obtained by maximal direct stimulation. The action potentials originating some distance away from the recording leads had for some reason failed to reach them, while apparently impulses had been initiated in a majority of the muscle fibres nearby. Katz and Kuffler suggest that this phenomenon could be accounted for by supposing that most of the fibres composing this muscle are multiply innervated, a motor ending being supplied to each fibre as it travels through each of the innervation zones on its course through the length of the muscle. "In this way," they write, "impulses originating at nerve-endings far away from the amplifier leads would collide during conduction with those coming from the more proximal junctions, and so could not be recorded." The results of experiments performed to test the validity of this hypothesis render it a very probable one. The object of each experiment was to abolish the muscle action potentials initiated nearest the recording leads, so that those arising farther away would be able to travel through unhindered and so be recorded. This was done, for example, by cutting one of the two main nerve branches supplying the muscle, by blocking some of the motor endings with the drug curare, and

by submaximal nerve stimulation. These procedures led to some or all of the myoneural junctions adjacent to the recording leads being put out of action, with the result that the early spike potential was partially or wholly abolished and replaced by a discrete potential wave of long latency.

Unfortunately, in conformity with the modern trend in neurophysiology, Katz and Kuffler made no attempt at any histological confirmation of these findings. However, they drew attention to a paper by Sandmann (*Arch. Anat. Physiol. Lpz.*, 1885, 240) which has apparently escaped notice in recent literature. Sandmann dissociated various muscles of the frog by repeated boiling after soaking them for several days in dilute sulphurous acid, and then subjected the isolated muscle fibres to a modified gold chloride technique. He observed that though fibres with one motor ending often occurred in the sartorius muscle, there were many which possessed two or three, or even four to six, discrete motor terminations evenly spaced apart along the length of the fibres. The same condition occurred in m. cutaneus (dorsi or pectoris not specified), but in mm. gracilis and semimembranosus only the longest fibres were multiply innervated (by two motor endings) and in mm. gastrocnemius and triceps brachii fibres of all lengths had only a single endplate. Sandmann's findings with regard to the sartorius muscle thus provide a histological verification for the conclusion indicated by Katz and Kuffler's experiments. However, Sandmann's work has to be compared with the later and probably more refined investigations of Kulchitsky, Lawrentjew, and Mather, which indicate that the multiple innervation of muscle fibres in the sartorius and gastrocnemius muscles of frogs is extremely rare. These conflicting observations may be due to the use of different Anurans by the workers concerned; Kulchitsky *et al.* used *Rana esculenta*, *R. temporaria*, and *R. pipiens*; Sandmann does not state which kind of frog he used, and Katz and Kuffler worked with the toad *Hyla aurea*. It is conceivable that a multiple motor supply to single muscle fibres is a normal occurrence in muscles composed of long fibres whose function demands that contraction should be executed with maximum rapidity. If this were so one would expect the same muscles in different Anura to vary in this respect according as to whether the species concerned was mainly a slow terrestrial crawler or of the more active kind which mainly swims and jumps. The efferent innervation of amphibian muscles clearly calls for further histological investigation; one of the first steps might well be the examination of the sartorius in *Hyla aurea* after the degeneration of sensory and sympathetic fibres.

In mammals Garven's finding (1925) of a motor endplate situated at either end of two long muscle fibres in the hedgehog's m. panniculus carnosus remains the only convincing instance of multiple innervation that has been reported. A few years ago Agduhr (*Upsala Läkarefören. Förhandl.*, 1939, 45, 401) published further evidence in support of his long-standing claim of a double innervation of individual muscle fibres in the cat's interosseus and lumbrical muscles, but his paper does nothing to remove one's doubt as to the validity of this interpretation in the light of Wilkinson's critique (1929) of Agduhr's earlier work. Furthermore, Wilkinson's own findings were negative in this respect when examining the same material.

The intrafusal muscle fibres of the muscle spindles appear to constitute a special case; according to Barker (*Quart. J. Micr. Sci.*, 1948, 89, Pt. 2) these fibres receive a motor innervation at each end, so that the number of endplates possessed by a spindle is always approximately double the number of intrafusal muscle fibres composing it. In between the two ends, or poles, of the intrafusal muscle fibres there is a region where they contain a dense aggregation of central nuclei and lose their cross-striations. Thus the two poles of each fibre appear to be separated by an area that is not contractile, and Barker suggests that each pole functions as a contractile unit independently of the other. The fact that he frequently found the two poles of each muscle fibre innervated by separate motor fibres of different diameters would appear to support this conclusion and put this kind of multiple innervation in a different category from that described for extrafusal muscle fibres.

Is there more than One Kind of Efferent Innervation?—The claim that the maintained contraction characteristic of muscle tone is controlled by a different motor system from that serving other types of contraction has frequently been made during the course of the present century. But the nature of this "tonic" motor system has never been satisfactorily demonstrated and current opinion (see, for example, Lloyd, Ch. 6, p. 153, in *Howell's Textbook of Physiology*, 15th Ed., 1946) echoes that of Sherrington (1932) who believed tone to be "simply part and parcel of ordinary motility; a manifestation of that ordinary rhythmic neuro-muscular activity by which are executed all our reflex and other acts involving skeletal musculature." However, the recent works of Häggqvist (e.g. *Acta Med. Scand.*, 1940, 104, 8) and Kuffler and Gerard (*J. Neurophysiol.*, 1947, 10, 383) suggest that there may be a dual efferent innervation of muscle after all.

Häggqvist maintains that the majority of skeletal muscles are

innervated by two kinds of myelinated motor fibres arising in the ventral roots, large fibres measuring on an average about $10\ \mu$ in the roots and terminating in typical *en plaque* endplates, and smaller fibres with an average diameter of between 2 and $4\ \mu$ which terminate in the *en grappe* type of endplate first described by Tschiriew (1879) in reptilian muscle. The individual muscle fibres are supposed to be innervated by both motor systems, each one bearing the two kinds of endplate. The large motor fibres are said to produce ordinary contractions, while the finer fibres provide "the substratum of tonic innervation." In experiments with transitory ischemia of the spinal cord in rabbits Häggqvist produced a flaccid paralysis in the hind-legs of the animals. Subsequent examination of the ventral roots showed a selective degeneration of the large fibres, whereas the majority of the small fibres were intact. In these experiments, according to Häggqvist, the ordinary contractility of the muscles is lost owing to the destruction of the *en plaque* endings and their fibres, while the hypertonic condition is due to the persistence of the small motor fibre system which has survived the ischemia. For this hypothesis to be convincing it would be essential to present abundant photographs of the histological evidence supporting it, but these are lacking in Häggqvist's papers; a drawing such as that produced in Fig. 1 of his 1940 paper (showing a degenerated *en plaque* endplate next to an intact one of the *en grappe* type on the same muscle fibre) would have perhaps been adequate a generation ago, but it carries little weight today. The *en grappe* motor ending has never previously been found in the limb musculature of mammals, and it seems unlikely that it would have escaped notice in all the earlier investigations. However, the existence of this type of ending is well established in amphibians and reptiles, where it occurs in addition to the *en plaque* ending; furthermore, in a recent study of motor endings Tello (*Trab. del Instit. Cajal de Invest. Biol.*, 1944, 36, 1) claims to have seen the two kinds of endplate innervating the same muscle fibre in a snake. Against this background Häggqvist's thesis seems less improbable, but without the necessary histological proof it remains unconvincing. Moreover, in this matter one has to take into consideration the work of Carey (e.g. *Anat. Rec.*, 1941, 81, 393; *Am. J. Path.*, 1942, 18, 288), who maintains that there is a change in shape of motor endings correlated with their functional activity, and the belief of Wilkinson (1929) and others that the *en grappe* ending is merely an immature form of the *en plaque*.

There is no doubt as to the existence of a group of fine motor fibres emanating from the spinal cord to take part in the innervation

of mammalian skeletal muscle, but Leksell's recent study of this efferent component in cats (*Acta Physiol. Scand.*, 1945, **10**, Suppl. 31) strongly suggests that the fibres concerned innervate the muscle fibres of the spindles rather than extrafusal muscle fibres in the manner maintained by Häggqvist. Leksell found the conduction velocity of the fibres in this group to be between 20 and 38 per cent. of that of the larger "alpha" motor fibres (assuming this to be 100 m.p.s.), and following the terminology of Erlanger and Gasser he designates them "efferent gamma" fibres. By selective stimulation of these small motor fibres, conduction in the larger "alpha" fibres being blocked by pressure, he showed (a) that the isometric contraction was reduced to 1-2 per cent. (10-20 gm.) of its maximum value, and (b) that there occurred a large increase in the afferent discharge from the muscle. Both these observations are in accord with the view that the "gamma" fibres innervate the intrafusal muscle fibres. However, Leksell found that an increase in the afferent discharge from the muscle could also be produced when, without a pressure block, an efferent stimulus was applied which was subthreshold for the "gamma" fibres and activated members of the "alpha" group only. The muscle spindles thus appeared to be innervated both by the small "gamma" efferent fibres and fibres larger than these belonging to the "alpha" group. Barker's finding (1948, *ref. as quoted*) of both small (3-4 μ) and relatively large (6-7 μ) nerve fibres taking part in the intrafusal motor innervation of rabbit muscle spindles seems to furnish histological evidence in support of this conclusion.

At present, therefore, there is no valid reason for accepting the postulation of a dual efferent innervation for the skeletal muscles of mammals, and it seems unlikely that muscular tone is anything more mysterious than a "maintained, low grade, asynchronous, stretch-evoked reflex activity" (Lloyd, 1946, *ref. as quoted*) mediated by the stretch receptors and the *en plaque* motor fibres. In amphibia, however, Kuffler and Gerard (1947, *ref. as quoted*) have recently extended the work of Sommerkamp (1928), Swerdloff (1933), and Tasaki and Mizutani (1944) and convincingly established the existence of a dual innervation of frog muscle by large and small motor fibres which produce different types of mechanical response. Stimulation of a single large motor fibre (about 15 μ in diameter) evokes the familiar twitch contraction, but repetitive excitation of a single small fibre (about 5 μ in diameter) elicits a slow local contraction limited to one or two regions of the muscle fibre. Direct observations and recording experiments indicated that individual muscle fibres were innervated by both types of motor fibre, each

one responding to both nerve stimulations with a local shortening or an overall twitch as the case might be. Kuffler and Gerard found that the small motor fibres were distributed to the majority of the limb muscles with the exception of mm. sartorius and adductor longus; their threshold was 3-6 times higher than that of the most excitable large motor fibres, and the ratio of the conduction velocities of the large and small fibres was found to be 5:1. The fact that stimulation of the dorsal roots and the sympathetic chain failed to elicit the local contractions indicates that the small motor fibres leave the cord via the ventral roots. Thus, according to these experiments, the skeletal muscle fibres of the amphibians used (*Rana pipiens*, *R. catesbiana*, and occasionally toads) appear able to execute either a rapid twitch or a slow local contraction in an analogous way to the muscle fibres of crustaceans (see review on crustacean muscle by Wiersma, 1941, *Biol. Symp.*, 3, 259). It seems probable that the small motor fibres studied by Kuffler and Gerard are identical with those seen in frog's muscle by a number of histologists in the past forming "fine nerve endings" (Murray, 1924) or *en grappe* terminations (Pézarid and May 1937) in addition to the ordinary endplates formed by larger fibres. However, it would be premature to speculate about these results until we possess further histological data about the efferent innervation of amphibian muscle, both as regards the motor endings and the size and distribution of the nerve fibres supplying them. It is also desirable that experiments should be made to demonstrate the relationship of this small motor fibre contracture to other types of muscle contraction. Further, the possibility that seasonal variations have an effect upon this phenomenon should be investigated. Slow and prolonged contractions appear to be characteristic of muscle fibres with centrally placed nuclei, e.g. those of "red" muscles, and the muscles of hibernating animals (Patrizi, 1894), and in this respect it is interesting to note that during the inactive winter months the nuclei of amphibian muscle fibres migrate from the periphery to take up a central position. It may be that the non-propagated type of contraction described by Kuffler and Gerard is only exhibited by a certain kind of muscle fibre peculiar to amphibian muscle. Katz and Young (*J. Physiol., Proc. Physiol. Soc.*, 1948, 107) examined the possibility that the contractions were entirely located in the intrafusal muscle fibres of the spindles, but failed to find any evidence for it.

The Morphology of the "en plaque" Motor Endplate.—In 1944 Tello (ref. as quoted) and Gutmann and Young (*J. Anat.*, 78, 15) published conflicting observations with regard to the relationship

of the ramifications of the *en plaque* motor ending and its sole-plate. The examination of endplates in amphibia, reptiles, and mammals, impregnated by a combination of the silver methods of Gros and Cajal, led Tello to believe that the axon ramifications are enwrapped by a nucleated sheath which appears to him to be a continuation of the Schwann tube. The nuclei of these "arborisation tubes" are said to be quite distinct from those embedded in the underlying sarcoplasm of the sole-plate. On the other hand, Gutmann and Young (Bielschowsky preparations of rabbit muscle) maintain that the axon ramifications lie naked on the surface of the sole-plate, indenting its surface and often separated from it by clear spaces. It seems likely that these clear channels in between the nerve ramifications and the sarcoplasm, together with nuclei regarded by Gutmann and Young as situated within the endomysium ("outer endplate nuclei"), constitute those features of the endplate which Tello interprets as the sheaths and nuclei of the arborisation tubes. From the point of view of myoneural conduction, Gutmann and Young's interpretation seems to be the most probable one. It is interesting to note the complete swing away from Boeke's view that the axoplasm of the motor terminals and the sarcoplasm of the sole-plate are continuous. Both Tello and Gutmann and Young maintain that the ramifications lie outside the sole-plate, below the sarcolemma and on the surface of the membrane of the muscle cell. According to Barker (1948, *ref. as quoted*) the sensory terminations of muscle spindles also lie on the surface of the membrane surrounding the muscle fibre (i.e. the "sarcoplasmatic" membrane of Gutmann and Young). It has been assumed by histologists in the past that nerve-endings terminating upon the sarcolemma (i.e. "epilemmal") are sensory, while those which terminate underneath it (i.e. "hypolemmal") are motor. The above observations suggest that this is not the case; apart from the endings formed by a small group of fine sensory fibres innervating endo- and perimysial connective tissue, all nerves terminating in relation to muscle fibres appear to be hypolemmal.

NOTES

Fraunhofer's Collaborator (M. Schofield)

The arrival of George Bontemps at a well-known Smethwick glass-works in 1848—an event partly due to the Revolution and one benefiting British optical glass development considerably—occurred just a century after the birth of Guinand, a pioneer in this field, whose bicentenary brings to mind one or two striking points. Bontemps had paid Guinand's son Henri 3000 francs for "instruction" in optical glass making, a sum which Bontemps later charged Lucas Chance for his services at Smethwick. So we look to Guinand *père*, as well as to Chester Moore Hall, Dollond, Vernon Harcourt and Faraday, for the foundations from which sprang Britain's present lead in these post-war years. Guinand began as Swiss watch- and clock-maker and was attracted to optics, much as Galileo's life was changed by the swing of the pendulum lamp at Pisa, by a chance peep at some rough form of telescope. An instrument-maker named Droz took him in his care, and soon Guinand was making his *lunettes* and *lentilles* for telescopes. Though he appreciated the need for achromatic combinations, the fact that flint glass was only being made in England proved a barrier to progress. But Guinand took up the chemical aspects of glass as spare-time study; and, since Fraunhofer in Bavaria had arrived at the same stage, the two decided to join forces in 1805, collaborating for nine years in a workshop established in the ancient abbey of Benedict-Beurn.

Later came the period of Guinand's return to his native Switzerland, to take over the Brenet's workshop at Neuchâtel and make flint and crown lenses as well as spectacles. The 1824 exhibition in Paris brought him some recognition; for Louis XVIII had heard of his achromatic objectives and invited Guinand to France to found a factory to be supported by the State. Guinand decided he was too old. His son Henri brought to France the processes which, thanks to Bontemps, benefited both that country and Britain. In a Bakerian lecture Faraday referred to the Guinand-

Fraunhofer partnership : " It is certain that the public are not in possession of any instruction relative to the method of making a homogeneous mass fit for optical purposes, beyond what was possessed before their time . . . the knowledge they acquired was altogether practical and personal, a matter of minute experience." Yet today, with British optical glass improved beyond all imagination, two essential points in producing the superior grades are : the use of platinum vessels for the melt (Faraday used platinum) ; and Guinand's basic idea in continuously stirring the melt, then allowing it to cool in the glass pot, breaking it up, and selecting the best pieces as blanks.

Miscellanea

The following were elected to be Fellows of the Royal Society at the meeting held on March 18 : Dr. T. E. Allibone, director of the Research Laboratories of Associated Electrical Industries ; Dr. F. P. Bowden, lecturer in physical chemistry, University of Cambridge ; H. Constant, deputy director (research), National Gas Turbine Establishment ; Dr. S. F. Dorey, chief surveyor, Lloyd's Register of Shipping ; E. H. Farmer, assistant director and senior organic chemist of the British Rubber Producers' Research Association ; Prof. O. R. Frisch, Jacksonian professor of physics in the Cavendish Laboratory, Cambridge ; Sir John Fryer, secretary of the Agricultural Research Council ; Prof. T. M. Harris, professor of botany in the University of Reading ; Prof. W. H. Heitler, senior professor in the School of Theoretical Physics, Institute for Advanced Studies, Dublin ; A. L. Hodgkin, assistant director of research in physiology, University of Cambridge ; Dr. G. M. Lees, chief geologist to the Anglo-Iranian Oil Company ; Prof. R. A. McCance, professor of experimental medicine, University of Cambridge ; Dr. K. Mahler, senior lecturer in mathematics, University of Manchester ; Dr. Sidnie M. Manton, lecturer in zoology at King's College, London ; Dr. Dorothy M. Needham, of the School of Biochemistry, University of Cambridge ; Prof. J. H. Orton, professor of zoology, University of Liverpool ; Sir Leonard Parsons, emeritus professor of diseases of children, University of Birmingham ; Dr. S. Peat, reader in organic chemistry, University of Birmingham ; Prof. G. W. Robinson, professor of agricultural chemistry, University College of North Wales, Bangor ; Dr. W. A. H. Rushton, university lecturer in physiology, Cambridge ; J. W. Ryde, senior physicist in the Research Laboratories of the General Electric Company ; G. R. S. Snow, Fellow of Magdalen College, Oxford ; Dr. E. W. R. Steacie, director, Division of Chemistry, National Research Council, Canada ;

Dr. J. A. Todd, lecturer in mathematics, University of Cambridge ;
Dr. F. Yates, chief statistician, Rothamsted Experimental Station.

H. M. the King has approved the award of the Founder's medal of the Royal Geographical Society to Mr. Wilfred Thesiger, for his contributions to the geography of southern Arabia, and of the Patron's medal to Mr. Thomas H. Manning, of the Geodetic Survey of Canada, for his explorations and survey work in the Canadian Arctic. The Victoria medal of the Society has been awarded to Prof. Frank Debenham, professor of geography in the University of Cambridge.

Dr. J. E. Keyston, formerly assistant director of scientific research, Admiralty, has been awarded the United States Medal of Freedom with Bronze Palm for promoting interchange of scientific information and collaborating with American research groups during the Second World War.

The Holweck Prize and Medal of the Physical Society and the Société Française de Physique have been awarded to Prof. Yves Rocard, professor at the Sorbonne, and director of the Physics Laboratory of the École Normale Supérieure, University of Paris.

Other medals awarded during the quarter include the Duddell medal of the Physical Society for 1947 to Dr. R. J. Van de Graaff of the Massachusetts Institute of Technology ; James Alfred Ewing medal of the Institution of Civil Engineers for 1947 to Sir John Cockcroft ; Linnean medal of the Linnean Society of London for 1948 to Dr. Agnes Arber, F.R.S. ; the Bessemer Gold medal of the Iron and Steel Institute for 1948 to Mr. W. J. Dawson, formerly of Hadfield's, Ltd. ; the Sir Robert Hadfield medal of the Iron and Steel Institute for 1948 to Mr. A. Preece, reader in metallurgy in the University of Leeds ; and the Institute of Metals (Platinum) Medal for 1948 to Dr. R. C. Stanley, chairman and president of the International Nickel Company of Canada, Ltd.

General Smuts has been elected chancellor of the University of Cambridge in succession to the late Lord Baldwin.

Air Chief Marshal Sir Roderic Hill has been appointed rector of the Imperial College of Science and Technology from October 1 in succession to Sir Richard Southwell.

Lord Brabazon of Tara has been elected president of the Royal Institution in succession to the late Lord Rayleigh.

Mr. Norris E. Dodd, Under-Secretary of Agriculture in the United States, has been appointed director-general of the United Nations Food and Agriculture Organisation, in succession to Sir

John Boyd Orr, who has retired. The British Minister of Food has agreed to release Sir Herbert Broadley, deputy secretary, Ministry of Food, for appointment as deputy director general of the Organisation.

Dr. Joseph Needham, F.R.S., has left Unesco to resume his work at the University of Cambridge.

Prof. F. J. M. Stratton, O.B.E., F.R.S., has been appointed deputy scientific adviser to the Army Council.

We have noted with great regret the announcements of the death of the following scientific workers during the quarter: Prof. F. O. Bower, F.R.S., emeritus professor of botany in the University of Glasgow; Mr. Griffith Brewer, a pioneer of aeronautics in Great Britain; Prof. R. T. Chamberlin, emeritus professor of geology in the University of Chicago; Prof. T. D. A. Cookerell, emeritus professor of zoology in the University of Colorado; Dr. H. A. Deslandres, For.Mem.R.S., formerly director of the Observatory of Meudon and of the Observatory of Paris; Dr. A. L. Du Toit, F.R.S., geologist; Dr. Bernard Dyer, agricultural chemist; Prof. A. S. Eve, C.B.E., F.R.S., emeritus Macdonald professor of physics in McGill University; Prof. Selig Hecht, professor of biophysics in Columbia University; Sir Arthur Huddleston, C.M.G., O.B.E., formerly director of the Royal Technical College, Glasgow; Prof. A. Lacroix, For.Mem.R.S., permanent secretary for thirty-four years of the Paris Academy of Sciences; Prof. Burton E. Livingston, emeritus professor of plant physiology, Johns Hopkins University; Mr. G. S. W. Marlow, secretary of the Faraday Society; Prof. A. Ogg, formerly professor of physics in the University of Cape Town; Prof. M. C. Potter, emeritus professor of botany in the University of Durham; Prof. E. Waymouth Reid, F.R.S., emeritus professor of physiology in University College, Dundee; Sir Charles Grant Robertson, C.V.O., formerly principal and vice-chancellor of the University of Birmingham; Sir Franklin Sibly, K.B.E., formerly vice-chancellor of the University of Reading; Prof. R. B. Thomson, emeritus professor of botany in the University of Toronto; Mr. Orville Wright, a pioneer of the aeroplane.

The United States Geological Survey has resumed publication of the Geophysical Abstracts after a four-year interval, during which they were issued by the U.S. Bureau of Mines.

The Geophysical Abstracts are published quarterly as an aid to those engaged in geophysical research and exploration. The bulletin covers world literature on geophysics contained in periodicals,

books, and patents. It deals with exploration by gravitational, magnetic, seismic, electrical, radioactive, geothermal, and geochemical methods and with underlying geophysical theory and related subjects.

Copies may be purchased singly or by annual subscription from the Superintendent of Documents, Government Printing Office, Washington 25, D.C. For subscription, the Superintendent of Documents will accept a deposit of \$5.00 in payment for subsequent issues. When this fund is near depletion the subscriber will be notified. The deposit may also be used to cover purchase of any other publication from the Superintendent of Documents. The present price of each copy of the *Geophysical Abstracts* is 20 cents.

The Report of the Water Pollution Research Board for 1946 (H.M. Stationery Office, 1s.) gives an account of the work done on the treatment of water, sewage, and industrial wastes, and on the effects of polluting substances on surface waters. Work is described on the application of DDT and Gammexane to percolating filters at a number of sewage works for control of the small flies which breed in these filters. While both substances are effective in killing fly larvæ, under certain conditions the effluent from a filter thus treated may be toxic, and further research is being undertaken.

Further work on chlorination of sewage effluents is described. It had previously been found that certain effluents, though not containing directly poisonous substances, were rendered toxic to fish when chlorine was added in an amount smaller than the chlorine demand. This occurred when the sewage contained spent gas liquor, and it has now been shown that the toxic compound is cyanogen chloride, formed by the interaction of the chlorine with thiocyanate.

An interesting method for the cleansing of mussels has been developed, in which sea water, after each cleansing operation, is treated with chlorine and then by aeration with diffused air. Aeration removes excess chlorine and replaces the dissolved oxygen which had been removed from the water by respiration of the mussels in the cleansing tank. In semi-scale plant 26 successive batches of mussels were cleansed satisfactorily in the same sea water. The mussels are immersed for 16-20 hours in sea water, and it is found that after two treatments they are free from pathogenic bacteria. Before the above process was evolved, difficulty had arisen because the water was not always sufficiently saline for the treatment, and in any case was only used once, fresh sea water being obtained for each immersion period.

An account of the development and research work carried out by the British Thomson-Houston Co. Ltd., during 1947 is given in the January-February issue of their journal *B.T.H. Activities*.

To assist in the country's efforts to increase production, the company has concentrated much of its attention on the building of new power stations and the installation of more generating machinery in existing stations. Specialised equipment for use in industry, particularly for steel production, is being developed, and a great deal of work is being done in connection with indoor and outdoor lighting using the fluorescent type of lamp.

Of chief interest in the research field is the design and building of a novel form of betatron for the Clarendon laboratory at Oxford. A reduction in power input and weight has been achieved by separating the A.C. magnetic fields required to accelerate and guide the electrons. Two new types of toroid for use in the betatron have been developed. These chambers, in which the electrons circulate over a million times and travel approximately 900 miles, have previously been made from plates and cylinders of glass with waxed joints. One of the new versions is made from glass tubing which is formed into a ring 16 in. in diameter. The other is made in two halves which are then sealed together in a specially built oven to form a ring of tubing of rectangular cross-section.

Progress has also been made in many other fields. A repeatable flashlamp has been developed for photographic work which will give several thousand intense flashes without deterioration, and also a mercury cadmium lamp for use in film studios. The latter is being tried in America as well as in this country, and may possibly change the technique used in Hollywood. Miniature radio receivers have been produced, including a five-valve set complete with battery to fit into the breast pocket and a crystal set so small that it can be mounted inside an ordinary headphone earpiece. Work has continued on the lead sulphide photo-conductive cell and the semi-conductor bolometer for the detection of infra-red radiation. Improvements in the methods of producing silicones are being made, and an electric motor impregnated with silicone varnish has been running for over 900 hours at a temperature of 250° C., under which conditions it is giving three times its rated horsepower.

For aeroplane mapping, whilst a very wide angle camera lens is desirable, the illumination at the edges of the image formed by it is usually only a fraction of that at the centre. During the recent war this difficulty was overcome in Germany by using a special type of lens. An account of the design and operation of this lens is

included in the February issue of the *Journal of Research* of the U.S. National Bureau of Standards.

A distortion-free lens forms an image on a plane surface, the illumination of which varies approximately as the fourth power of the cosine of the angle between the axis and the chief ray corresponding to the part of the image being considered. Thus for a 120-degree field, the illumination at the edge is only one-sixteenth of that at the centre of the image. This is a serious disadvantage when using a black and white emulsion and makes the use of colour film impossible. The illumination over the field could be made uniform by the use of a variable density filter, but the effective speed of the emulsion would be much reduced.

The German development was to make a lens with a large amount of negative distortion. With such a lens, the scale of the photograph rapidly decreases from the centre outward and, as a result, the illumination does not fall off nearly so rapidly. Due to the distortion, straight lines not passing through the centre become curved lines concave towards the centre, and the photographs cannot be used to build up a mosaic without further processing. It is interesting to note, however, that German air crews who happened by chance to have this distorted type of photograph available, found them extremely helpful provided they were flying over the central portion. This was due to the outer distorted parts of the picture suggesting the loss in detail of distant objects when viewed from the air.

To obtain undistorted prints from the distorted negatives, a copying or enlarging lens with a suitable amount of positive distortion is used. This lens, although it corrects the original distortion, introduces non-uniformity of illumination. This, however, offers no great difficulty, as the source of illumination is now under control and can be suitably adjusted.

The article ends by showing an example of the type of photograph obtained with the lens. While the distorted photograph is almost circular in shape, the finished product is square and shows very little distortion. To realise the full possibilities of wide-angle lenses, in this case 130°, ideal atmospheric conditions are required and the use of infra-red plates is probably an advantage.

The *Bell System Technical Journal* for January 1948 contains an account of an experimental electron beam deflection tube which was specially designed for Pulse Code Modulation (P.C.M.). In this system the amplitude of the signal is measured at frequent intervals, and each value is transmitted by means of a group of

on-or-off pulses of constant amplitude. At the receiving end the original signal is reconstructed, the amplitude of the signal at each instant depending upon the arrival or absence of pulses in each group. A note on this type of modulation was included in the January issue of *SCIENCE PROGRESS*.

The essential requirements of a P.C.M. modulator are to measure the signal amplitude to the nearest step on the amplitude scale and then to generate the group of pulses which will represent this amplitude. While this can be done using conventional valves, the electron beam deflection tube has been designed to speed up as well as to simplify the operation.

The design of the tube is similar to the Cathode Ray tube in so far as it has an electron gun and X and Y deflector plates. The screen of the Cathode Ray tube, however, is replaced by a metal output plate, in front of which is placed a code masking plate. The masking plate has numerous square holes punched in it, the positions of the holes being arranged so that they lie on equally spaced vertical and horizontal lines. Only when the electron beam passes through a hole and reaches the output plate does a current pass through the tube. The holes are so arranged, that reading along the horizontal lines all the possible combinations of punched and "unpunched" holes are available. These correspond to the possible combinations of on-or-off pulses available for the groups. Thus if groups of four pulses were used, the holes would lie in four vertical lines and sixteen possible combinations, requiring sixteen horizontal lines, would be available. In operation, a linear sweep is applied to the X-plates and the voltage to be coded to the Y-plates. Thus the voltage to be coded makes the beam move to that horizontal line, which, as it is swept, will give rise to the correct group of pulses required to represent that particular input voltage.

The system as it stands would only work satisfactorily if the electron beam were infinitely sharp. For a finite beam there will be sweep positions where the beam straddles and sweeps out a combination of two codes. This is prevented by introducing a grid of horizontal wires in front of the masking plate, each wire lying between adjacent codes. An electrical feedback system from this "quantizing" grid to the signal deflector plates constrains the beam to the proper level for the code corresponding to the voltage step within which the signal amplitude falls.

Coding tubes of this type have been used very successfully with an experimental multi-channel pulse code system. They require very little attention apart from slight adjustment of potentials not more than once a day.

The *Bell Laboratories Record* for March 1948 contains an account of a new magnetic material, known as Superalloy, which has been developed in the Bell Laboratories. It is the result of nearly fifty years' research work on the problem of finding a material with a high initial permeability, and shows a marked improvement on the range of Permalloys which were developed during and just after the first world war.

The magnetic permeability of a substance varies with the applied field and in the case of silicon steel, for example, ranges from an initial value of about 500 to a maximum value of 10,000. Where large currents are available, as in power generating and transforming equipment, the values of permeability obtainable have been very satisfactory. On the other hand, in telephone and communication circuits, where such large currents are not used, a material with a high initial permeability has long been desired.

The great success achieved with Superalloy far exceeded expectations. Its initial permeability is about 500 times that of soft iron, while its maximum permeability, which is of the order of 1 million, is obtained for a field of less than 0.005 oersteds. Its hysteresis loss is small and shows a great improvement on molybdenum Permalloy, the next best alloy in the range.

During the war, the advent of Superalloy, with its high initial permeability, made it possible to build transformers, for use in radar apparatus, which had a frequency band three times the best previously obtainable. There are many other fields, including television and radio-relay circuits, in which the new alloy will probably be used to advantage in the future.

A Scientific and Philosophical Society was formed in Nairobi a little over a year ago to meet the need felt by the scientific workers in the Government Departments and business firms of Nairobi for a central meeting-place. The interests of the Society cover a wide field as can be seen from the Society's first publication. Papers have been read during the year on the use of the Radio Sonde in the service of meteorology in East Africa, on agricultural subjects and also on topics of sociological interest. That the Society should be interested in sociological questions is very understandable when it is remembered that, as is stated in the editorial, "the Society originated in the capital of a country where the development of a primitive people is taking place at a more rapid rate than has ever before happened in the world's history."

ESSAY REVIEW

ATOMIC ENERGY FOR THE NON-SPECIALIST. By N. FEATHER, Ph.D., F.R.S., Professor of Natural Philosophy in the University of Edinburgh. Being a Review of *The Atom*, by SIR GEORGE THOMSON, F.R.S. Third edition. [Pp. viii + 196, with 15 figures and 1 folding plate.] (Oxford: at the University Press, 1947. 5s. net.), and of *The Atom and its Energy*, by E. N. DA C. ANDRADE, D.Sc., Ph.D., F.R.S. [Pp. xii + 196, with 12 plates and 17 figures.] (London: G. Bell & Sons, Ltd., 1947. 10s. net.)

It is commonplace enough to remark on the difficulties of book-production in the post-war era, but less than two and a half years from the end of hostilities the general reader already has a wide choice of titles dealing with the problems of atomic energy in all its aspects—scientific, political and ethical. Nor is this surprising, for it is equally a commonplace that these problems are among the most urgent with which the peoples and the governments of the world have to struggle. Nevertheless, it is reassuring to find that this spate of printing is not all of rather hastily composed popularisations: there is much to be said on the scientific side for the method of building on to a general account of fundamental atomic physics, written in more peaceful days, that which is startling and new. Against such a background it is easier to see the subject in its true proportions, and easier therefore to reach a sane judgment concerning the imputations which from time to time charge the scientist with responsibility for present ills. Both of the books under review exemplify this method of building-on, though in different degrees—and because of the eminence of their two authors, as well for their origins and mode of construction, it is good to welcome them and wish them a wide circulation.

Sir George Thomson's book is the third edition of a volume of the Home University Library first published in 1930 (second edition, 1937) "brought up to date" and with "chapters added" on nuclear energy; Professor Andrade's is more nearly a new book, though considerable portions of the first six chapters (out of eight) are based on the author's volume in the Nelson's Classics, last

published in 1936. These then are the pedigrees of the books—it is necessary now to look further into their contents.

The contents of Sir George Thomson's book are the more itemised, and the attempt has obviously been made more consciously, in it than in the other book, to present, at the semi-popular level, a text-book of atomic physics in miniature. On the whole it is a successful attempt: the author has taken the bold step of stating categorically in the beginning the basic elements of the modern physicist's views on the structure of matter, introducing the fundamental particles and indicating their respective properties. Then, more leisurely, he has expanded his theme, and here and there given some inkling of the classical experiments which led to the development of the views already enunciated, and to the recognition, discovery or identification of the fundamental particles now accepted. But in general his method has been to show how the theorist's picture explains the experimenter's results, rather than to build up that picture, bit by bit on a blank canvas, by the consideration of brute fact ascertained by experiment. In spite of contrary predilections, one has to admit that the author's method has many advantages, if his aim is the text-book in miniature.

It must always be difficult to bring a small book up-to-date. To do so after ten years when the subject is atomic physics is a well-nigh hopeless task. Revision in two directions will doubtless appear desirable. The author will be tempted to add new chapters dealing with developments which were hardly foreseen at the date of the earlier edition; here the developments in the large-scale utilisation of atomic energy provide the obvious temptation. As already indicated, new chapters have in fact been added to Sir George Thomson's book dealing with this topic. But there is the almost greater need—and the less attractive job—of altering the existing text by a sentence or two here, a word or two there, so as to remove what has become obsolete and substitute what is current in the theoretical picture. Sir George Thomson had the misfortune to write his book originally before the discovery of the neutron and, in spite of the production of a second edition in 1937, his third edition still shows signs of its early origin. Thus (p. 40): "The explanation has come since the War as a result of the remarkable experiments of Dr. Aston," and (p. 42): "the much more accurate one that has since been used by the American physicist, Dr. Millikan"—there is an air of up-to-dateness about both these extracts to which neither the early experiments of Aston nor the classical determination of the electronic charge by Millikan can now lay claim. The phraseology here, merely as a result of the passage

of time, gives us distorted history : just occasionally, too, there are distortions which appear to represent the author's misreading of the history of science. There is clear evidence (pp. 87, 89) that he supposes the proton to have been regarded as an ultimate particle as early as 1911 ; in fact, from a note found in Rutherford's papers it would appear that he himself looked upon 1920 as the year in which the term was first introduced. Perhaps all this is carping criticism ; the reader must decide on the basis of his own reactions, but he should be warned that he will also find a number of places in this generally useful book where the writing gets a little breathless, where, if he is not thoughtful, he will probably lose the even thread of the story. Thus (p. 71) : " The rays must be regarded as producing ionisation by the brute force of their impact, which bores its way through the molecules, knocking out some of the electrons with which it happens to come into particularly close collision " ; or (p. 79) : " However strong the light the electrons came off no faster ; the only difference was that there were more of them," or, when he has just been told (p. 110) : " If the waves pass over an obstacle like an atom, their direction is modified and the modification is transmitted back to the electron and affects its path," he reads on the next page : " The electron waves as such cannot transmit a signal." Popularisation truly is an art for which eternal vigilance is necessary in the artist !

There is one further comment on Sir George Thomson's book. He is at pains to describe the operation of the expansion chamber and he finds it useful to refer time and again to the evidence which cloud-track photographs provide more convincingly than does any other type of experimental " result." It is all the more pity that he does not reproduce a single track photograph for the benefit of his non-specialist readers. Perhaps they will have been lucky and have seen such photographs before, but no apology would be needed, surely, in showing them again.

The remarks just made regarding illustrations do not apply to Professor Andrade's more expensive book (here, perhaps, is the answer to the reviewer's regrets) ; he gives us the cloud-tracks, and he even gives us a reproduction of a sixteenth-century Dutch master, but he, too, has had some trouble in bringing his old text up-to-date. It must be realised that this is a very personal little volume. There is a " moral " *motif* running through it, and many signs of pleasant idiosyncrasy. The moral is that the " successful " investigator gets the Nobel Prize in the end—as in the *Fairy Story Book*. But, unluckily for Professor Andrade's consistency, some of the characters of his 1936 collection have received the prize in

the interim—and he has failed to record their “success” in his latest edition. “Wot, no Nobel Prize?”, one reviewer wrote three times in the margin of the page. But he thoroughly enjoyed reading every word of the book.

In the final chapter on Atomic Energy, Professor Andrade goes beyond basic physics, and is solemn, daring and jocular in turn. He is solemn because he realises that “the atomic discoveries have a straight and immediate influence on the life, and the chances of life, of each of us”; he is daring in that he quotes a reputed figure (to 1 per cent. “accuracy”) for the amount of U-235 in the uranium bombs; he is jocular because he cannot help recalling that Rutherford, more than forty years ago, jokingly suggested “some fool in a laboratory might blow up the world unawares.” In 1928 it fell to the present reviewer to provide the traditional alphabet for the Post-Prandial Proceedings of the Cavendish Physical Society. The annexed quotation will show how the shadow of East and West crossed his playful vision even these twenty years past :

Z is for Zink with a K as Kapitza
Obtained from Kahlbaum—
And that you'll admit, Sir,
'S a little bit odd; never mind, it's the last,
It only remains for some heedless enthusiast
To excite in the atom an Omega Ray
And herald the dawn of the Ultimate Day.

“Heedless enthusiast!” the sad reader will exclaim—perhaps, in the upshot, “reluctant dragon” more nearly describes what happened.

REVIEWS

ASTRONOMY

Galaxies. By HARLOW SHAPLEY. [Pp. viii + 229, with frontispiece and 126 figures.]

The Story of Variable Stars. By LEON CAMPBELL and LUIGI JACCHIA. [Pp. vi + 226, with frontispiece and 82 figures.]

Between the Planets. By FLETCHER G. WATSON. [Pp. vi + 222, with frontispiece and 106 figures.]
(Philadelphia: The Blakiston Co.; London: J. & A. Churchill, Ltd., 1947. 18s. net each.)

THESE books are reprints of three of the eight Harvard Books on Astronomy, written by members of the staff of Harvard College Observatory (all themselves practising astronomers) for everybody who is interested in astronomy, whatever his technical background. Printed and illustrated in princely fashion, they summarise the present position in the several fields they cover in authoritative though popular style.

In *Galaxies*, one of the most active investigators of the extragalactic nebulae brings together the results of a lifetime's work (that of others as well as his own) into a connected whole. Starting from a discussion of our closest neighbours, the Magellanic Clouds, Shapley proceeds, with only brief diversions explaining how galactic distances and luminosities are determined and how our own Milky Way system is explored, first to the local cluster and then out into intergalactic space until the whole metagalaxy is brought under review. In a final chapter the thorny question of the interpretation of the red-shift and its relation to various cosmogonies can of course hardly be more than touched upon in a book of this kind. If this last chapter leaves the reader in somewhat of a maze, he may be comforted with a whisper that much the same may be said of the cosmogonist faced (as he still is) with observations whose critical evaluation is even yet a matter of dispute. The book may be recommended as an excellent descriptive introduction to the study of the external galaxies.

The Story of Variable Stars is much more than its title suggests. After introducing the reader to the technique of observing variations in stellar brightness and to the many different classes of variables, the authors go on to give a fascinating account of how stellar variability can be used for studying the outer atmospheres of stars (in the eclipsing variables), their deep interiors (in the Cepheids) and for measuring galactic and extragalactic distances (via the period-luminosity relation in the Cepheids). A chapter on novae, supernovae and similar explosive stars is a model summary of the observed facts which will prove useful even to professional astronomers whose special study lies outside this field. This book will be read with the

liveliest interest by all amateur astronomers, for it shows clearly what a vital part amateur observations have played and are still playing in our attempts to unravel some of the extremely tangled problems set by stars whose brightness varies owing to intrinsic causes.

The third book under review differs from the others in being the only published work yet available in its particular field: it is devoted entirely to summarising present-day knowledge of comets, asteroids, meteors and meteorites. The average textbook dismisses these objects in a few pages, and no technical monograph covering the field exists. As a result this book is already something of a classic, and with the rapid expansion of the radar method of studying meteor streams (a method which incidentally finds no mention in its pages—the volume reviewed is no more than a reprint of the first edition published in 1941) it is being extensively quoted in the current periodical literature. Such a state of affairs would indeed be deplorable were the book other than it is: a lucid and authoritative summary, illustrated by much detail presented in tabular form, of our present knowledge of comets and meteors and of the connection between them. As it is, professional, amateur and layman alike can find much of interest in its pages.

The books have, in common with the others in the series, uniformly startling dust-covers that the sensitive reader will promptly discard—the stout bindings make them quite unnecessary, anyway—and a profusion of portraits of astronomers which take up valuable space that might surely have been better used in giving us still more of the excellent celestial photographs with which the books are already packed. At their very modest price these books should find their way on to the shelves of all those who want up-to-date informative accounts of modern astronomy in its various aspects.

A. H.

PHYSICS

Sound. A Physical Text-book. By E. G. RICHARDSON, B.A., Ph.D., D.Sc.
Fourth edition. [Pp. viii + 344, with 118 figures.] (London: Edward Arnold & Co., 1947, 18s. net.)

THAT a fourth edition is called for only seven years after the third is good evidence of the value of this book. No large changes have been made, but Dr. Richardson has endeavoured to bring the whole text up-to-date to give a correct impression of the present stage of a subject now in active growth.

It is always very difficult when revising an established text to decide what to add and what to leave out. In a future edition, Dr. Richardson might like to consider including a brief note on the use of flash discharge tubes in stroboscopes and Schlieren photography; later references to devices for carrying out Fourier analyses; diagrams showing the principles of the harp pedal and pianoforte action; recent work on the effect of "stiffness" on the vibration, tuning and tone of piano strings; more illustrations of the vortices involved in edge tones; some examples of acoustic spectra; a note on formants; the use of supersonics in the detection of flaws in solids.

Others interested in the subject will have their own lists of omissions, but there is no doubt that the book as it stands is a well-balanced text very suitable for students of physics.

F. A. V.

Electricity. Text Book and Laboratory Manual for Inter. B.Sc., Higher School Certificate, University Scholarships, etc. By M. M. DAS, B.Sc., A.Inst.P. [Pp. xii + 483, with 303 figures.] (London : Thomas Murby & Co., Ltd., 1947. 25s. net.)

THIS book shows that Mr. Das is an experienced and conscientious teacher, aware of most of the pitfalls encountered by earnest students. With some exceptions his explanations are very full, in places even leading to repetitions which are hardly necessary. A knowledge of physics to about School Certificate standard and some acquaintance with elementary calculus are assumed. The standard reached is about that of Higher School Certificate, but some additional work would be necessary for scholarship papers. On the whole, the treatment is up-to-date, though the later chapters on cathode-rays, X-rays, etc., are somewhat sketchy. The chapters on geomagnetism and the magnetic properties of materials are unusually full for a book of this kind.

There are a number of places in which the text could be improved, in addition to the elimination of repetitions and various verbal slips. For example, Sucksmith's form of suspended magnet magnetometer is better than the one described (p. 32); it is desirable to explain that "frictional electricity" has nothing to do with friction (p. 51, *e.g.*); the earth connection is omitted from Fig. 24 (p. 107); it should be pointed out that the results of applying Newton's Law of Cooling on p. 229 are much in error unless the temperature rise is very small or forced convection used; the methods described on pp. 242-4 for comparing E.M.F.s of cells are obsolete; "specific-conductivity" should be replaced by "conductivity" or "specific conductance" (p. 333); some account of the electrode-potential system in a Leclanché cell, always puzzling to a student, should be included (p. 347); as given in equation (1) of p. 440, A is not a universal constant and even if it were its value would not be 60.2.

Included in the text are brief instructions for over 100 experiments to be carried out by student or teacher, and at the end of each chapter collections of examples, taken mainly from published examination papers.

F. A. V.

Theory and Applications of Electricity and Magnetism. By CHARLES A. CULVER. [Pp. viii + 594, with 407 figures.] (New York and London : McGraw-Hill Book Co., Inc., 1947. 25s. net.)

A BOOK which aims at serving as "a basis for advanced study in physics and chemistry and also to lay the foundation for courses in electrical engineering" must, almost by the nature of its attempted scope, find itself criticised for its omissions if for nothing else. The chemist might claim that he has to be satisfied with one or two applications of electricity to chemical technology and a discussion of "pH," while the physicist might complain that the continuity is broken by the frequent interpolation of practical applications. So it may be; but only too often the physics student is ignorant of some of the simple applications of electricity, such, for instance, as the electrical motor and generator.

The order of presentation is somewhat different from normal, as the procedure is dictated by a desire to keep the practical outlook to the fore. This is not entirely undesirable and is often much more welcome to the beginner than a more formal and logical treatment. The book would serve

as a very useful text for pass degree students, although requiring to be supplemented in places, and would act as an excellent companion volume to a more formal treatise for the honours student. The mathematical treatment is kept to a minimum without avoiding some of the more difficult problems, and in many cases an enlightening discussion takes the place of a lengthy calculation. A good selection of problems of varying degree of difficulty follows most chapters.

There are many small points that could be criticised, such as the bare statement of Ohm's law that the current in a conductor is proportional to the potential difference. Also a puzzling statement appears on p. 148; it is there stated that of the three principal effects resulting from an electric current, *viz.* (1) the production of heat, (2) the causing of chemical changes and (3) the production of a magnetic field, only the first is always present. To say the least, this gives the impression that there are cases when an electric current is *not accompanied* by a magnetic field! One further criticism is that a number of the photographs of actual pieces of apparatus are badly reproduced and thus much of their value is lost.

There are also some misprints and slips, such as the misspelling of Dunnington's name both in the text and the index, and the misstatement on p. 48 about paramagnetics "whose susceptibility is somewhat greater than unity and whose permeability is correspondingly in excess of unity."

The scope of the book is wide, covering a large range of applications of the thermoelectric and electrochemical effects, as well as treatments of motors, generators and transformers. The last 180 pages are devoted to modern physics and electronics.

W. E. DUNCANSON.

Electromagnetism. By J. C. SLATER, Ph.D., and N. H. FRANK, Sc.D.
[Pp. xiv + 240, with 40 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 17s. 6d. net.)

In 1933 Slater and Frank published their *Introduction to Theoretical Physics*; and now, fifteen years later, they have decided to divide it into three distinct parts, each complete in itself, of which this is the second; the others are *Mechanics* and *Quantum Theory*. The material contained in the present volume represents about 140 pages of the original comprehensive treatise now expanded to about 240, and brought up-to-date by the inclusion of recent work on cavity resonators. As a result of the expansion the material becomes more digestible; and the new book continues the tradition of readability and reliability so characteristic of its progenitor.

The subject matter is eminently suitable for a good second-year honours physics class, though some of the students might find the mathematics a bit heavy. And, since more than half the space is devoted to waves, including a good account of physical optics, some people may wish that the earlier more elementary ground had been more fully covered. Thus, there is hardly any mention of current flow in conductors (Kirchhoff is not even mentioned in the index), no bridge circuits, and practically no use made of the powerful, though mathematical, technique of conformal representation.

The fundamental electric vectors are now taken to be $\mathbf{E}$ and $\mathbf{B}$, with $\mathbf{D}$ and $\mathbf{H}$ deduced from them. The thorny question of magnetism is introduced by the Biot-Savart law, which avoids all mention of Oersted's coils.

(These are only referred to, in passing, in the excellent introductory ten pages.) This technique has many points in its favour, though it has not yet won complete approval in Britain.

One other point must be mentioned; the units used are the mks rationalised set. This system is evidently acceptable in America, but it has hardly achieved acceptance over here with us, and to those who do not find it familiar it is a thorn in the flesh. Fortunately, an excellent appendix relates together the various systems of units.

This is a very pleasant book which it is a pleasure to recommend. What a pity that it costs dollars!

C. A. COULSON.

Nuclear Physics in Photographs: Tracks of Charged Particles in Photographic Emulsions. By C. F. POWELL and G. P. S. OCCHIALINI. [Pp. xii + 124, with 51 plates and 16 figures.] (Oxford: at the Clarendon Press, 1947. 18s. net.)

THIS beautiful collection of pictorial records of nuclear processes obtained by the photographic emulsion method will find a place on the shelves of all who are interested in displaying the progress of nuclear physics. It is difficult on general grounds to select one or more photographs for special praise, because the choice depends on the particular problem in which the observer is interested. In addition, further subjectivity arises in that this is a work of art as well as being a valuable scientific record. The explosive disintegration of a nucleus recorded in the frontispiece is exceptionally striking. On the other hand, perhaps the most exciting and certainly the most topical are photographs 42-50, showing meson tracks. The orientation of the plates is capricious; sometimes the primary particles enter from below, sometimes from above, and in plates 12 and 13 they enter from right to left and from left to right respectively.

The descriptive text is less successful. For example, it would not be generally agreed that, as the preface implies, schoolboys have now arrived at a stage when it is preferable to explain billiard balls in terms of nuclei, rather than nuclei in terms of billiard balls. There are many examples of looseness of phrasing and in some cases words are used incorrectly. On p. 3 the word "extraordinary" should presumably read "noteworthy," for the occurrence of a regular pattern is one of the most ordinary of physical phenomena. On p. 64 the word "speed" is used where "velocity" would be more accurate. On p. 36 it is somewhat odd to invite inspection of something which cannot be seen. Contrary to the statement on p. 57, improvements in technique have now made it possible to record disintegrations on almost every cloud chamber photograph. It also seems rather a superficial comparison to regard the $\text{Li}^6(n, \text{H}^3)\text{He}^4$ reaction as a prototype of "fission," since this is not essentially different in kind from the $\text{Li}^7(p, \text{He}^4)\text{He}^4$ reaction of Cockcroft and Walton. The true fission process with heavy nuclei is probably very different. On p. 71 it appears that the text should read $E - 0.35$ and not $E + 0.35$.

These minor blemishes are capable of easy correction, and since this excellent work is likely to become a classic they may well be removed in future editions.

F. C. CHAMPTON.

Electrons (+ and -), Protons, Photons, Neutrons, Mesotrons, and Cosmic Rays. By R. A. MILLIKAN. Second edition. [Pp. x + 642, with 124 figures.] (Chicago: The University of Chicago Press; London: Cambridge University Press, 1947. 30s. net.)

THIS latest edition of Professor Millikan's classic work *The Electron* now includes a large part of the domain of nuclear physics, cosmic rays and allied phenomena. The first half of the book is mainly concerned with the properties of the electron and the extra-nuclear structure of the atom; the second half of the book is mostly devoted to cosmic rays, but it includes two excellent chapters on the neutron and on the release of nuclear energy.

The emphasis, both in content and in interpretation, is placed on the magnificent and sustained contributions made to the advancement of the subject by Professor Millikan and his brilliant collaborators in the U.S.A. The author's great skill as a teacher is reflected in his style, which has the merits of directness, vigour, simplicity and the stressing of the high-lights, usually with the assistance of italics. In addition it is as exciting as an explorer's log-book and no physicist can fail to gain something valuable both in facts and mental outlook from a study of this work.

While subscribing, however, to the author's "profound belief in the historical approach both in science and in its teaching," it would seem that this has been carried too far in the present edition. The reviewer may have become austerity-minded, but a saving in paper and cost of at least one-third might have been achieved, together with a gain in conciseness, if less time had been devoted to the description of experiments long-since invalidated and to interpretations which are now of interest only to the scientific antiquarian. Again, it is generally accepted that the terms negative and positive electron refer to definite entities and not, as the author frequently implies, simply to the quantity of electricity equal to the electronic charge.

In an unsuccessful attempt to be comprehensive the title has now become unwieldy. Most of the photographic reproductions of cloud tracks are excellent, but exception may be taken to the faintness of those on pp. 344 and 345. It is perhaps desirable that, after the original untouched publication of such photographs, further reproductions in textbooks and general reviews should be touched up if necessary to aid clarity. There are a few printer's errors; for example, Blockett for Blackett on p. 510, and portion for proton on p. 519.

With regard to the control of the atomic bomb, Professor Millikan is to be congratulated on remarks which will remain timely and urgent until a genuine solution is achieved.

F. C. CHAMPION.

What is Atomic Energy? By K. MENDELSSOHN, M.A., Ph.D., F.Inst.P. [Pp. 180, with 12 plates and 37 figures.] (London: Sigma Books, Ltd., 1946. 6s. net.)

THIS little book is No. 3 of the "Sigma Introduction to Science" series. "Its aim is to give with a minimum of technicalities a broad survey of the field of atomic physics." It may be said at once that this aim has been achieved, and that the non-scientist reader who follows the text with the close attention it deserves will finish with a good general qualitative knowledge of the principles of nuclear physics. The story, often as it has been told, proves as fascinating as ever: the facile apprehension of the trained

reader makes it difficult to evaluate the effort needed by the untrained to pursue closely reasoned arguments in smallish type over 180 pages, however clear the exposition and apt the illustrations. Popular scientific best sellers reach astronomical circulations: it may be permitted to wonder to what extent these are assured by the insatiable curiosity of the professional scientist.

The present volume has intriguing figures and diagrams well drawn by Victor Reinganum and the author; the plates are excellently reproduced from a representative set of photographs, many of which are not usually available. A new and pleasing feature in a book of this kind is the appeal to diagrams of energy troughs, hills, and valleys. The attentive reader who has had his scientific curiosity awakened may, however, well ask what was the initial temperature of the 65 kettles of water brought to boiling by one kilowatt-hour, and, in the same Fig. 7, consider that the unspecified 10 anti-aircraft shells launched into space by the same unit of energy might be 40-mm. Bofors shells, or 88-mm. shells with a ceiling of over 50,000 feet. Although the book is well printed on good white paper, it is marred by hurried proof reading; there are many more errata than are recorded on the sticker. A feeling of revulsion induced by finding the word "phosphorus" misspelt seven times out of nine on two consecutive pages was removed this morning when a casual turning over of the original edition of Horace Walpole's *Memoirs* revealed that the great man shared the views of most undergraduates on the spelling of "separated."

C. R. B.

Meet the Atoms: A Popular Guide to Modern Physics. By O. R. FRISCH. [Pp. xiv + 226, with 26 figures and 7 plates.] (London: Sigma Books, Ltd., 1947. 12s. 6d. net.)

No physicist who has followed recent progress can have any doubt that few are better qualified than Professor Frisch to write about atoms. This book shows he is also a brilliant expositor. Even with these qualifications the book would not have reached such a high standard if Professor Frisch had not obviously spent a great deal of time and thought in finding the right words, the best examples and in deciding just what to leave out. As Lise Meitner says in the Introduction, the result "makes me think of the fine classic lectures by Tyndall," and also of Sir Oliver Lodge and Sir William Bragg at their best.

After reading the book the layman should have a good idea of how the physicist (particularly the atomic and nuclear physicist) goes about his researches, why he is so sure about some of his conclusions and so diffident about others. Details of specimen calculations are given in such a way that anyone who has done Fourth Form mathematics, however long ago, and is prepared to do a little thinking, can follow them.

This friendly book can be commended without reserve to professional physicists to admire the way in which Professor Frisch has carried out his task (and to learn some tips for their own teaching), to all between the ages 15 and 95 who wish to learn something of the background of present-day atomic and nuclear physics, and to be given as a present to anyone who believes he is not interested in science!

The book is well-printed (in America, though published in this country) and the few remaining misprints are trivial.

Will Professor Frisch please spare a little time and energy from his new and exacting duties at Cambridge to write more books to delight and instruct us.

F. A. V.

The Electron Microscope. By V. E. COSSLETT, Ph.D., M.Sc., M.A., F.Inst.P. Sigma Introduction to Science, 8. [Pp. viii + 128, with 12 plates and 40 figures.] (London: Sigma Books, Ltd., 1947. 7s. 6d. net.)

THIS is a welcome addition to the steadily growing popular library on the subject of the electron microscope. It gives a concise and comprehensive survey of the types of electron microscopes now in use and is illustrated by some excellent electron micrographs. It must, however, be said that after the elementary nature of the first chapter, some parts of Chapters 2 and 3 will demand a considerable effort at understanding on the part of the general reader. The discussion in Chapter 3 of the action of the electro-magnetic lens is excellent and contrasts favourably with the poor treatment usually meted out to the subject in other popular books.

It seems to me that rather too much space has been devoted to the description of the more complex scanning electron microscope which, as yet, has hardly reached a useful stage of development. Instead, I should have welcomed some account of the new three-stage instruments such as, for example, the model at Delft in which many unique and interesting features are incorporated. Also, the subject of electron diffraction, which is, in itself, an all-important branch of electron optics, deserves a more adequate exposition than that given in Chapter 2. These, however, are omissions of which the general reader is hardly likely to be aware and he will find his interest aroused and stimulated by this book. It should reach a wide public.

G. INGLE FINCH.

Micro Waves and Wave Guides. By H. M. BARLOW, B.Sc.(Eng.), Ph.D. [Pp. x + 122, with 70 figures.] (London: Constable & Co., Ltd., 1947. 15s. net.)

THE greater part of this little book is devoted to a consideration of the propagation of electro-magnetic waves in wave guides. The subject is first approached in a physical way by considering the guide as made up of suitable elements of transmission lines, and the wave travelling along it as being composed of plane waves reflected back and forth between the boundaries. The usual mathematical development, employing Maxwell's equations with the appropriate boundary conditions, is then given in full for guides of different shapes. A preliminary chapter (only seven pages) surveys the methods of generating micro waves, in a purely descriptive manner, and a final chapter of seventeen pages deals sketchily with those techniques which are involved in using wave guides, such as methods of launching the wave, irises and other obstacles, piston attenuators, wave-guide junctions, and the radiation from slots.

The book can be recommended as a concise and accurate account of the ordinary mathematical theory of propagation in guides. It might have been more valuable if the mathematical theory, which is now well-known, had been dealt with at less length, and if some of the practical points dealt with in the last chapter had been considered more fully. If this had been done

there would be more justification for the claim, made on the dust cover and in the preface, that "a special effort has been made to capture the attention and interest of those whose outlook is essentially practical." But as a straightforward mathematical account, both of rectangular and circular guides, the book will be found useful and concise.

J. A. RATCLIFFE.

Radar System Engineering. Edited by L. N. RIDENOUR. [Pp. xviii + 748, with 446 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 37s. 6d. net.)

DURING the war, a very large team of workers was assembled at the Massachusetts Institute of Technology to undertake research and development on radar and allied problems, the organisation being termed the Radiation Laboratory. At the conclusion of hostilities, the Radiation Laboratory was disbanded, but some of the experts remained for a time to write up a comprehensive account, both of their own work and of that done by others in America and Britain. The volume under review is the first of a proposed series of 28 volumes, under the general editorship of Professor Ridenour. According to his preface, the other volumes will discuss the developments in micro-wave and pulse techniques, etc., without limiting the applications to radar.

Radar System Engineering surveys the general problems of radar from the engineering-design point of view. The factors affecting maximum range, the nature of the reflections from actual targets, the choice of wave-length and pulse duration, and the set-up of the organisation to make use of the radar information, are amongst the general topics discussed. The principles of the transmitting, receiving and indicating circuits are then thoroughly treated, though each of these will be the subject of one or more of the subsequent volumes.

The history of radar is discussed in an interesting way and, as far as one with no war-time knowledge of radar can tell, due credit is given to British work. Owing to the wide field covered in this volume, no less than thirty-two specialists contributed to it, and it says much for the editorial work that the result is a well-written, continuous treatment of the subject. The volume is well produced, with good figures. The set of photographs of P.P.I. displays is especially interesting to those who have not had an opportunity to see aircraft radar in action.

If the remainder of this series fulfils the promise of the first volume, the result will be an authoritative and pleasing account of work in these fields.

C. R. S.

Applied Thermodynamics. By A. C. WALSHAW, Ph.D., M.Sc., D.I.C., A.C.G.I., A.M.I.Mech.E. [Pp. xi + 401, with 180 figures and one folding plate.] (London: Blackie & Son, Ltd., 1947. 30s. net.)

As the author states in his introduction, his book is intended for those who have already taken an elementary course in the subject and are studying for Part II of the B.Sc.(Eng.) or the examinations of the Professional Institutions.

It is perhaps a little questionable whether the first of the four sections into which the book is divided is not too advanced for the average student at this point in his studies. The experience of most lecturers is that students of engineering do not take readily to the notion of the mol. Much of the

third chapter, therefore, which deals with combustion, might be better left to the end of the course. A similar remark applies to the discussion on the Consequences of Kinetic and Quantum Theories.

In his chapter on heat transmission, the author gives a lucid digest of the work of Lander on the Laws of Convection.

The remaining three sections are quite within the grasp of the student. The first of these deals with steam plant and includes boilers, condensers, engines, reheating and feed-heating circuits, nozzles and turbines. Full account has been taken of the boiler feed-pump work, unusual in such texts, but essential in connection with modern high-pressure boilers.

Dr. Walshaw has made a special study of the losses of heat from engine circuits. This is of importance, but possibly overstressed in the text in comparison with the small amount of space devoted to the Willans Line, which is so important in problems involving the fuel economy of plant.

Part III deals adequately with the internal combustion engine under the headings of cycles and operation, including the gas turbine, carburation, combustion, power developed and testing, while a study of compressors and refrigerators forms Part IV.

There is a comprehensive set of worked examples to clarify the theory and a set of questions, with answers, on each chapter. The diagrams are excellent and the whole work scholarly.

B. LLOYD-EVANS.

Mathematical Theory of Rocket Flight. By J. BARKLEY ROSSER, Ph.D., ROBERT R. NEWTON, Ph.D., and GEORGE L. GROSS, Ph.D. [Pp. viii + 276, with 41 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 27s. net.)

WHEN the Americans entered World War II they turned to a consideration of the newer types of armament then being developed by government scientists in Great Britain, and in particular they turned to "rockets." With characteristic enthusiasm hundreds of American chemists, physicists, engineers and mathematicians set to work further to develop the techniques on which only a handful of British experts had been engaged. For the guidance of newcomers some American experts periodically issued expository reports. As time went on, new discoveries were made and ideas were clarified; the reports were then revised and re-issued. The final reports were extremely good. The book now under review is an amplified version of that section of the general report which deals with the "mathematical theory of rocket flight." In the preface, acknowledgment is made of the indebtedness of the three authors to unnamed British scientists, to many unnamed, and to a few named, American scientists.

It is worthy of note that, although the book purports to deal with only one aspect of the motion of rockets, it was written by a team of people who are, respectively, a professor of mathematics at Cornell University, a member of the technical staff of the Bell Telephone Laboratories, and a research engineer in applied mathematics of the Grumman Aircraft Engineering Corporation. The work demands co-operation of this kind.

The book bears the impress of the need which it was designed to satisfy. It is well-written, lucid and discursive. It is well worth having, both for its intrinsic value and also for the fact that it is, to the best of my knowledge, the first detailed account of the subject which has been made available to

the general public. The volume is primarily for mathematicians and engineers interested in ballistics, though the general scientist-in-the-street will find much of the non-mathematical parts of the book of interest, in that these parts will give him an idea of "what makes a rocket go." The paper, printing and binding conform to the standard of excellence usually associated with "McGraw-Hill."

The book contains five sections and five appendices. The appendices are interesting, but specialised. Apart from Chapter V, which is entirely mathematical, the sections have been written so as to contain a general descriptive account of some of the principles of rocket design adopted by the Americans. They also go into some detail over difficulties encountered in attempts to systematise ideas and explain experimental results. The discussions centre round a simplified mathematical model of the problem; the simplifications are necessary in order to make mathematical analysis possible, but are not too far removed from reality to give meaningless results.

Chapter I deals with the general equations of motion of a rocket which is made to follow its path by the addition of fins at its tail, and indications are given of the way in which the equations of motion must be modified if a slow spin about the longitudinal axis is superimposed. The account needs careful consideration, dealing as it does with the motion of a body of varying mass. The actual motion of the rocket is then considered in three stages:— (i) while on the projector (Chapter IV), (ii) while in the air, but with the fuel or propellant still burning—that is while the rocket is still being accelerated (Chapter III), and (iii) while in the air, but with the fuel or propellant completely consumed—that is, when the rocket is subject only to the influence of gravity and aerodynamic resistance. Chapter V deals with some of the mathematical functions which arise in the work; these functions are intimately connected with Fresnel integrals, but are more suitable for interpolation.

The primary objects of the book are, of course, to lay down the principles upon which calculations can be made to determine the trajectories of rockets, and also to elucidate and to assess the factors which contribute to the well known characteristic of rocket projectiles, that they deviate from their theoretical trajectories much more than do shells. The first objective does not pose a very difficult problem; the second one does. The Americans have not, so far, been able to advance a theory to account for the observed dispersion of rockets in the air in terms of measurements on the rockets taken before launching. Nor, for that matter, have the British—yet! Perhaps the peace, or other developments in armaments, have diverted the attention of government scientists to more pressing matters, so that perhaps the problem will never be solved.

L. ROSENHEAD.

CHEMISTRY

Dissociation Energies and Spectra of Diatomic Molecules. By A. G. GAYDON, D.Sc.(Lond.). [Pp. xii + 239, with 4 plates and 39 figures.] (London: Chapman & Hall, Ltd., 1947. 25s. net.)

ALTHOUGH compilations of the data on dissociation energies derived from the analysis of spectra have appeared from time to time in treatises on molecular spectra, this book represents the first attempt to review the subject of dissociation energies as a whole. In this respect Dr. Gaydon's book

must be very welcome to many, but especially to physical and industrial chemists, for whom reliable values of dissociation energies are vital.

The book may be divided into four parts roughly equal in length. The first quarter contains a review of the theory of atomic and molecular spectra, including a short chapter on potential energy curves. The second quarter is concerned with the general determination of dissociation energies from spectroscopic data, *e.g.* convergence limits, Birge-Sponer and other extrapolations and predissociation phenomena. This is followed by two chapters on methods of determining dissociation energies by electron impact and thermochemical means. The remaining quarter of the text is devoted to detailed discussions of the dissociation energies of certain individual molecules of special interest, *e.g.* nitrogen, nitric oxide, carbon monoxide and the much-disputed latent heat of sublimation of carbon. Finally, a list of recommended values for the dissociation energies of about 250 molecules is given, together with references to the data on which each value is based and brief explanatory or critical notes on the data.

The principal value of this book undoubtedly lies in its comprehensiveness. The critical manner in which a very extensive and complex collection of spectroscopic data have been sifted out and presented in relation to the problem of dissociation is entirely admirable. In cases where controversy still exists, the author has clearly tried to give a fair presentation of different points of view before coming to his own conclusions.

In the opinion of this reviewer it is perhaps unfortunate that so much of the early part of the book is devoted to a routine introduction to molecular spectra. Since many better introductions to this subject already exist, this space might have been more profitably employed to probe more deeply into the theoretical aspects of some of the problems discussed.

G. B. B. M. S.

Chimie Organique. Vols. I and II. By A. KIERMANN. [Vol. I: pp. 220, with 19 figures; Vol. II: pp. 195, with 7 figures.] (Paris: Librairie Armand Colin, 1947. Frs. 100.— each.)

It is an encouraging sign to receive textbooks from post-war France, and the present two small volumes will doubtless form a welcome addition to the organic chemical literature of that country. The first of the volumes is largely devoted to theoretical considerations, while the second deals with simple functions, and a third volume on complex functions is contemplated to complete the series. The books, like so many French works, have paper covers, are uncut and lack a comprehensive index. The first volume should help to fill the gap in the French literature on the elementary theoretical aspects of organic chemistry, especially as a short bibliography of larger works is included at the end. The range of subjects discussed is quite wide, although the usefulness of such chapters as those on the historical evolution of organic chemistry (pp. 5), syntheses and degradations (pp. 6) and oxidation and reduction methods (pp. 4) is open to question.

The comparatively extensive treatment of the general aspects of organic chemistry in a book of this size tends to make the second volume very brief by comparison and only the very elementary facts of the subject are discussed. The subject matter is treated according to the nature of the functional groups, which are arranged, following hydrocarbons, as monovalent, *e.g.* halides, alcohols, ethers, amines, etc.; divalent, *e.g.* carbonyl com-

pounds, imines; trivalent, *e.g.* acids, nitriles; tetravalent, *e.g.* carbonates and such compounds as carbon dioxide, carbon tetrachloride, etc. Each function is discussed with reference to the aliphatic, alicyclic and aromatic series respectively and a final chapter deals with a few examples of heterocyclic compounds.

The existence of the numerous recent textbooks in the English language on elementary organic chemistry makes it improbable that these volumes will have a wide appeal in this country or America, but the work is worthy of note for the unusual emphasis placed on the theoretical background and general principles of the subject.

A. W. JOHNSON.

The Terpenes. Vol. I: The Simpler Acyclic and Monocyclic Terpenes and their Derivatives. By J. L. SIMONSEN. Second edition revised by J. L. SIMONSEN, D.Sc., F.R.I.C., F.R.S., and L. N. OWEN, B.Sc., Ph.D., F.R.I.C. [Pp. xvi + 479.] (Cambridge: at the University Press, 1947. 30s. net.)

THE chemistry of the terpenes constitutes an almost indispensable introduction to the study of more complex natural products such as the carotenoids, the steroids and the triterpenes, and it is essential that chemists should be kept abreast of all recent developments in this classical branch of organic chemistry. The appearance of a second edition of this standard work, which has already earned a deservedly high international reputation, is indeed welcome. In its general character, plan and scope the book remains unchanged, but the original text has been most carefully revised in the light of recent advances and much new material has been introduced.

It is interesting to note that many of the additions result from the development of new physical and chemical methods and their application in the terpene field. Thus recent progress in the determination and interpretation of light absorption data has thrown new light on many structural problems, and terpene chemistry has profited by the application of the Diels-Alder reaction, oxidation with selenium dioxide and lead tetra-acetate, etc., to mention but a few. In spite of the many alterations and additions which have been made, the liberal provision of graphic formulæ, such a notable feature of this work, has not suffered in any way, and a more comprehensive index adds further to its value.

E. R. H. JONES.

The Methods of Cellulose Chemistry. By CHARLES DORÉE, M.A., D.Sc., F.R.I.C. Second edition. [Pp. xii + 543, with 81 figures.] (London: Chapman & Hall, Ltd., 1947. 42s. net.)

THE first edition of this outstanding contribution to the literature of cellulose was published in 1933 and reviewed in *SCIENCE PROGRESS* in 1934, Vol. 28, p. 551. Since then, the book has become so well known to chemists concerned with the preparation of cellulose and its derivatives, and with analytical control methods in the various cellulose industries, that they refer to it familiarly by the author's name. We therefore welcome a new and enlarged "Dorée."

The enlargement is really greater than the increase in the number of pages (499 to 543) indicates, for the printer has conformed to the "authorised economy standards." The expansion is achieved partly by adding new

matter to the original chapters and partly by writing entirely new chapters. Thus, Part I, on Normal Cellulose, contains a new chapter entitled "The Structure and Apparent Molecular Weight of Cellulose and its Modifications." Part II, on Synthetic Derivatives of Cellulose, is introduced by a new chapter entitled "Cellulose Esters and Ethers—Some General Methods and Considerations," and the old Chapter XIV, on esters and ethers, is now divided into two.

The additional matter is a reasonably complete epitome of the results of private and co-operative research since 1930, but the author went to press before he could deal adequately with the use of periodic acid and nitrogen tetroxide as oxidising agents for cellulose. It is surprising, however, that the paragraphs on the dry weight of cotton do not include a reference to an important paper by Shorter and Davidson published in 1930. The note on p. 7 that "the commercial sheet known as Cellophane" presents cellulose in a useful form for physical and colloid investigations should be accompanied by the warning that the material may contain glycerol (for softening) or be fire- or water-proofed. The main criticism that may be made of the subject matter, however, is that the new information has not been thoroughly digested and is sometimes at variance with previous matter which is still allowed to stand. This is particularly so in the theoretical portions, where the author seeks to explain the underlying chemistry of the experimental methods. He writes as a spectator rather than as an active worker in the field of cellulose chemistry.

The book is written in a style that one has come to associate with much modern technical writing; that is to say, it is full of offences against literary form and grammar. Far too frequently one form of a verb has to serve for both a singular and a plural noun; thus: "The filtrates are concentrated and the copper estimated" and "solutions A and B are mixed and the stopper tied down." Another common fault is that of the unrelated subject and verb: thus, "the nitrate is pressed, then boiled with alcohol, squeezing after each boil." Does the nitrate squeeze itself? Again, the tense switches capriciously from the present to the past with remarkable frequency. There are dozens of examples of these faults in the book. Are they the penalty we pay for adhering to the convention that scientific work must be written in the passive voice? The reviewer would have rejoiced to see the "methods," so ably collected together by the author, set out in the terse imperative of a cookery book.

J. C. WITHERS.

Adventures in Man's First Plastic: The Romance of Natural Waxes. By NELSON S. KNAGGS. [Pp. xiv + 329, with 92 plates.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1947. 40s. 6d. net.)

THIS book is an account of the sources and uses of waxes, and the story of the author's experiences in the Amazon basin and the wilder parts of Texas and Mexico in search of new sources of vegetable waxes. As will be inferred from the title, it is not a textbook, but a popular non-technical account of the subject.

About a third of the book is devoted to the author's travels, which include an expedition in the jungles of the Amazon in search of the *Cauassu* plant, the leaves of which yield a wax not unlike carnauba; a visit to Brazil to

study carnauba wax production ; and journeys in Texas and Mexico in search of candelilla wax.

One of the many interesting chapters is that on the uses of waxes throughout the ages, which takes the reader far in time and space, and deals, *inter alia*, with such diverse subjects as the "lost wax" process of metal casting in art, used essentially today as thousands of years ago, mummies, oil and wax painting, Madame Tussaud, Javanese batik making, lithography, and carbon paper.

Other chapters deal with spermaceti, shellac (hardly a wax), Chinese insect wax, ouricuri wax, esparto grass wax, beeswax, ozokerite, montan, paraffin, and numerous lesser-known waxes. Much of this, particularly the chapters on shellac and Chinese insect wax, will be new ground to most readers, although some may detect shades of Melville and Maeterlinck in the chapters on spermaceti and beeswax respectively.

The book is well-written and illustrated, and holds the attention of the reader by the enthusiasm of the author. It will have an appeal to the curious reader, and, whilst not claimed to be of great scientific value, it should form an interesting background to the serious student.

Towards the end of the book, pp. 318-19, a few unorthodox chemical terms and a questionable definition of a fat might well have been omitted.

T. MALKIN.

The Chemistry of Commercial Plastics. By REGINALD L. WAKEMAN. [Pp. xii + 836, with 324 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1947. 60s. net.)

With the present multiplicity of books of varying quality on plastics, it is pleasurable to be able to report the production of a book of real value. This particular volume has the excellent format and general appearance associated with so many American technical publications. In content it is comprehensive, covering all important, and some less-important, high polymers, including synthetic rubbers. The style of writing makes the text easy to read, and the illustrations, both photographs and diagrams, are first-class.

The title is perhaps the only misleading feature. Although the chemistry of plastics is dealt with reasonably fully, it is by no means comprehensive: the section on phenolic resins, as an example, is quite inadequate in this respect. Furthermore, the book deals with far more than commercial plastics, since it describes many materials, *e.g.* polymethyl isopropenyl ketone, not yet on the market.

The author provides a most generous collection of tables and graphs of properties, set out in an attractive manner. Technological aspects of fabrication, techniques, etc., are only dealt with incidentally, the principles being stressed at the expense of unnecessary detail.

No polymers of any significance are omitted, and the various historical notes on materials are well done. Bibliographies, after each chapter, are stated by the author to be based on personal selection from a large number of references; readers will necessarily have different preferences in this connection. The indexes are comprehensive and useful. Very few errors have been noted and the number of misprints is remarkably small for such a long book. One case occurs in l. 3, p. 4, where "1861" should surely be "1681."

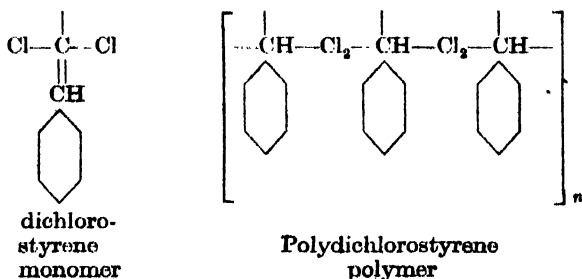
This volume should be attractive to both the expert and the general reader, and is likely to prove one of the most popular works yet written on plastics. It strikes a happy mean between the encyclopædic Carleton Ellis, and the numerous semi-popular attempts to describe plastics. It should be on the shelves of all interested in its subject matter.

N. J. L. MEGSON.

The Chemistry and Technology of Plastics. By RAYMOND NAUTH. [Pp. viii + 522, with frontispiece and 278 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1947. 57s. net.)

THIS book attempts to cover, chemically and technologically, the whole field of plastics and synthetic rubbers. The format is of the high standard associated with American publications, while the illustrations, although sometimes not easy to relate with the text, are excellent. The general standard and style are, however, best indicated by the following random quotations:

p. 231: "The alleged formula is presumed to be as follows:



p. 291, l. 11: "The chlorine atoms attach to crosslink some of the isoprene monomers [of natural rubber]."

p. 318: "If we mix isobutylene with butadiene under heat and pressure, there is a union of the two gases into a syrupy substance, which is later purified to form the isobutylene polymer." [This reaction is in fact carried out at -70°C . in a solvent.]

p. 490, Glossary: "*Transfer moulding*—a type of mold so designed to permit semi-injection moulding of thermosetting materials; not to be compared with the

"*Treacle Stage*—which indicates a thermosetting resin in liquid form applied to cast resinoids."

p. 491, Glossary: "*Linear Molecule*—a molecule of a large number of straight-chain polymers."

"*Loss Factor*—the sum of the power factor plus the dielectric constant."

The book contains innumerable misprints and inaccuracies of this type, the chemistry is generally out-of-date when it is not incorrect, and the arrangement is slipshod and difficult to follow. For example, Chapter 5, "Amine-Aldehyde Resins," includes sections on aniline-formaldehyde condensations, new resin cements ("cycleweld" and polyphenol resins) and cold-molded plastics. The longest section, namely Chapter 14, "Resin-Bonded Plywood and Veneer," which is only indirectly connected with plastics, appears to be the freest from inaccuracies.

A final quotation from p. 405 perhaps best illustrates the tone of the book :

"Hence we can set an equation for 'Know-how' or engineering skill, thus :

$$\frac{[AKM]_n + [WNTD]_n}{EX^1 + EX^2} = [KH]_n$$

in which

AKM	= All known methods
EX ¹	= Experiences which were successful
WNTD	= What Not To Do
EX ²	= Experiences, costly and bitter
KH	= Know-how

Which in summation means : 'good engineering is the end product of a forward, open mind plus experience squared.' "

Those already engaged in the high polymer industry might possibly find certain useful pointers in this expensive book, but others are more likely to be misled.

N. J. L. MEGSON.

GEOLOGY

Igneous Minerals and Rocks. By E. E. WAHLSTROM. [Pp. ix + 367, with 135 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1947. 33s. net.)

THE author's aim has been to provide a text for the "beginning student in microscopic igneous petrography," and at the same time to make it of use to more advanced students. It is not to be wondered at, therefore, that he found the task a difficult one. The reviewer thinks that he has been much more successful in his second aim than in the first, for the book is, in his opinion, far too advanced for elementary teaching.

The book is divided into two parts : I. Igneous Minerals ; II. Igneous Rocks. An introductory chapter heads the first part, and gives definitions, a statement of the composition of the earth's crust, and the geological occurrence and origin of igneous rocks. In this the author shows that he is well aware of recent developments of theory in regard to the origin of granite.

Chapter II deals with "Optical Examination of Rock-forming Minerals." This is condensed, as the subject is treated in much more detail in an earlier book by the author, but quite sufficient is given to serve the main purpose of the book. It is pointed out that, for mineral identification, the immersion technique has many advantages over the thin-section technique, although the latter is the better for the study of texture and the order of crystallisation.

Chapter III deals with "Supplementary Methods for examining Minerals in Igneous Rocks," and Chapter IV with "Minerals in Igneous Rocks," in which the principal types are described according to a definite scheme. This part is very well done and is illustrated by excellent photomicrographs and optical diagrams. It is followed by a long series of tables for Mineral Identification (Chapter V).

Part II—Igneous Rocks—begins with Chapter VI on the "Geological Occurrence of Igneous Rocks." Chapter VII deals with "Structure, Texture, Colour, Alterations." The author uses the terms structure and texture with much the same connotations as are current on this side of the Atlantic, but he

analyses the differences much more acutely than is usually done. "Chemical Composition and Classification" are the subjects of Chapter VIII. The evaluation of chemical analyses, methods of calculating norms and modes, and variation diagrams, are the subjects that appear under the heading of chemical composition. A useful method is given for the *direct* calculation of norms without the use of molecular numbers. Classifications based on chemical composition are briefly discussed.

Chapter IX deals with "Mineral Composition and Classification." It treats of such subjects as the composition of natural glasses, essential and accessory minerals, mineral characteristics, sequence of crystallisation, quantitative estimations, megascopic classification, qualitative and quantitative mineral classifications. The classification used in this book differs little from those previously proposed by several authors. It is eminently sane and simple, following Rosenbusch qualitatively, Iddings and Niggli in a quantitative sense. Albite is rightly (in the reviewer's opinion) regarded as an alkali feldspar in classification, going with orthoclase and microcline rather than with the more calcic plagioclase feldspars.

Chapter X is occupied by brief descriptions of igneous rocks, accompanied by excellent figures of rocks and thin sections. The value of descriptions of such exotic rocks as "masonite," "bigwoodite," "kaiwekite," "penik-kavaarite," "miagite," etc., for elementary, or even for advanced, students, may be questioned. The final chapter (XI) contains an outline scheme for the identification of igneous rocks in thin sections.

The book is well printed on good paper and has a number of excellent illustrations which are also illuminating—not always the case even with well-produced illustrations. The style is rather bald and staccato, but this probably results from the effort to cram in as many facts as possible. There are few mistakes or misprints; "Elaeolite" is misspelt (p. 80), and trachyte is spelt "trachite" throughout the book, no doubt in mistaken deference to nomenclatorial uniformity.

G. W. T.

The Permo-Triassic Formations : A World Review. By R. L. SHERLOCK, D.Sc., A.R.C.S., F.G.S. [Pp. 367, with 16 figures.] (London : Hutchinson's Scientific and Technical Publications, 1948. 3ls. 6d. net.)

WHEN Murchison instituted the Permian System on the basis of a sequence of continental sediments he provided the materials for an enduring controversy, for some have thought that he committed a stratigraphical blunder. Dr. Sherlock was one of these. Coming up against the Permian problem early in his geological career, he made a study of it for forty years, at home and abroad, in the field and in the literature, and he lived just long enough to see the results of his labours in print.

After an introductory section the book proceeds to a comprehensive stratigraphical survey. No corner of the earth—except Antarctica—where Permian and/or Triassic rocks are known is left unnoticed, and a tremendous amount of information is summarised. Certain of the regional accounts are far from modern, but altogether a vivid, interesting and useful world-picture is presented.

In the third section the thesis is unfolded that the Permian is not a valid system and that it is not clearly separable from the Carboniferous below or

the Triassic above. Various workers have advocated its assignment to one or the other, but Dr. Sherlock, emulating the judgment of Solomon, would divide it between them. From a consideration of the marine fossils, tetrapods, plants and tectonics, he argues for a break between Lower and Upper Permian, the former being essentially Carboniferous, the latter merging into the Trias. He makes many excellent points in support of his contention, but labours under the inevitable disadvantages that he who has to rely on the printed word is apt at times to misconstrue it, and that knowledge of local geology is always well ahead of publication.

Clearly the last word has not been said on this important topic. In parts, at least, of Gondwanaland there is no evidence of an important intra-Permian biological break, and American workers on the Texan and other exposures will still be convinced that there is a Permian System, even if they are not sure where its top and bottom are! But the considered opinions of a man of Dr. Sherlock's standing are not to be lightly brushed aside; the book presents a case to answer, and as a survey of the Permian problem it is invaluable.

W. R. B.

Glacial Geology and the Pleistocene Epoch. By R. F. FLINT.
[Pp. xviii + 589, with 6 plates and 88 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 36s. net.)

PROFESSOR FLINT of Yale University brings to his task of summarising the Quaternary Era (the author prefers the term Pleistocene) a wide knowledge of the subject and a mature judgment. The result is an excellent book. A short analysis of ice-flow and its causes is followed by a stimulating treatment of existing glaciers and of the main features of glacial erosion and deposition. The bulk of the book deals with the Glacial Period; the expansion and shrinkage of the ice; its drainage and periglacial features; the succession of the drifts; pluvial periods; chronology; the causes of the Ice Age; changes in sea-level; and post-Glacial fluctuations. The authoritative discussion of the American evidence (which receives equal treatment to that of the rest of the world) is particularly welcome to non-American readers who can supplement the somewhat meagre space devoted to their own continent from other standard works.

The author is cautious and critical. He disapproves of the fixed chronology based on Milankowitsch's theory, which he rejects partly on the grounds of its inadequacy; he doubts the objective reality of much of the varve correlations; he discounts the correlative value of fluctuations of sea-level; and except for the Wisconsin (with its opening Iowan phase) lends no support to the view that the glacial epochs should be further subdivided as Soergel, Milankowitsch and others demand. Essentially, he seeks the cause of glaciation in a world-wide cooling associated with solar radiation when the earth itself, because of Tertiary uplift, was especially responsive to such change.

The emphasis throughout is morphological and genetic. Floral and faunal questions receive little attention and the question of Early Man, both skeletal and cultural, is avoided largely because of its complexity and difficulty.

The book is provided with an adequate number of good illustrations, a good index, and a useful bibliography (mostly American). Misprints are few; *western* on p. 318 should read *eastern* and two lines are transposed on

p. 163. Errors are few. Kirmington is not on the coast (p. 341); the map on p. 121 omits recent findings of erratics in the Lake District, Kintyre and Inishowen; the arrows in Pl. 6 indicating the ice-flow between the Wash and Firth of Forth should be reversed; and the "York Line" should be extended to include the Hunstanton Boulder-clay. These errors in no wise detract from an admirable presentation.

J. K. C.

Catalogue des Fossiles Tertiaires du Bassin de Paris. By RAYMOND FURON and ROBERT SOYER. [Pp. 240, with 32 plates.] (Paris: Paul Lechevalier, 1947. Frs. 380.-.)

THE Paris Basin has long been celebrated for the profusion and good preservation of the Lower Tertiary fossils which may be collected there. Monographs on various groups, particularly those of Deshayes and Cossmann on the mollusca, are well known to palaeontologists, but the present work is the first attempt to catalogue all species, animal and vegetable, known to have been found in the area. About 5000 species are listed, the geological range of each is briefly indicated, and a locality is mentioned where it may be collected. The collector wishing to identify his specimens will find 500 species, including those most characteristic of the various stages, illustrated in the 32 collotype plates, but the only indication of where descriptions and illustrations of the remainder are to be found is a brief bibliography. Detailed references would have enhanced the utility of the work, but no doubt considerations of space have necessitated their omission.

Both in France and in England the progress of geological knowledge has owed much in the past to the activities of the amateur collector, and unfortunately there are signs that in this country the popularity of "fossil-hunting" as a hobby has declined in recent years. It is, therefore, satisfactory to know that French collectors are still numerous enough to have called for the publication of the present work, which should prove useful as a check-list and as a guide to the latest nomenclature.

L. R. C.

BOTANY

Ancient Plants and the World they Lived in. By HENRY N. ANDREWS, Jr. [Pp. xii + 279, with 166 figures.] (New York: Comstock Publishing Co., Inc.; London: Constable & Co., Ltd., 1947. 25s. net.)

An Introduction to Paleobotany. By CHESTER A. ARNOLD. [Pp. xii + 433, with frontispiece and 187 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 27s. 6d. net.)

THE two books under review have been produced by university professors who have made important contributions to our knowledge of the plants of the past.

Ancient Plants and the World they Lived in, by Professor H. N. Andrews, may be described as a semi-popular work for readers without any special acquaintance with botany. Although the serious study of fossil plants began in the early part of last century, few attempts have been made to provide a book dealing with the past history of the earth's green carpet comparable with the many semi-popular works on extinct animals. The present work tells how the remains of ancient plants have been preserved

and how they are found in different parts of the world. An idea of the structure and appearance of many of them is conveyed by photographs and drawings of the fossil forms and of living forms which resemble the ancient types, also by drawings showing the probable appearance of the fossils when growing. Although many of the plants that are described lived and died two or three hundred million years ago, their tissues have been preserved in astonishing perfection; a number of photomicrographs of sections through their organs are reproduced. The material is considered under various headings, such as Ferns and their Ancestors; Lingering remnants of the Coal Age; Sequoia, a living link with the past. There is, of course, a chapter on coal, reminding the reader of the great economic importance of thick beds of fossil plants, illustrated with interesting photographs of opencast mining in the U.S.A., which show the gigantic mechanical excavators used. Two chapters deal with the evidence provided by fossil plants of considerable changes in the climate of the Arctic regions and of the pacific coast of North America during Tertiary times. The work of Professor Chaney and his collaborators is summarised, showing how the warm-loving vegetation gradually retreated southwards and was replaced by the more hardy plants from the north. Similar evidence had previously been obtained in Europe, but doubt had been felt as to its significance.

Some problems of plant evolution are considered by the author, but these do not form the core of the book. Professor Andrews shows that fossil plants possess much intrinsic interest and he has given us a very attractive volume.

In *An Introduction to Paleobotany*, Professor Chester Arnold has produced a comprehensive textbook for students of botany, containing a great quantity of information and many references to recent discoveries. The material is well selected from a very wide field and the description of structural details is reduced to a minimum. The illustrations are numerous and of excellent quality; many of them are original. A short list of selected references concludes each chapter; among these are mentioned papers which are probably little known to many European palaeobotanists. The information obtained from mummified and petrified specimens is properly integrated, and the survey is made more complete than that given in some of the older works by the inclusion of chapters on the algae and the angiosperms, as well as on the conifers and ginkgos. A new and useful feature is a chapter devoted to taxonomy and nomenclature, which explains the special difficulties and problems confronting the palaeobotanist, and the reasons for the multiplication of names which often seems so confusing to the student accustomed to the study of living plants and animals. It is to be hoped that the recognition of these problems will lead to the universal adoption of a system which an international committee is now trying to formulate, and so avoid the confusion and waste of effort which has arisen in palaeobotanical literature in the past.

Professor Arnold's survey of the plants of the past is arranged in sections corresponding to the main taxonomic groups of the plant kingdom. In assigning fossil types to these groups he adopts a conservative attitude, but, in view of the great increase in the number of forms now known from palaeozoic times, a more critical treatment would have been welcomed. Many of the characters formerly regarded as diagnostic for groups are now known to have occurred in different lines through parallel evolution and consequently a reconsideration of the position is needed. The author shows that he is aware of some of the problems arising from study of the older groups, such

as the possibility of an affinity between the Pityææ, which he has studied so admirably, and the Calamopityææ. His inclusion of a number of widely different plants under the heading of Ancient Ferns seems, however, to be too comprehensive a grouping. The known characters of the structures included in the Cladoxylææ differ widely from those of any plant known to be a fern, and the similarities are few. One may even doubt if the important discoveries made by Professor Arnold of heterosporous sporangia attached to the fronds of *Archæopteris* justify its close association with the ferns. There may have been several groups of plants in Palæozoic times that had large fronds and sporangia, whose relationships to the ancestors of the true ferns were as remote as those between the Equisetales and Sphenophyllales. But once a student has been taught to associate plants of different types he tends to exaggerate their similarities and to become blind to their differences. Another source of regret is that in dealing with the carboniferous pteridosperms the author describes the younger types before the older forms. The plants of the Lower Carboniferous rocks are much older than those of the Coal Measures, and several of the structures found in the later beds take on a new significance if compared with the earlier forms. But despite these criticisms Professor Chester Arnold has given us a valuable book for the university student in the later stages of his training, and one that is worthy of a place in all botanical libraries.

H. H. T.

The Evolution of "Gossypium," and the Differentiation of the Cultivated Cottons. (The final report of the Genetics Department, Cotton Research Station, Trinidad, British West Indies.) By J. B. HUTCHINSON, R. A. SLOW and S. G. STEPHENS. [Pp. xii + 160, with 10 plates and 10 figures.] (London: Oxford University Press, 1947. 15s. net.)

To monograph a genus which includes cultivated species is a notoriously difficult task: to do it successfully, as in the present instance, is something of an achievement. The information contained in this book is of interest, not only because of its bearing on the development of an economically important crop, but because in tracing this development light is thrown on such evolutionary problems as the origin of characters, the significance of polyploidy, and the inter-relationships of variability and dispersion.

The text is divided into four parts: (1) the classification of the genus; (2) the evolution of species; (3) the differentiation of the true cottons; and (4) the significance of the genus in evolutionary studies. These four parts are nicely co-ordinated, and together provide the reader with a well-balanced picture of the evolution and present state of the genus.

The taxonomic treatment is of particular interest to plant breeders, for it is a treatment based on experimental evidence and illustrates the application of genecological principles. The cultivated cottons appear in the taxonomic Sections *Herbacea* (Old World, $n = 13$), and *Hirsuta* (New World, $n = 26$), two of the six cenospecific groupings which represent major lines of evolutionary differentiation within the genus. Actually eight sections are recognised, as the wild American group is subdivided into three. Within these sections specific rank has been accorded only to those populations whose integrity is maintained by a genetic barrier to free gene exchange, with the result that the number of listed species has been reduced from a bewildering array to

five. These five taxonomic species represent five geneecological ecospecies. At the infra-specific level, that is within the field of genetic harmony between populations, taxonomic treatment has very wisely not been carried beyond the more clearly differentiated geographical assemblages—the *varieties* (*sub-species* would perhaps have been more appropriate since the ultimate crop units are also known as varieties). The *varieties* are referred to as ecotypes, but used thus the term ecotype seems rather redundant, for it is difficult to imagine any regional population that is entirely devoid of ecotypic significance.

The wild lintless species, though geographically much scattered, are individually low in variability, whereas four of the five linted species are remarkable for their great store of intra-specific variation. In fact, the outstanding feature of the genus is the relatively rapid accumulation of genetic variation within the cultivated species, and the evolutionary success of polyploids in cultivation. The dynamic nature of the cultivated races is also striking, populations which were at one time differentiating under the comparative isolation provided by distance are now interbreeding as a result of the transfer of commercially desirable types across old racial boundaries.

The book is a notable addition to crop-plant as well as to evolutionary literature.

J. W. GREGOR.

Leguminous Forage Plants. By D. H. ROBINSON, Ph.D., B.Sc., N.D.A. Second edition. [Pp. viii + 119, with 35 figures.] (London: Edward Arnold & Co., 1947. 7s. 6d. net.)

A SECOND edition of this very serviceable little book has now appeared. It is designed to enable interested persons to recognise the more important leguminous crop-plants grown on farms, and it describes them with sufficient clarity to enable inexperienced observers to recognise them. Much of its virtue lies in the carefully chosen illustrations in the form of text-figures. Technical terms are generally avoided, but a glossary is included for those which are necessary. Data for chemical composition to indicate the relative nutritive values of the different species are usually given, as well as details of some of the less-familiar modern strains of well-known species. A table of seed statistics represents another valuable feature, and equally useful are the drawings of the principal types of seed. This is, therefore, a useful and timely book.

W. H. P.

BIOLOGY

General Biology. By P. D. STRAUSBAUGH, Ph.D., and B. R. WEIMER, Ph.D. Second edition. [Pp. viii + 718, with 28 plates and 350 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 28s. 6d. net.)

STRAUSBAUGH and Weimer have extensively rewritten their nine-year-old textbook for this new edition, and it is now perhaps the best of its kind that America has produced in recent times. Certainly it stands far ahead of most of its numerous rivals, and those teachers who do not feel that their wants are wholly satisfied by British biology books would do well to look at it.

It is written for "college students": in this country it will be rated as at approximately Higher Certificate level, well-written and easily read, and

though not necessarily covering the whole of any particular syllabus, it would be valuable as a fresh approach for the pupil. Perhaps it would be even more useful for older students, with no grounding in biology, and with no examination to pass, who want to have a general picture of the subject.

Botany and Zoology are skilfully interwoven, and one is constantly passing from the one subject to the other without any sense of discontinuity. There is a strong emphasis on those biological facts that are relevant to human welfare, and full use is made of the topical interest of recent biological advances. The abundant illustrations, diagrams and photographs are well done and novel: even Darwin has been redrawn.

W. H.

Conseil Permanent International pour l'Exploration de la Mer. Annales Biologiques, Vol. No. II, 1942-1945. [Pp. 174, with numerous figures.] (Copenhagen: Andr. Fred. Høst & Sons, 1947. Kr. 12.00.)

THIS volume is of special interest in that it includes reports from a number of different nations concerning the effects of the second world war upon the yield of the sea fisheries. The recovery of the steam trawling fishery for hake by the Milford Haven fleet provides a striking example. Mr. Hickling tells us that, in most of the years between the two wars, too intensive fishing was leading to the fishing-out of the larger hake at a rate of decrease of about 15 per cent., the mortality rate being estimated at about 46 per cent. Following the war-time requisitioning of fishing vessels in 1940, the mortality rate for the large hake fell to 21 per cent. Consequently, in the years 1942-44, a fishing fleet reduced to an aggregate gross tonnage of between 8000 and 9000 tons annually landed a greater weight of hake than was landed, between the wars, by a fleet aggregating from 20,000 to 30,000 tons. The recovery of the fishable stocks was thus complete. Or again, the Porcupine Bank had a complete rest from fishing from December 1940 to August 1944. When fishing was resumed, the Bank was found to be carrying a population of haddock five times as dense as in the best pre-war year. Elsewhere in the volume, we read of greatly improved stocks of haddock in Swedish waters; of the greater quantity of plaice per day's fishing, or per unit of fishing gear, by Dutch and Danish fishermen; of an enormous increase in the stock of herrings in the southern North Sea. It is very important to observe, however, that in all these instances, as, indeed, in general, the utmost care and caution must be exercised before these manifest anomalies in fishing yield can be satisfactorily accounted for. Each fishery requires to be treated separately and fully, and in the light of many contributory factors.

E. F.

Genetics. By EDGAR ALTENBURG. [Pp. xii + 452, with 148 figures.] (New York: Henry Holt & Co., Inc.; London: Constable & Co., Ltd., 1946. 16s. net.)

In this book the author states that he emphasises "modern genetics," which apparently is defined as "the science which concerns itself with the study of the germ plasma." It is, however, admitted that a clear understanding of "classical genetics" is necessary in order to appreciate modern developments, although Mr. Altenburg says he has planned his book so as to emphasise principles rather than highly technical details.

In spite of these avowed intentions, the book does not differ materially from the generally accepted ingredients of a genetical textbook, although it does include chapters on certain practical applications of genetic principles, such as inbreeding and outbreeding, methods of selection, and the artificial production of mutations. There are also constant allusions to human and sociological problems, which certainly add to the general interest of the subject matter. Further attempts to achieve the emphasis on general principles are made by considering heredity and development, and the genetic basis of evolution.

It has now become a common feature of genetic textbooks to include "problems" at the end of each chapter, and the book under review is no exception. To be of much value, this feature has to be planned carefully and phrased meticulously. In this book, the author has allowed himself to be carried away by questions for the sake of questioning, and some of the "problems" can hardly be said to exist, as when the student is asked—"How can we most readily account for the fact that a child resembles both parents equally?" (p. 20).

The author has striven to use a simple and clear style, but constantly mistakes colloquialism for clarity, and sacrifices accuracy. It does not help the beginner to be told that "at the metaphase stage the chromosomes dangle from the middle of the cell" (p. 6), or that "in reduction division the chromosomes do a very unusual thing" (p. 6). Sentences such as these may become positively misleading, and are not helped by homely analogies such as the likening of the action of a gene to the cogwheel in a watch (p. 16).

The book has, however, many valuable aspects from the point of view of the elementary and general reader. The diagrams are for the most part very clear, though the photographs are not good, and one might have expected better pictures of a dividing nucleus than those on p. 5. But the choice of material is good, and there are many interesting aspects of genetics included which are not commonly dealt with in genetic textbooks.

G. D. H. B.

Heredity and Variation in Microorganisms. Cold Spring Harbor Symposia on Quantitative Biology, Vol. XI. [Pp. xii + 314, with 183 figures, including 14 plates.] (Cold Spring Harbor: The Biological Laboratory, 1946. \$6.00.)

THE resumption of the Cold Spring Symposia, discontinued since 1942, is welcomed by all biologists and workers in neighbouring fields. The present report, consisting of 27 communications, is probably the most important of the eleven so far published, because it marks the emergence of a new development in biology, begun with the work of Winge, Tatum and Beadle and C. & G. Lindegren in the years 1937-40.

The movement started with the study of the inheritance of apparently simple characters—colony form, ability to ferment simple sugars, etc.—in spore-forming yeasts possessing a well-marked sexual cycle permitting of study along the lines of classical genetics. This development was joined to studies by Tatum and Bonner on the artificial production of mutants in the now-famous mould *Neurospora crassa*; this was first achieved by the use of U.V. and X-rays, and later by chemical agents such as the nitrogen and sulphur mustards. The papers in this volume mark the extension of these studies first to bacteria and then to viruses (bacteriophages). These

advances have greatly influenced the course of the neighbouring subject of biochemistry by facilitating the study of biosynthesis and providing a tool whereby information may be gained concerning the steps by which the cell builds up the chemical units of its own material from the constituents of its medium.

D. Bonner, in a paper entitled "Biochemical Mutations of *Neurospora*," deals both with the fundamental aspects and later developments of this subject. He describes the methods by which the mutants are obtained and the nature of the resulting mutations. Each mutation consists in loss of ability to synthesise some essential constituent of the cell, such as an amino-acid, a growth factor or a purine. Bonner advances the theory, now widely if not universally accepted, that each mutation involves the loss of one gene controlling the production of one enzyme necessary for the catalysis of one step in the biosynthesis of a compound essential to the make-up of the cell; this inserts a block—or removes a link—in the chain of reactions by which the compound is built up. Mutants thus obtained can grow only if supplied with the compound itself or with an intermediate in the chain lying between the block and the end product. This article gives many examples of the manner in which such mutants can be used to determine the course of biosynthesis and of the important and interesting results so far obtained. The intermediates thus unmasked may be known cell metabolites, but occasionally, as in the biosynthesis of nicotinic acid, unknown compounds in the chain may be isolated.

To carry out such studies most effectively involves the use of genetical material in which apparently similar mutants can be distinguished from one another by heterocaryon formation and outcrossing to determine the loci of the genes concerned. Much information on biosynthesis can, however, be obtained from cells such as bacteria not susceptible of artificial crossing; this is set out in an article by E. L. Tatum entitled "Induced Biochemical Mutations in Bacteria," and it is encouraging that in all instances studied the steps in biosynthesis of the same compound in widely differing cells are the same and conform also to what is known of biosynthesis in the animal.

Genetical studies on bacteria lag behind those on cells with a well-marked sexual cycle permitting artificial crossing. Recent cytological work has, however, established the existence in bacteria of well-defined nuclei. An article by J. Lederberg and E. L. Tatum entitled "Novel Genotypes in Mixed Cultures of Biochemical Mutants of Bacteria" produces good evidence that during the growth of mixed cultures of multiple mutants of a strain of *E. coli* some form of conjugation occurs involving the passage of genes from one cell to another and the production of new stable races having all the synthetic abilities of the prototrophic parent from which the mutants were produced.

These findings will recall the famous work of Avery, MacLeod and McCarty on the transformation of pneumococcal types through the agency of pure desoxyribonucleic acid obtained from the transforming cell. Developments of this work are reported in the present volume in a paper by M. McCarty, H. F. Taylor and O. T. Avery, in which the rôle played by serum in this transformation is subjected to a searching analysis.

It is inevitable that, in an effort to apply genetical studies to forms of life of ever-decreasing size, attempts should be made on the genetics of viruses. A. D. Hershey begins his highly technical article on "Spontaneous Mutations in Bacterial Viruses" thus: "It is reasonable to suppose that

among the viruses we observe the operation of the principles of heredity at an elementary level of organisation not far removed from the gene itself."

In the paper which follows a study is made of the mutation of certain bacteriophages of *E. coli* in respect of such properties as host range and ability to lyse. Mutation in animal and plant viruses is also discussed.

In an article entitled "Induced Mutations in Bacterial Viruses," M. Delbruck and W. T. Bailey study the results of infecting one strain of bacteria simultaneously with pairs of unrelated viruses, showing that only one of the pair, the "successful" virus, multiplies in the host cell, though the "excluded" type exerts an influence on the multiplication of the "successful" one.

There appears at the moment to be a spate of work on the mutations of bacteriophage, probably because this is the only type of virus which easily lends itself to such studies. How far facts thus gleaned will turn out to aid the study of plant and animal viruses remains to be seen.

Amongst other papers, N. W. Pirie treats of animal and plant viruses in a critical article entitled "The State of Viruses in the Infected Cell."

Of special interest to bacterial chemists is an article by S. Spiegelman on the mechanism of adaptive enzyme formation entitled "Nuclear and Cytoplasmic Factors controlling Enzymatic Constitution." The cancer problem is represented by an article by J. G. Kidd on the "Distinctive Constituents of Tumor Cells" and by one on "Variation in the Leukemic Cells of Mice" by E. C. MacDowell.

It is impossible in this review to pick out all the plums in this rich pudding and the selection of subjects perhaps reflects the interests of the reviewer rather than the relative importance of the material selected. All biologists are recommended to study this report for themselves and to pick out their own plums.

M. S.

PHYSIOLOGY AND BIOCHEMISTRY

The Physiology of Man in the Desert. By E. F. ADOLPH and Associates. [Pp. xiv + 357, with 184 figures.] (New York and London: Interscience Publishers, Inc., 1947. 39s. net.)

THIS book is a record of wartime research done for the U.S. Office of Scientific Research and Development, by a team of eleven physiologists led by E. F. Adolph. It is not, and, as the authors make clear, it is not intended to be, a full review of the subject.

The team worked in the Colorado Desert of California in association with military training units, who provided many of their subjects. Their problems were practical ones connected with desert warfare and centred round the paramount need for water for sweat formation in a desert environment. How much water do men need, can they be trained to do with less, how much work can they be expected to do on a limited supply? The answers to these and many other problems were worked out in an admirably thorough and systematic manner, and are reported in this book. The whole work forms a model study in applied physiology, linked at some points to questions of more general biological interest; for example, in a quantitative comparison of the way in which men and other animals replace their water deficits.

E. F. Adolph has himself stressed the fact that different people find

different points of interest in the same data. (Introduction to *Physiological Regulations*, Jaques Cattell Press, 1943.) He and his team had opportunities for collecting data which are unlikely to recur in peace time. The facts they collected are themselves likely to prove of interest to other workers, if reported fully enough. The reviewer feels that the present report might have proved more useful in the long run if more of the crude data had been given. This could have been done in some parts without increasing the length of the book, e.g. in scatter diagrams in which data on different subjects are not separated. There is also scope for shortening the text by cutting out some tedious repetition of elementary physiological facts, associated with separate authorship of different chapters. However, this is the only criticism the reviewer can make of a book which makes a very definite contribution to the literature of the subject.

R. D. HARKNESS.

The Biological Standardisation of the Vitamins. By KATHERINE H. COWARD, D.Sc. Second edition. [Pp. viii + 224, with 38 figures, including 7 plates.] (London: Baillière, Tindall & Cox, 1947. 16s. net.)

WRITTEN from the laboratory as well as from the study, the second edition of this popular and useful book will be welcomed, partly because all that made the first edition unique and valuable has been retained and partly because, in its new features, the book continues to help the laboratory worker in the same way as before, and especially to adapt himself to the changes in the statistical approach to his problems which have developed in recent years. The author's wide practical experience of the care of animals, their preparation for assays, the design of assays and the calculation of results is again placed at the disposal of the laboratory worker; and the excellent illustrations, whether charts, line drawings or reproduction of photographs, are retained for the instruction of the beginner and the convenience of the more experienced worker. The developments of the last few years, whether of technique, information, or assay data have been incorporated, the advances made possible by the work of the Permanent Commission on Biological Standardisation of the Health Organisation of the League of Nations have been included, and a new chapter on Vitamin E has been added. The author has always recognised the limitations and imperfections of the methods of biological assay of the vitamins, the crudeness of the results they yield, and the need for recognising and estimating the magnitude of their inevitably large errors. In recent years important changes have been introduced into the measurement of these errors, the modern practice being to calculate them from the data yielded by the assay itself rather than to estimate them from the results of a number of assays, and the estimation of the fiducial limits of error is now preferred to the older methods which yielded only approximate values. Few workers have contributed so much valuable assay data as the author of this book and her colleagues; and to those familiar with the first edition it is only necessary to state that workers in this field, who have been helped in their difficulties over the statistical approach to their problems and analysis of their data in the past, will find the same kind of help in these new developments where, as before, the data yielded by actual assays are used for illustration and calculation by these new methods.

PERCIVAL HARTLEY.

Detoxication Mechanisms: The Metabolism of Drugs and Allied Organic Compounds. By R. TROWYN WILLIAMS, Ph.D., D.Sc. [Pp. viii + 288, with 1 figure.] (London: Chapman & Hall, Ltd., 1947. 25s. net.)

THIS is an admirable book, which will be useful to workers in many fields of biochemistry, pharmacology and chemotherapy. The author has collected and arranged in orderly fashion a mass of detailed information on the metabolic fate of organic compounds foreign to the body. The compounds are dealt with in their ordinary chemical classifications, there is an excellent index and the name of each individual substance as it comes up for discussion is printed in bold type; it is, consequently, easy to discover either what is known about the metabolic fate of a given substance or what substances of a given type have been investigated. The bibliography occupies 23 pages and appears to be substantially complete up to 1945. Publishing delays have prevented the author from covering the literature of 1945 and 1946 completely.

The question-begging title "Detoxication Mechanisms" is a misnomer; the author is well aware of this, but has preferred not to discard a phrase which is still current in textbooks of biochemistry. His sub-title is more accurately descriptive, but is not entirely satisfactory, because he is mainly, if not entirely, concerned with metabolic processes which lead to recognisable derivatives of the original substance in the urine; he would be the first to admit that other metabolic pathways are frequently involved, but the fragments of the original molecule are, without the help of labelled elements, unrecognisable as such; indeed, he rightly stresses the need for quantitative studies and the construction of "balance-sheets." The author's own views on the vexed question of "detoxication" are modestly reserved to the last few pages of his book: a foreign organic compound, introduced into the body, must run the gauntlet of a very large number of biochemical mechanisms; what happens to it will depend upon its own susceptibility to particular chemical reactions and upon its ability to act as substrate to particular enzyme systems. The chemical changes which it does undergo may lead to more or to less toxic substances; this is a matter of chance, but if any of its metabolic products are easily excreted by the kidney, these products will appear in the urine. In the reviewer's opinion readers might well begin by reading the first and last chapters before plunging into the main body of the work; the first chapter has an excellent historical introduction to the subject and the last a balanced discussion of theories of "detoxication."

H. R. I.

MEDICINE

Fundamentals of Immunology. By Wm. C. BOYD, Ph.D. Second edition. [Pp. xviii + 503, with 50 figures.] (New York and London: Interscience Publishers, Inc., 1947. 36s. net.)

WRITTEN by a chemist who specialises in the field of immunochemistry, *Fundamentals of Immunology* belongs to that extremely valuable group of textbooks which aims at introducing both student and research worker to the more specialised fields in biological and medical science. At the same time it forms a useful survey for immunologists, who must find it increasingly difficult to cover the various aspects of a very complex subject. Experimental

findings and theories are presented and discussed, not because of their historical importance, but because of their contribution to present-day immunological thought, and, by suitably chosen illustrations and reference to the more important papers in each section, it has been possible to cover a very wide field, without sacrificing clarity of outline.

The author's special interest in the chemical structure of antigens and the nature of the antigen-antibody reaction *in vitro* has ensured an adequate survey of these points, occupying nearly half the book, which is fully justified by its importance for an understanding of the immune reaction. Considerably less attention is devoted to practical problems and especially those of interest to the clinical pathologist, but these are adequately dealt with in other works. Nevertheless, he will find much of interest; technical details are given of laboratory tests and frequent references are made to their application. Blood grouping, which is advancing rapidly both in knowledge and complexity, requires more adequate treatment if the clinical worker is to follow its more recent developments.

As a second edition, Professor Boyd's book does not differ radically from the first edition of 1942, nor does it include, except by reference, much recent work which was often the outcome of war-time research. Nevertheless, it does succeed in imparting a good general background to immunological studies, which is its primary aim, and it will be admirable if in subsequent editions this can be achieved while at the same time serving the needs of so many types of reader.

E. S. D.

Dynamische Reaktionspathologie. Die Entstehung einer autonomen Heilkunde als Ueberwindung der Grundlagenkrise der heutigen Medizin. By KURT VON NEERGAARD. [Pp. 317, with 26 figures.] (Basel: Benno Schwabe & Co., 1946. Swiss Frs. 24.-.)

PROFESSOR von Neergaard, in this epistemological monograph, has tried to show that medicine has reached a turning-point in its history and is faced with a new orientation. The advent of modern quantum physics has, in his opinion, made obsolete the physico-chemical approach of the past, and the mechanistic outlook must give place to a new one, in line with quantum physics and quantum biology. The author builds up a cosmology in which medicine, and especially pathology, is placed on a special, separate plane above biology, and it will be the task of the future to find and define the laws which govern this domain. In spite of a very persuasive phraseology, the new conception remains vague and somewhat metaphysical, and endless repetitions, particularly of attacks on the quantitative physico-chemical methods of present-day use, do not make the argument more convincing.

Almost every conceivable aspect of medicine is touched upon, and a specially long discussion is devoted to the term "disease." This chapter is very interestingly written, and it is here that the concept of "Dynamische Reaktionspathologie" is developed. This phrase has been chosen to emphasise two things, (a) the time factor, past and present, involved in any pathological process, and (b) the all-important state of reactivity of the affected individual; whilst humoral and organic changes are considered to be subordinate. The supreme task of the doctor is to supervise this state of reactivity. The individual patient, of whom rightly a chrono-holistic view

should be taken, is a singular entity, his deviation from "health" a singular problem to the doctor. True, it may fit into a general scheme of diseases more often than not, but the latter case introduces a principle of uncertainty. On this point the author has some valuable observations to make.

To increase the natural, active reaction of the organism is the aim of physiotherapy, of which Professor von Neergaard is an exponent. In his view this branch is called upon to become the mediator between scientific school-medicine and nature healing. The immense advances which, during the last decades, have been made in biochemistry in general, and in dietetics, hormone and vitamin research in particular, do not seem to be sufficiently appreciated by the author, though—on occasion—he acknowledges some of these achievements.

The book—so the author states—is the theoretical outcome of his experience in the field of the vast range of catarrhal and rheumatic diseases which, unfortunately, have so far shown themselves so very refractory towards the present-day laboratory methods.

In the third part of the book, the effects of the new theoretical outlook (or is it not the pressure of the political and economic changes which at present shatter the world?) on the organisation of the medical services are discussed. Here, incidentally, some praise is given to the practical teaching in the British Medical Schools.

Written in a typically Germanic philosophical style, the book makes no easy reading and contains many newly coined words and phrases. The literature cited is almost entirely in German. There is no index.

F. J.

HISTORY OF SCIENCE

On Understanding Science: An Historical Approach. By JAMES B. CONANT. [Pp. xvi + 145, with 10 figures.] (U.S.A.: Yale University Press; London: Oxford University Press, 1947. 10s. 6d. net.)

In this series of Terry Lectures the President of Harvard University discusses with becoming diffidence the problem of imparting a sound understanding of science to students who are to become lawyers, writers, teachers, politicians, public servants and business-men. Such an understanding, he holds, is imperative in order that science may be assimilated into the secular cultural pattern and thereby become an element of strength to so many who "feel spiritually lost in the modern world." Moreover, he holds rightly that in this age science and its applications have a profound bearing upon matters of public policy. On the other hand he has no sympathy with any idolatry of science.

In considering the pedagogic problem he prefers the historical approach to the analytical. So he is led to study examples of science in the making, and to propose "the establishment of one or more courses at the college level on the Tactics and Strategy of Science." Such courses would be based upon case histories chosen from certain fields of science ranging from the seventeenth to the nineteenth centuries. The subjects would be selected so as to bring out such fundamental principles as the difficulties attending each new advance; the importance of new techniques and of the controlled experiment; the interplay between experiment and observation and the growth

of new concepts and generalisations; and the development of science as an organised social activity.

As specific illustrations he gives interesting reviews of seventeenth-century work on the "spring of air," and of investigations in the following century concerning electricity and combustion. Here he points out two significant factors: that a useful but mistaken concept bearing the hall-mark of time may be a barrier to advance; and that experimental discoveries must fit the period in which they are made. In an acute analysis of the Phlogiston Theory he suggests that Lavoisier was led into the right track by reason of the very pronounced increase of weight occurring in the burning of phosphorus and sulphur—"the increase of weight arises from a prodigious quantity of air that is fixed," wrote Lavoisier on 1 November 1772, the key word being "prodigious."

These lectures afford much food for thought, and their interest is such as to fortify in the mind of the reader the modest hope expressed by the author that his proposals may lead to the institution of some trial courses of lectures based broadly upon the proposals he has expounded so ably in this publication. The book is rather highly priced.

JOHN READ.

An Outline of the Development of Science. By MANSEL DAVIES.
[Pp. viii + 214, with 26 figures, including 16 plates.] (London: Watts & Co., 1947. 3s. 6d. net.)

IN this recent addition to the *Thinker's Library*, Dr. Mansel Davies of Leeds University sketches the growth of science, which he defines as "the conscious effort man has made to understand and control his environment."

Beginning with the ancient Eastern civilizations, the story passes rapidly through the Greek and mediæval periods to the dawn of European science. The achievements of the Renaissance and the age of Newton serve to exemplify the characteristic aims and methods of the scientist, while a chapter on the "spirits" of the eighteenth century examines critically the various attempts to explain phenomena by reference to hypothetical fluids, æthers, vital forces and the like. A long account of physical science since 1800 is balanced by a sketch of the rise of biology which even finds room for such recent developments as the synthesis of penicillin. The concluding pages are concerned with the scientific study of man—his descent, behaviour and community life—conceived as linking the traditional sciences with the humanities.

Dr. Davies has not overloaded his little book, but has carefully selected representative topics for brief but lucid explanation. One or two points call for correction. Thales of Miletus is wrongly credited (p. 13) with describing, in the sixth century B.C., how a freely suspended piece of magnetite always sets in the same direction, a property unknown to the ancient world. On page 83 (line 15), *John* should read *James*; on page 105 (line 11), *felix* should in both cases read *felis*, and on page 174 (line 19), 1917 should read 1907.

The plates include portraits of scientists from Copernicus to Freud, together with illustrations of historic apparatus and of archaeological exhibits.

A. A.

MISCELLANEOUS

The Psychology of Ego-Involvements. By M. SHERIF and HADLEY CANTRIL. [Pp. viii + 525.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1947. 36s. net.)

"Ego" to these authors is not born complete or ever entirely completed by experience. It is a name for the facts, as they allege, firstly that, once the human organism achieves separate existence, no bit of its behaviour is ever wholly conditioned by stimuli acting upon a single or simple group of receptor-effector elements, but involves the active co-operation of a number of such systems beyond those immediately reached by the effective stimulus ; and, secondly, that certain of these actively co-operating groups come into play frequently, and whether because of this, or for some other reason, become firmly established. Then the organism looks upon them as its own. They influence markedly everything it does and their influence is called an "ego-involvement." They are always subject to change, though normally it can be shown to be change of an orderly kind, and is directly related, in important ways, to the general process of ageing. The name adopted by the authors for these actively co-operating receptor-effector systems is "attitude," a rather confusing name, but not disastrously so, provided their assigned character can be kept clearly in mind.

The general basis of the book being thus laid down, the argument proceeds in an extremely able and well-ordered manner, throughout its long course. Attitudes are thoroughly discussed with a wealth of experimental reference ; experiments demonstrating "ego-involvement" in the sense indicated above are described, the growth of the ego, with special reference to its re-formation in adolescence is given prolonged consideration, and particular attention is given to problems of social behaviour. Finally, there are discussions of how the ego may break down, and the book ends upon a note of frank and effective criticism of psychoanalysis.

Beyond question it is a good book. If at first sight it seems very long this is mainly because of its admirably full and documented account of concrete, and especially experimental, evidence. A short notice of this kind may produce the impression that the treatment is heavily theoretical and academic. It is not. It can be read with pleasure, not only by the professional psychologist, but by everybody for whom problems of human conduct carry a deep interest.

F. C. BARLETT.

Science News V. Edited by JOHN ENOGAT. [Pp. 168, with 16 plates and 13 figures.] (West Drayton : Penguin Books, Ltd., 1947. 1s. net.)

THE Penguin series of *Science News* has become a recognised easy means for the general reader of keeping up-to-date with modern scientific research. This number, the fifth, is more medical than its predecessors, but several other sciences are well represented, including Meteorology, Speleology, Physics and Biochemistry, all covered by well-known workers in these subjects. Dr. Sherwood Taylor writes an interesting account of British Chemistry during the last 100 years in celebration of the Centenary of the Chemical Society. The book is well illustrated with 16 pages of plates.

T. C.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

- A Philosophy of Mathematics.** By Louis O. Kattsoff, Associate Professor of Philosophy, University of North Carolina. Ames, Iowa: The Iowa State College Press, 1948. (Pp. x + 266.) \$5.00.
- Applied Differential Equations.** By F. E. Relton, M.A., D.Sc., Professor of Mathematics at the Faculty of Engineering, Egyptian University, Cairo. London and Glasgow: Blackie & Son, Ltd., 1948. (Pp. viii + 264, with 12 figures.) 20s. net.
- Computing Mechanisms and Linkages.** By Antonín Svoboda. Edited by Hubert M. James. Office of Scientific Research and Development, National Defense Research Committee. Massachusetts Institute of Technology Radiation Laboratory Series. New York and London: McGraw-Hill Book Co., Inc., 1948. (Pp. xii + 359, with 178 figures and 34 tables.) 27s. net.
- The Symbolic Method of Vector Analysis. The "j" Operator Simply Explained.** By W. H. Miller, A.M.I.E.E. Classifax Publications, 9 White Moss Avenue, Manchester 21. (Pp. ii + 28, with 16 figures.) 3s.
- Elements of Nomography.** By Raymond D. Douglass, Ph.D., Sc.D., Professor of Mathematics, and Douglas P. Adams, M.A., Assistant Professor of Graphics, Massachusetts Institute of Technology. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. x + 209, with 111 figures.) \$3.50.
- Theory of Servomechanisms.** Edited by Hubert M. James, Professor of Physics, Purdue University, Nathaniel B. Nichols, Director of Research, Taylor Instrument Companies, and Ralph S. Philips, Associate Professor of Mathematics, University of Southern California. Office of Scientific Research and Development, National Defense Research Committee. Massachusetts Institute of Technology Radiation Laboratory Series. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. xiv + 375, with 159 figures.) \$5.00.
- Theoretical Aerodynamics.** By L. M. Milne-Thomson, M.A., F.R.S.E., Professor of Mathematics in the Royal Naval College, Gresham Professor in Geometry. London: Macmillan & Co., Ltd., 1948. (Pp. xx + 363, with frontispiece and numerous figures.) 40s. net.
- The Solar System Analysed. Old Laws from a New Angle.** By F. C. Attwood. Auckland, N.Z.: The Dawson Printing Co., Ltd., 1947. (Pp. iv + 88, with 10 figures.)
- La Foudre.** Par Ch. Maurain, Membre de l'Institut, Professeur honoraire de Physique du Globe à l'Université de Paris. Collection Armand Colin, No. 248. Paris: Librairie Armand Colin, 1948. (Pp. 215, with 7 figures.) Frs. 120.-.

- Exclusion Principle and Quantum Mechanics.** Lecture given in Stockholm after the award of the Nobel Prize of Physics, 1945, by Wolfgang Pauli, Professor at the Swiss Federal Institute of Technology, Zürich. Neuchâtel: Éditions du Griffon, 1947. London Agents: H. K. Lewis & Co., Ltd. (Pp. 51.) 4s. 9d. net.
- Hyperfine Structure in Line Spectra and Nuclear Spin.** By S. Tolansky, D.Sc., Ph.D., D.I.C. Second edition. Methuen's Monographs on Physical Subjects. London: Methuen & Co., Ltd., 1948. (Pp. viii + 120, with 26 figures and 9 tables.) 6s. net.
- Cosmic Rays.** By L. Jánossy. The International Series of Monographs on Physics. Oxford: at the Clarendon Press, 1948. (Pp. xiv + 424, with 6 plates and 129 figures.) 35s. net.
- Applied Atomic Power.** By Edward S. C. Smith, M.A., Professor of Geology, Union College, A. H. Fox, Ph.D., Associate Professor of Mathematics, Union College, R. Tom Sawyer, B. of E.E., M.E., Manager, Research Department, American Locomotive Company, and H. R. Austin, Executive Vice President, The Kellogg Corporation. New York: Prentice-Hall, Inc.; London and Glasgow: Blackie & Son, Ltd., 1947. (Pp. xii + 227, with frontispiece and 31 figures.) 20s. net.
- The Cathode Ray Oscillograph in Industry.** By W. Wilson, D.Sc., B.E., M.I.E.E., M.Amer.I.E.E., M.Brit.I.R.E., M.I.I.A., F.Inst.P., Manager, G.E.C. Development Laboratory, Witton. Third edition. London: Chapman & Hall, Ltd., 1948. (Pp. xii + 252, with frontispiece and 197 figures.) 18s. net.
- Radon: Its Technique and Use.** By W. A. Jennings, B.Sc., A.Inst.P., Physicist to the Royal Northern Hospital and Prince of Wales's General Hospital Joint Radiotherapy Centre, and S. Russ, C.B.E., D.Sc., F.Inst.P., formerly Physicist to the Middlesex Hospital, London. London: John Murray, for the Middlesex Hospital Press, 1948. (Pp. x + 222, with 14 plates and 49 figures.) 18s. net.
- Technique of Microwave Measurements.** Edited by Carol G. Montgomery, Associate Professor of Physics, Yale University. Office of Scientific Research and Development, National Defense Research Committee. Massachusetts Institute of Technology Radiation Laboratory Series. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. xx + 939, with 627 figures and 56 tables.) \$10.00.
- Radar Beacons.** Edited by Arthur Roberts, Associate Professor of Physics, State University of Iowa. Office of Scientific Research and Development, National Defense Research Committee. Massachusetts Institute of Technology Radiation Laboratory Series. New York and London: McGraw-Hill Book Co., Inc., 1947. (Pp. xx + 489, with 246 figures and 12 tables.) \$6.00.
- Introduction to Wireless.** By W. E. Pearce, B.Sc. London: G. Bell & Sons, Ltd., 1948. (Pp. viii + 247, with frontispiece and 190 figures.) 7s. 6d. net.
- Electrons in Gases.** By Sir John Townsend, F.R.S., Hon. Fellow, New College, Oxford. London: Hutchinson's Scientific and Technical Publications, 1948. (Pp. viii + 166, with 31 figures and 37 tables.) 25s. net.
- Qualitative Analysis by Spot Tests. Inorganic and Organic Applications.** By Fritz Feigl, Eng., Dr. Sc., Laboratório da Produção Mineral, Ministério da Agricultura, Rio de Janeiro. Third English edition translated by Ralph E. Oesper, Ph.D., Professor of Chemistry, University of Cincinnati. Amsterdam and New York: Elsevier Publishing Co., Inc.;

- London : Cleaver-Hume Press, Ltd., 1947. (Pp. xvi + 574, with 45 figures and 37 tables.) 43s. net.
- Semimicro Qualitative Organic Analysis.** By Nicholas D. Cheronis, Chairman of the Department of Chemistry, Chicago City College (Wright Branch) and Director of the Synthetical Laboratories, and John B. Entriokin, Head of the Department of Chemistry, Centenary College of Louisiana. New York : Thomas Y. Crowell Company ; London : Constable & Co., Ltd., 1947. (Pp. xiv + 498, with 67 figures and 42 tables.) 25s. net.
- Physical Chemistry.** By E. D. Eastman, Ph.D., late Professor of Chemistry, and G. K. Rollefson, Ph.D., Professor of Chemistry, University of California. International Chemical Series. New York and London : McGraw-Hill Book Co., Inc., 1947. (Pp. viii + 504, with 89 figures and 45 tables.) \$4.50.
- A General Discussion on Swelling and Shrinking.** *Transactions of the Faraday Society*, Vol. XLII B, 1946. London : Gurney & Jackson, 1948. (Pp. 304, with numerous figures and tables.) 20s. net.
- Mechanical Behaviour of High Polymers.** By Turner Alfrey, Jr., Assistant Professor of Polymer Chemistry, Polytechnic Institute of Brooklyn. High Polymers, Vol. VI. New York and London : Interscience Publishers, Inc., 1948. (Pp. xiv + 581, with 246 figures and 27 tables.) 57s. net.
- The Chemistry of the Polysaccharides.** By Robert J. McIlroy, M.Sc. (N.Z.), Ph.D. (Birmingham), F.R.I.C., Senior Lecturer in Chemistry, Canterbury University College, New Zealand. London : Edward Arnold & Co., 1948. (Pp. viii + 118, with 5 figures.) 10s. 6d. net.
- Worked Examples in Chemistry.** By H. P. Starck, M.A., Head of the Science Department, Technical College, Kingston-on-Thames. London : Hutchinson's Scientific and Technical Publications, 1948. (Pp. 176.) 12s. 6d. net.
- Chemical Insect Attractants and Repellents.** By Vincent G. Dethier, A.M., Ph.D., Professor of Zoology and Entomology, The Ohio State University. U.S.A. : The Blakiston Co. ; London : H. K. Lewis & Co., Ltd., 1947. (Pp. xvi + 289, with 69 figures and 39 tables.) 25s. net.
- Nucleic Acids and Nucleoproteins.** Cold Spring Harbor Symposia on Quantitative Biology, Vol. XII. Cold Spring Harbor, L.I., N.Y. : The Biological Laboratory, 1947. (Pp. xii + 279, with 258 figures, including 20 plates.) \$7.00.
- The Fungi.** Vols. I and II. By Frederick A. Wolf, Department of Botany, Duke University, and Frederick T. Wolf, Department of Botany, Vanderbilt University. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1947. (Vol. I : pp. x + 438, with frontispiece and 153 figures ; Vol. II : pp. xii + 538, with frontispiece and 82 figures.) Vol. I, 36s. net ; Vol. II, 39s. net.
- Nature and Prevention of Plant Diseases.** By K. Starr Chester, Ph.D., Department of Botany and Plant Pathology, Oklahoma Agricultural and Mechanical College. Second edition. London : H. K. Lewis & Co., Ltd., 1947. (Pp. xii + 525, with frontispiece and 224 figures.) 25s. net.
- Soil Conservation : An International Study.** FAO Agricultural Studies No. 4. Washington, U.S.A. : Food and Agriculture Organisation of the United Nations, 1948. (Pp. viii + 192, with 97 photographs, 3 maps and 1 folding plate.) \$2.00.

- Animal Colour Changes and their Neurohumours.** A Survey of Investigations, 1910-1943. By George Howard Parker, Harvard University. Cambridge: at the University Press, 1948. (Pp. x + 377, with 126 figures and 18 tables.) 30s. net.
- The Bee Community.** By F. H. Metcalf, B.Sc. A Bee Craft Publication, Bracken Dene, Manor Way, Petts Wood, Kent, 1948. (Pp. 102, with 6 plates, 39 figures and 13 tables.) 7s. 6d. net.
- Harmony of Nature.** A Study in Co-operation for Existence. By L. Richmond Wheeler, Ph.D., M.Sc., B.A. (Lond.), F.L.S. London: Edward Arnold & Co., 1948. (Pp. viii + 200.) 10s. 6d. net.
- New Biology, IV.** Edited by M. L. Johnson and Michael Abercrombie. West Drayton: Penguin Books, Ltd., 1948. (Pp. 160, with 16 plates and 26 figures.) 1s. 6d. net.
- Hallmarks of Mankind.** By Frederic Wood Jones, D.Sc., M.B., B.S., F.R.S., F.R.C.S., Sir William Collins Professor of Human and Comparative Anatomy in the Royal College of Surgeons of England. London: Baillière, Tindall & Cox, 1948. (Pp. vi + 86, with 23 figures.) 10s. 6d. net.
- Recent Progress in Hormone Research.** The Proceedings of the Laurentian Hormone Conference. Vol. II. Edited by Gregory Pincus. New York: Academic Press, Inc. (Pp. vi + 427, with 148 figures and 94 tables.) \$8.00.
- Annual Review of Physiology, Vol. X.** Victor E. Hall, Editor, Jefferson M. Crimmon and Arthur C. Giese, Associate Editors, Stanford University. Stanford, California: Annual Reviews, Inc. and American Physiological Society, 1948. (Pp. xii + 552.) \$6.00.
- Lectures on the Liver and its Diseases.** Comprising the Lowell Lectures delivered at Boston, Massachusetts, in March 1947. By H. P. Himsworth, M.D., Professor of Medicine in the University of London, Director of the Medical Unit, University College Hospital, London. Oxford: Blackwell Scientific Publications, 1947. (Pp. xiv + 204, with 58 figures, including 1 coloured plate.) 18s. 6d. net.
- Déterminisme et Libre Arbitre.** Entretiens présidés par Ferdinand Gonseth, Professeur à l'École Polytechnique Fédérale. Recueillis et rédigés par H.-S. Gagnebin. Second edition. Neuchâtel: Éditions du Griffon, 1947. London Agents: H. K. Lewis & Co., Ltd. (Pp. 191.) 10s. net.
- The Limits of Science.** By the late Leon Chwistek, Ph.D. Translated from the Polish by Helen Charlotte Brodie, M.A., and Arthur P. Coleman, Ph.D. International Library of Psychology, Philosophy, and Scientific Method. London: Kogan Paul, Trench, Trubner & Co., Ltd., 1948. (Pp. lviii + 347, with 12 figures.) 30s. net.
- Notes on Descartes' Règles pour la Direction de l'Esprit.** By S. M. Jacob. Published by the Author and sold by the International Book Club, 11, Buckingham Street, London, W.C.2. (Pp. 36.) 4s. 6d. net.
- An Outline of the Development of Science.** By Mansel Davies. The Thinker's Library, No. 120. London: Watts & Co., 1947. (Pp. viii + 214, with 16 plates and 9 figures.) 3s. 6d. net.
- John Couch Adams and the Discovery of Neptune.** By W. M. Smart, M.A., D.Sc., Regius Professor of Astronomy in the University of Glasgow. Printed originally as No. 11 of *Occasional Notes of the Royal Astronomical Society*, August 1947. (Pp. 56, with 3 plates.) 5s. net.
- Yale Science. The First Hundred Years, 1701-1801.** By Louis McKeehan. Publication No. 18, Historical Library, Yale University School of

Medicine. New York: Henry Schuman, Inc., 1947. (Pp. x + 82, with 2 figures.) \$2.50.

The Early Work of Willard Gibbs in Applied Mechanics, comprising the text of his hitherto unpublished Ph.D. thesis and accounts of his mechanical inventions. Assembled by Lynde Phelps Wheeler, Everett Oyster Waters, and Samuel William Dudley. Publication No. 17, Historical Library, Yale University School of Medicine. New York: Henry Schuman, Inc., 1947. (Pp. viii + 78, with frontispiece, 2 plates and 33 figures.) \$3.00.

Scientific Discovery. By R. R. Butler, M.Sc., F.R.I.C., F.C.S. The County College Series. London: The English Universities Press, Ltd., 1947. (Pp. 252, with 71 plates.) 15s. net.

Scientists Starred, 1903-1943, in "American Men of Science." By Stephen Sargent Visher, Professor of Geography, Indiana University. Baltimore: The Johns Hopkins Press; London: Oxford University Press, 1947. (Pp. xxiv + 556, with 117 figures and 206 tables.) 25s. net.

Horology. By J. Eric Haswell, F.B.H.I. Reprint, with Supplement. London: Chapman & Hall, Ltd., 1947. (Pp. xvi + 288, with 16 plates and 107 figures.) 16s. net.

Also. The Failure in German Science. By Samuel A. Goudamit, Professor of Physics, Northwestern University. London: Sigma Books, Ltd., 1947. (Pp. xiv + 259, with frontispiece and 13 figures.) 15s. net.

Science News VI. West Drayton: Penguin Books, Ltd., 1948. (Pp. 144, with 16 plates and 7 figures.) 1s. 6d. net.

SCIENCE PROGRESS

NUTRITION AND LIVER DISEASE

By H. P. HIMSWORTH, M.D., F.R.C.P.

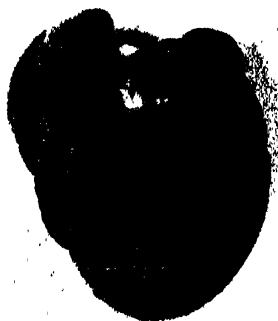
Professor of Medicine in the University of London; Director of the Medical Unit, University College Hospital Medical School, London

THE last ten years have seen such revolutionary changes in our attitude towards the causation and perpetuation of liver disease that it is not, perhaps, too much to hope that the impasse in this field has, at last, been broken. For long the only types of liver injury, the cause of which could be ascribed with any certainty, were those due to certain poisons. In respect of these the association between cause and effect was clear, and comparable damage could be reproduced in experimental animals by exposure to the same noxious agents. Later, although in the absence of experimental confirmation, a similar mechanism was deduced for certain infectious illnesses, such as infective hepatitis, for which a specific infective cause was known, and in which similar liver damage occurred. It was not, therefore, surprising that this conception of the production of hepatic injury by the intervention of external agents dominated the field of liver pathology, nor that it was assumed to be only a matter of time before most, if not all, types of liver disease yielded to a similar explanation. But, despite the most assiduous investigation, the vast majority of hepatic lesions continued to defy such attempts at explanation and all efforts at experimental reproduction. Then, in 1935, T. E. Weichselbaum, in a paper the importance of which was overlooked for several years, produced evidence that rats on a low protein diet developed what he called hæmorrhages into the liver. A few years later his results were confirmed, the hæmorrhages were recognised as areas of necrosis—sudden death—of liver cells, and, in addition, fibrotic lesions of the liver were produced by deficient diets. A new possibility had now emerged: liver injury might be due, not to the presence of a noxious agent, but to the absence of an essential nutriment.

As might be expected, the appreciation of the potential importance of this discovery was followed by an outburst of experimental

activity, and data rapidly accumulated. But there was no agreement as to the dietary factor the absence of which led to the liver damage, and it is no exaggeration to say that, at one time or other, every known dietary constituent, save carbohydrate, was indicted. The liver lesions produced included fatty infiltration, hæmorrhagic necrosis, and fibrosis ("cirrhosis") of the liver and, as the diets used by most investigators produced all three indiscriminately, it was not surprising that most workers concluded that the three lesions constituted successive stages of one sequence. Among the early workers only one group dissented from this view (Daft, Sebrell and Lillie, 1942), suggesting that necrosis and fibrosis were different conditions but, as they were unable to produce the one independently of the other, their suggestion did not command general assent. In 1944, however, the existence of two distinct sequences of liver injury, each attributable to a different dietary deficiency, was demonstrated in rats (Himsworth and Glynn, 1944). One led from acute necrosis to a coarse fibrosis of the liver, which was termed post-necrotic scarring (Fig. 1, Plate I); the other led insidiously, without any sudden necrosis, from fatty infiltration to a fine fibrosis termed diffuse hepatic fibrosis (Fig. 2, Plate I). Thus both sequences ended as "cirrhosis of the liver," and the differentiation was not between necrosis and fibrosis, but between fatty infiltration with its subsequent fibrosis, and necrosis with its resultant scarring.

The necrosis sequence is correlated with deficiency of protein, not the proportion in the diet but the amount eaten. Different proteins vary in their ability to protect against the lesion. Casein, for example, protects in amounts as small as 500 mg./rat/day, but even 1.0 g./rat/day of yeast protein is ineffective. Seeking for the reason for this difference, it was noted that protection was proportional to the amount of sulphur-containing amino-acids in the particular protein, and it was then found that adequate supplements of either methionine or cystine gave complete protection to rats receiving the deficient diets. The decision as to which of these two amino-acids was the essential protecting agent promised special difficulties, for methionine is converted to cystine in the body, and cystine spares the body's stores of methionine. Deficiency of either thus leads to necrosis. A fortunate observation enabled the problem to be solved. Methionine, but not cystine, deficiency was found to cause particular changes in the blood. Cystine prevented hepatic necrosis but not the blood changes; while traces of methionine prevented the blood changes but not necrosis. It was thus evident that dietetic hepatic necrosis was correlated with cystine deficiency (Glynn, Himsworth and Neuburger, 1945). A peculiar discrepancy



B

FIG. 1.—Post-necrotic scarring.

- A. Macroscopic, showing deep irregular scars separating normal liver tissue.
- B. Microscopic. Upper half of section, scar; lower half, normal liver.

FIG. 2.—Diffuse hepatic fibrosis.

- A. Macroscopic. Liver covered uniformly with fine granulations.
- B. Microscopic. Fine bands of fibrous tissue permeate the liver, destroying the normal architecture.

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now emerged. Reputable workers all over the world were confirming these results; equally reputable workers, apparently using the same diets, were failing to do so. The explanation was indicated by Paul Gyorgy (1947). Trying to discover why hepatic necrosis in his rats was common on some occasions, but absent at others, he noted that its varying incidence was correlated with the type of fat used. Analysis showed that protective fats were rich in vitamin E, and it was subsequently demonstrated that lack of vitamin E sensitised rats to deficiency of cystine.

When it was realised that diffuse hepatic fibrosis was the sequel to heavy, prolonged, fatty infiltration of the liver, the brilliant work of C. H. Best and his colleagues, on the factors responsible for fatty infiltration, lay ready to hand for application to this problem (Best and Lucas, 1943). They had shown that fatty infiltration might arise in two ways: from ingestion of a diet containing excessive amounts of fat; from deficiency of certain dietary factors which normally prevent the accumulation of fat in the liver. These latter they called lipotropic factors and they showed that deficiency of these, even when the diet contained no fat, led to fatty infiltration. The two most important lipotropic factors are choline and methionine. Their common property is the possession of a labile methyl group and, it is probable, that the lipotropic action of methionine is due to its being able to donate such a grouping and so augment the supply of choline when that is limited by dietary deficiency (Du Vigneaud, 1942-43). Methionine can thus serve as a source both of choline and of cystine and can, in consequence, protect both against acute necrosis and against fatty infiltration of the liver. Naturally this finding raised some doubts as to whether these two sequences were fundamentally distinct. Might not diffuse hepatic fibrosis be but post-necrotic scarring modified from its usual anatomical form by the presence of fatty infiltration? To test this possibility rats were given high fat diets, deficient in choline but rich in vitamin E, and containing casein and cystine each in sufficient amounts to protect completely against necrosis. Conspicuous diffuse hepatic fibrosis, without any sign of necrosis, developed (Glynn, Himsworth and Lindan, 1948).

The question now arises: Have these experimental lesions their counterparts in disease of the liver in man? It is at once apparent that lesions, anatomically indistinguishable, have long been known. Experimental, dietetic necrosis is of the type known as massive necrosis, for it wipes out whole lobules and not merely zones of cells within lobules. The human condition of acute yellow atrophy is indistinguishable. Acute yellow atrophy in man leads, in the

survivors, through the stage of subacute red atrophy to a coarsely fibrotic and nodular liver which Marchand (1895) first distinguished and Mallory (1911) called toxic cirrhosis. The necrotic sequence in experimental animals proceeds through the same stages, and the final stage—post-necrotic scarring—resembles closely toxic cirrhosis. Turning to the fatty infiltration sequence it can be said at once that fatty infiltration and diffuse hepatic fibrosis, indistinguishable from that produced experimentally, is well known in man. It has been known for years as the typical hepatic fibrosis of alcoholics but, unfortunately, has been, and continues to be, confused with post-necrotic scarring and consigned to the pathological limbo of "cirrhosis of the liver."

To deduce from these similarities that the lesions occurring in man are necessarily the result of dietary deficiency would be entirely unjustifiable. There is ample evidence that the ultimate form of any pathological lesion in the liver is determined, not by its cause, but by the anatomical form of the initial injury. Thus massive hepatic necrosis, whether produced by diet or by poisons, inevitably leads to post-necrotic scarring; prolonged heavy infiltration of the liver, irrespective of the nature of the infiltrating agent, to diffuse hepatic fibrosis. But in the absence of any other explanation of such hepatic lesions, the possibility of their nutritional origin deserves consideration.

Looking at the broad distribution of liver disease in the world it is evident that its incidence is generally correlated with malnutrition (Himsworth, 1947). It is particularly prevalent in tropical countries, but it is equally prevalent in colder climates, such as north China, where malnutrition is equally common. This high incidence in the tropics, naturally, suggested first that the explanation of its cause might lie in those diseases peculiar to the tropics but, with increasing experience of the pathology of such, this explanation of the frequency of the types of liver lesion under consideration has become increasingly doubtful.

Acute massive necrosis of the liver (acute yellow atrophy) is an uncommon disease anywhere. It is known to be associated with exposure to certain poisons, such as cinchophen and trinitrotoluene, and to be the form of death in the rare fatalities from infective hepatitis; but a considerable number of cases remain unexplained. Nevertheless, there are two known facts about its incidence which may be pertinent. Deaths from acute massive necrosis in malnourished native races suffering from infective hepatitis are twenty times as common as among well-nourished white troops suffering from the same infection. In temperate climates, under peace-time

conditions, over half the cases of acute necrosis occur in pregnant or nursing women. When one considers the natural provision by which the nutritional demands of the foetus are satisfied even, in times of stringency, at the expense of the mother, this latter observation may not be unimportant. But the general indication is that, in this form of liver lesion, a positive agent—poison or virus—is involved as the primary factor, and that the role of malnutrition is, in general, confined to that of exaggeration. Such a suggestion would agree with the experimental work of Whipple and his school (Miller and Whipple, 1942), who showed that doses of chloroform, which in normal dogs caused only a mild necrosis comparable to that of the ordinary mild case of infective hepatitis in man, produced in protein-depleted dogs a fatal massive lesion.

When we come to consider diffuse hepatic fibrosis in man we are on much firmer ground. As has already been said, this lesion, when seen in temperate climates, is traditionally associated with alcoholism. It has long puzzled those who would indict alcohol as its cause that an identical lesion is even more common in the tropics among native races which, by reason of poverty or religion, never touch alcohol. Further, among such races the condition is common among children. But, wherever in the tropics the incidence of this fibrosis is high, there malnutrition is prevalent.

In recent years considerable attention has been paid to a condition first described in central Africa and known as Kwashiokor. In its African form the condition is undoubtedly a complex of several deficiency diseases, but in various guises it has now been recognised among malnourished people throughout the world. In the famines after the last war cases appeared in Europe, large numbers being seen in Budapest during the siege. One common feature of this condition, wherever it is found, is a heavy fatty infiltration of the liver. By means of repeated sampling of the liver, the condition has been followed in the same patient, step by step, from fatty infiltration to diffuse fibrosis. At every stage the changes are indistinguishable from those in experimental animals receiving diets producing comparable degrees of fatty infiltration of the liver (Davies, 1948). Lastly, rats have been fed on the actual diets eaten by the Bantu races, among whom the condition is common, and the characteristic diffuse fibrosis developed (Gillman *et al.*, 1945).

On examining the diets of races subject to this condition they are found, invariably, to be poor in protein, almost devoid of fat, and to consist largely of carbohydrate. Evidently the explanation of the fatty infiltration to which they lead lies, not in an excess of dietary fat, but, at least in part, in a deficiency of lipotropic factors.

Crude foods with a high protein content are the main sources of lipotropic factors, and it has been found that the characteristic fatty infiltration of the liver can, in early cases, be cured by simply increasing the intake of milk (Waterlow, 1947). After the siege of Budapest was raised, and protein foods became available again, the condition disappeared and has remained absent. Whether other and simultaneous dietary deficiencies contribute to the development of the condition is as yet uncertain, but there is some evidence that certain vitamin deficiencies, particularly that of riboflavin, may play some part.

There can, therefore, be little doubt, not only that a sequence of pathological changes from fatty infiltration to diffuse hepatic fibrosis occurs in man, and is in every way comparable to that in experimental animals, but, further, that such a sequence is, at least in part, due to subsistence on a diet deficient in lipotropic factors. It remains now to see whether these findings can be related to the problem of "alcoholic cirrhosis" in temperate climates.

A century ago Rokitaniski (1849) commented on the frequency of fatty infiltration in the livers of alcoholics and expressed his opinion that, on the basis of this "adipose deposit," cirrhosis of the liver was laid down. The great French clinicians of the nineteenth century taught, as a matter of course, that there were two stages in the development of alcoholic cirrhosis. The first, in which the liver was enlarged and infiltrated with fat, they called steatosis of the liver; the second, in which the organ was small and fibrotic, they termed portal, atrophic, or Lænnec's cirrhosis. For some reason this knowledge did not take root and withered under the sterilising error which conceived hepatic fibrosis as a single entity—cirrhosis of the liver—instead of the similar end result of different initial injuries. But in the thirties of this century the facts were rediscovered by Connor (1939) and supported with such unequivocal evidence that the occurrence, in alcoholics, of the fatty infiltration, diffuse hepatic fibrosis sequence can no longer be doubted. It seems, therefore, that the problem of understanding, and controlling, "alcoholic cirrhosis" resolves itself into determining how fatty infiltration of the liver is brought about in alcoholics. Looked at from this point of view the explanation obtrudes itself at once. The majority of alcoholic drinks contain few, if any, lipotropic factors. Over-indulgence in alcohol causes gastritis and impairs appetite, particularly for those rich protein foods which supply such factors. Alcoholism leads to poverty and so limits still further the addict's ability to obtain an adequate diet. All these factors

lead to malnutrition, and it is common knowledge that deficiency diseases in general are frequent among alcoholics. In extreme cases addicts live for weeks on nothing more than a quart of whisky and a few biscuits each day. It is not difficult to see how such, and similar, diets produce the requisite degree of fatty infiltration at a speed proportional to their nutritional inadequacy. That alcohol itself contributes by directly injuring the liver is improbable for, despite assiduous efforts to demonstrate such an action, the evidence is still equivocal in the extreme. It thus appears that the traditional fibrosis of the liver found in chronic alcoholics can best be explained, not as the expression of a direct toxic action of alcohol, but as the result of malnutrition consequent upon alcoholism. As such it falls into line with the similar lesion of non-alcoholics in tropical races.

On the basis of these considerations it appears that dietary deficiency, in man, is capable of producing by itself the fatty infiltration, diffuse hepatic fibrosis sequence, and of facilitating the establishment of that other sequence which leads automatically from massive necrosis to post-necrotic scarring. The mechanisms by which these efforts are produced are now under investigation and already some headway has been made.

In the last analysis deficiency diseases are the expression of intracellular disturbances consequent upon lack of essential nutrients. From the point of view of intracellular function it matters not how this deficiency is brought about. It may arise as the direct result of an inadequate diet. But it may also arise when the diet is irreproachable, either because the cells are incapable of using particular nutriments or because these are prevented from reaching the cells. About the former we know little, but it is possible that the explanation of the action of many poisons will be found along these lines. Considerable knowledge has, however, been gained about the latter and indications of further possibilities are rapidly accumulating.

Broadly speaking, sufficient amounts of essential nutriments may fail to reach the cells for one of five reasons. There may be insufficient in the diet; they may be destroyed, or supplements may not be synthesised, in the intestine; they may fail to be absorbed; impaired circulation of the blood, either local or general, may restrict their transport to the cells; excessive amounts may be lost from the body.

Direct dietary deficiencies have already been considered and in a similar category falls malnutrition consequent upon intractable vomiting or lack of appetite. An interesting variant of this

mechanism concerns certain poisons. These produce massive necrotic lesions of the liver indistinguishable from those produced by cystine deficiency. Of such, selenium is an example. When rats are fed on grain grown on seleniferous soil such lesions develop, but are prevented by adequate supplements of protein or sulphur-containing amino-acids. Analysis of the proteins from seleniferous grain shows that selenium has replaced sulphur in the amino-acids which normally contain such. Apparently selenium-containing amino-acids are inutilisable. It is possible that trinitrotoluene, which combines with amino-acids, may produce its effects in a similar way (Himsworth and Glynn, 1944).

The important part played by intestinal bacteria in destroying ingested vitamins, and also in synthesising further supplies of certain of these substances, has only recently been appreciated. There is, however, as yet no evidence that the intestinal flora influence significantly the supplies of amino-acids.

Deficiency diseases, consequent upon failure to absorb requisite quantities of nutriment, are not uncommon in this country. The failure may be due to a mechanical defect, such as a communication between the stomach and colon, whereby food is short-circuited past the small intestine. It may be due to inflammation of the guts and the too rapid passage of their contents. Enlarged, grossly fatty livers are a feature in severe cases of gastro-enteritis in childhood, and recently it has been claimed that they yield to large supplements of choline and hydrolysed protein. Insufficiency of digestive enzymes is a rare condition, but it occurs in cystic fibrosis of the pancreas, in which condition quantities of undigested protein pass through the gut. Fatty infiltration of the liver followed by a diffuse hepatic fibrosis is found in this condition.

Excessive loss of nutriments from the body occurs in pregnancy and during lactation. If, further, by reason of vomiting or lack of appetite, the mother's food intake is curtailed, the consequences may be serious. The relative frequency of massive necrosis of the liver in such women has been mentioned already. Experimentally, more specific means for removing essential nutriments from the body are known. Nicotinamide is largely excreted in a methylated form. Its administration in excessive doses arrests growth and leads to heavy fatty infiltration of the liver. Supplements of choline prevent the latter; supplements of methionine prevent both.

But perhaps the most interesting type of conditioned nutritional deficiency is one which is not usually regarded from this point of view. It is obvious that a deficiency of blood-borne nutriments may arise within the cells of an organ either because the blood

reaching them is inadequately charged with these substances, or because, being adequately charged, insufficient blood flows through the organ. There is now considerable evidence that, in many liver diseases, significant retardation of the intrahepatic blood flow occurs and that, as a result, cells distant from the inflow receive blood largely depleted of its nutriment or oxygen, and suffer in consequence. In acute liver damage the cells are swollen with water; in chronic fatty infiltration they are swollen with fat. It is easy to demonstrate that such columns of swollen cells compress the blood channels which lie between them and so restrict the circulation. Deficiency of cystine leads to sudden dropsy of the liver cells; deficiency of lipotropic factors to prolonged distension with fat. The consequent mechanical disturbances of the circulation within the liver play their part in producing the damage associated with both. The effects of such restriction depend upon its degree. If practically complete, massive necrosis results; if less marked only those cells in the zones most distant from the inflow die; if mild, as in fatty infiltration, dramatic cellular death does not occur, only quiet degeneration (Himsworth, 1947). Finally, hepatic fibrosis itself interferes with the circulation. It has been known for some time that after fibrosis has reached a certain stage the condition progresses even though the exciting cause is removed (Cameron and Karunaratne, 1936). The contraction of the fibrous tissue obliterates blood vessels in their course and thus leads to death of the areas of liver they supply. More fibrosis replaces the dead liver tissue and so the process continues, inevitably and automatically, until the amount of functioning liver is reduced below the minimum. Then death ensues.

Even from this brief survey of developments in our knowledge of liver diseases during the last decade and a half, it appears that decisive progress is, at last, in train. Looking back now it appears that the impasse which, for so many years, held up investigators in this field was not entirely due to lack of knowledge. The conception that "cirrhosis of the liver" was an entity, and the assumption that because some injuries of the liver were due to the positive action of external agents, then all similar injuries should have a similar explanation, contributed in no small degree to this deadlock.

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AUGUSTIN FRESNEL : HIS TIME, LIFE AND WORK, 1788-1827*

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I

THIS is just a story. The scene is France : a cottage in Normandy, an office at the gates of Provence and a small bedroom at Ville d'Avray, where the end comes. There is no moral and little meaning ; the setting is romantic ; martial noises and other shouts are often heard. But the hero is frail and shy ; he tries hard to be commonplace, I do not know that it is a good story.

When the tale begins, France is tranquil. The American war has gone by almost unheeded, except in official circles. A commercial traveller in lightning-rods, spectacles and democracy has come to Versailles and has departed, much admired and greatly successful : Benjamin Franklin, as France saw him. The science of Physics, introduced into our drawing-rooms by Voltaire, is still very fashionable. In the Paris streets there is much talk of hard times ; business is slack, the Treasury empty (as usual) ; nobody in office seems to know quite what should be done : this is not new. Everybody not in office seems to know what should be done : the nation at large should be consulted : this is new. Bitter critics write in privately printed gazettes ; the Queen's conduct is openly derided. The town is good-humoured still, though restive : the year is 1787.

In the small village of Broglie (then called Chambrais), no disrespectful talk has yet been heard. This is the country seat of a family which came to France from Florence with the last Medici at the end of the sixteenth century. It soon became French and flourished, serving Crown and country in war and peace.

Marshal de Broglie, who fought with skill in the Seven Years War, was, in 1787, seventy ; the family owned wide tracts of land around the house of their name, on the south-eastern border of

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Normandy, near Bernay and Lisieux. The Broglie mansion, spared by our Revolution, is still there to-day, perched on a small hill, overlooking a little valley full of poplars and apple-trees. It is large, somewhat staid and dignified ; but it has simple lines, noble roofs, and the view from its stone terrace is both classical and soft, over the slate spire of the parish church. . . . Altogether, not an impressive castle, but a noble old building ; not a masterpiece, but a good house made to be lived in and where it was good to live. There is also, standing a little apart from house and stables, a dainty pavilion in which the architect has been completely successful: the estate agent dwelt there.

In 1787, this agent was called Mérimée and lived there with his wife and two grown children : honest and godfearing folks. The whimsical fancy of chance and of godfathers had presented these two children with curious christian names : the boy was Léonor and the girl Augustine. They had been educated at home, under the tender though unbending rule of their mother. A streak of what must be called puritanism in this language—jansenism, really—much simplicity and guilelessness, a great reserve in speech : a common enough atmosphere in the Normandy of these days. Even now, the most uncouth Norman peasant is a curiously diffident person ; he is able to talk louder and longer than anybody else, I believe, to be boisterous, violent and caustic—but only about things that are not dear to him. Should your conversation stray too close to people, things and thoughts he loves, he will not face it, his words will build a fog around the subject ; now is the time for him to offer you something more to drink, to laugh loud and talk of something else. Thus he hides from the stranger a few sacred and secret hoards of which nothing is ever said, even between father and son, or man and wife : things you have got to feel together, unexpressed—the family treasure and bond. A primitive feeling, a primitive force, which we may neither admire nor criticise : since your author was born in Normandy and because your nation, perchance, has inherited something of it.

In this guise Léonor and Augustine Mérimée were brought up. In this way their children will grow.

II

The sturdiest houses some time need repairs : in 1785, the stables and commons of Broglie were in a bad way. Estate agent François Mérimée travelled to Caen, the provincial capital ; there heard of a young man, one Jacques Fresnel, an architect and the

son of an architect, well spoken of. The appointment of Fresnel was proposed to the old duke and approved.

The job to be done was a long one, personal supervision necessary, Fresnel's health indifferent : the architect came to live at Broglie, where the estate agent put him up at his own house. Is there any need to go on with this part of the tale ? The work was only half done when Augustine Mérimée changed her name ; the first son, Louis-Jacques, was born when my tale begins and Augustin Fresnel was also christened at Broglie the next year, 1788. Later, two more boys came : Léonor and Fulgence. Meanwhile, Augustine's brother discovered himself to be a painter and the world soon agreed with him : his was the perfect career till one day when Monsieur Léonor Mérimée, Secrétaire de l'Académie Royale des Beaux-Arts, had a good look at his own work and decided it to be worse than bad : indifferent. He left off painting and spent the rest of his days in the study of painting technology : the manufacture and preparation of canvas, inks, paints and varnishes owe much to him, and many painters of my country still read his books on the subject. You may have guessed by now the christian name of his son : Prosper Mérimée, a great name in French literature.

The superficial contrast and subtle likeness between the two first cousins is quite striking : Prosper Mérimée, caustic, witty, man of the world ; Augustin Fresnel, retiring, religious, silent : both have inherited the same diffidence and hidden earnestness ; both will live their spiritual life secretly. Mérimée has tried hard to put nothing of himself in his tales, and the publication of his letters brought surprises to his best friends. Both had a respect for work well done, both had technological leanings. In 1860, the brilliant Prosper may have been the official wit of the Empress Eugénie ; but he also offhandedly appointed himself caretaker of our Cathedrals ; he knew much more about them than the greatest architects of his time . . . and often stayed the hand of romantic Viollet le Duc, when this master builder failed to perceive the difference between restoration and improvement.

Two brothers of Augustin Fresnel were engineers—one of them we shall meet again. Fulgence, the last of the four, was tempted by a scientific career, eventually decided for languages and learnt many of them ; loaned money to André Ampère and listened to his matrimonial woes ; went to Asia Minor as a Consul, travelled to Arabia ; acquired European fame as an orientalist and died in Bagdad : this practical strain, as you see, runs in the whole family.

III

Augustin Fresnel cannot have had any memories of Broglie Hall : he was much too young when the family prudently left this aristocratic neighbourhood, in 1789. Fresnel the elder wanted to go back to Caen. But the growing agitation having reached the town, he preferred to return to the small house in which he had been born, at Mathieu, five miles to the north of Caen. There he lived, almost in peace, his last few years, the only event in this part of his short life being his work of 1790 on the fortifications at Cherbourg, soon interrupted on account of the troubled times. At Mathieu, the sickly Augustin will spend twelve years of ailing childhood ; at Mathieu, his mother will remain, a widow, for twenty-five years.

This coy, smiling and rich farmland is known to-day all over the world and has been known to Britons for a thousand years. French Canadians and Cockneys went through Mathieu during the battle of 1944, wearing the same khaki uniform. Tales of these foreign troops, some of whom understood the dialect and were able to reply pungently, will be leisurely told for a long time, in the evenings, beside our log fires, while in the embers endlessly simmers the infamous brown brew which goes by the name of coffee. . . . One mile from Mathieu stands a borough whose name you know well : it is Douvres ; William of the bar sinister crossed it with his peers on his way to Hastings . . . many other famous ghosts walk there.

Some part of the intelligence of Augustin Fresnel seems to have awakened slowly : we have a tradition that, at the age of eight, he could not read well and had difficulty in remembering new words. There was no laziness there : all his life, Fresnel was to loathe even the simplest exercises of memory ; brother of a man who masters Arabic, his own language is the only one he will ever know well.

This small boy was not thought much of by the local pedagogue. He was silent, serious and slow and yet, in the eyes of the children of the neighbourhood, he shone as a leader and an authority ; they called him *l'homme de génie*. Tradition again tells us why.

Most of the fields of Normandy are surrounded by earth banks upon which tall hedges of elder bush and hazel trees grow. Throughout the country, the boys have for centuries cut hollow elder branches, emptied them of their pith, fitted in scraped and rounded hazel twigs. Elder pith is made up into pellets and dried in the shade. All the schools thus boast of a limitless supply of artillery. Augustin Fresnel, it seems, had specialised in the making of these toys, and, after careful consideration of the right dimensions for bore and for length, had succeeded so well that the boys of Mathieu

were able to lord it over all the neighbouring schools. It seems he went on experimenting in ballistics till his weapons became so effective that a deputation of frenzied farmers had to wait upon the schoolmaster; the supposedly slow-witted boy was then aged ten. I have not told here this seemingly insignificant episode just to win a smile from you, as time perhaps will show.

During these childish bucolics, the French Revolution had come, grown, raged, done much good and much harm. Five miles away, Caen was in a chronic state of uproar. One day, a girl called Charlotte Corday left the town with a small knife hidden in her gown; and Marat was murdered in his bath, Jean-Paul Marat who, before he went mad and master of Paris, had carried out researches in Physics and translated into French Newton's *Opticks* (1787). Marat went, Caen bowed under the storm. Robespierre rose and crashed. The Directoire was established, the war with England went on. Lazare Carnot was in charge; a son was born to him, whom some oriental whim made him call Sadi. The young Bonaparte swept away the Directoire, established a new order: nothing at all happened in Mathieu, sheltered from every wind. When Sadi Carnot was five, his father, now a private person, undertook his education: he had already decided that the boy was to go to the École Polytechnique. Madame Fresnel, in her small house, was dreaming the same dream. Caen was quiet again and it was high time to send the two older boys to a good school: Louis and Augustin went up to the École Centrale at Caen for three years—three years of obscurity. Augustin, who cannot work much, wins no particular honours, though we are told, for the first time, of a growing proficiency in Mathematics. In October 1803, Louis is examined in the annual competition for entry into the École Polytechnique and accepted: we shall not hear again of poor Louis, who perishes at twenty-two under Badajoz. In 1804, Augustin in his turn is accepted. In some subjects he has barely passed, but in Geometry his answers have surprised and delighted the examiner, already a well-known man: Legendre. The happy days are past and the frail boy of sixteen must now live a man's life.

IV

Can you imagine what life must have been in that new school suddenly opened in the capital city of an old nation which had as suddenly burst back into youth? The Napoleonic spirit was everywhere. Students who wore—and still wear—a military uniform; Professors and Assistants (most of them under thirty) whose names were Laplace, Ampère, Poisson, Cauchy, Monge, Gay-Lussac;

lectures brought to an end at the sound of drums. Napoléon was first and foremost an artillery officer ; in the École Polytechnique, he meant officers to be trained first of all. Second in his thoughts came the Administration he was then building : an army of engineers and civil servants, of roadbuilders and of peacemakers sent everywhere in the country where the last echoes of the great storm were slow to die : those ungrateful pupils who were not strong enough to give good military service had to staff this second army.

Since the École's records, still preserved, begin in 1808, we know next to nothing of Fresnel's life there. His health did not improve, though it does not appear he ever had to stop attending. He seems to have excelled in the graphic arts and in Geometry, to have made few or no friends. Lack of confidence in his own powers, combined with sudden bursts of audacity and craving after original work, must have kept him in an unhappy state of mind. What is sure is that the École instructed but did not inhibit him. Submissive he was, a faithful servant to his country he meant to be, a royalist he remained at heart : the family traditions and ties were stronger than the autocratic machine. The Revolution, which has spared the Mérimées, has brought to them all a horror of violence, a yearning for tolerance and liberalism : Fresnel's royalism is not the old unquestioning fealty to the Crown. He believes that no set-backs are possible in History : when Louis XVIII comes to Paris, Fresnel's allegiance will be completely sincere, because the King grants a Constitution and a Parliament.

His years at school end in a stay of a few months at the École des Ponts et Chaussées. Now an engineer, the Emperor sends him as a subaltern in the west, département de Vendée, the seat of the royalist rising which the Convention had barely managed to stifle. There Napoléon has built a town, a town with straight streets and big barracks. Roads are now needed, wide straight roads that will do away with these winding and covered lanes where so many men have lost their lives : this is Fresnel's work.

He is not fitted for it. Riding about the country : a tiring business. Looking for shingle beds, checking overseers' accounts : a tedious work, unrelieved by the emergence of stimulating new technical problems—nature is here very tame, if the inhabitants are not. More uncongenial to him than everything else, a great many people waiting to be ordered about. The shy and meditative boy loathes the authority vested in him, will loathe it all his life and will have to go on issuing orders all his life. "*Je ne trouve rien de si pénible que d'avoir à mener des hommes*" (There is nothing I loathe more than having to lead men).

The Imperial Administration cannot be bothered by such whims and fancies. Fresnel, always tired, always ailing, manages to keep abreast of his task. He has little or no time to read, he knows nothing of the scientific movement just beginning in Paris; but he gets promotion. He is sent to Nyons, in the Rhône valley—an intensely Bonapartist country—to work on a stretch of the great road leading from Spain into Italy. At Nyons, he is a little less bothered by the daily routine of his work and for the first time since he has left the *École Polytechnique* he can afford the luxury of real consecutive thinking. The trend of these first meditations is characteristic of the man, of his upbringing, of the youth and perhaps of his school: he seems to have tried to give a scientific basis to his metaphysical beliefs. But we shall never know for sure, since everything personal, almost every line of his not directly connected with his discoveries in Physics, has always been withheld and is to-day probably lost to us.

However, he soon turned away from speculations of this type. These are the days when Uncle Léonor Mérimée has passed judgment upon himself and there are reasons to think his technological and chemical interests have infected his nephew. Letters are exchanged about recipes for Indian ink and for glues; we find Fresnel proposing a new process for the manufacture of soda and Uncle Mérimée consulting several famous chemists about it. We find the same Mérimée presenting Ampère with a paper of Augustin's "fancies" and we see Ampère losing, as a matter of course, the paper to which Fresnel will never again refer. At last we find, in one of these letters, the first hint of a new preoccupation of our road engineer; on May 15, 1814, he writes: "I have seen in the paper that, a few months ago, M. Biot has read at the Institut a very interesting paper on the polarisation of light. I have tried hard, but cannot think what the word means"; the letter goes on to ask for books and data on this question. It seems that this request was overlooked for some time, since in a letter dated December 20, 1814, Mérimée says: "I seem to remember your asking for a work of Biot on the polarisation of light: do you still wish me to send it along?"—and, on Arago's authority, we may state that in the first days of 1815 he was still waiting for knowledge.

We must remember that the course of studies, at the *École Polytechnique*, consisted almost exclusively of Mathematics and Mechanics (Ampère was Assistant Professor of Calculus). The only Professor of Physics, Hassenfratz, was one of the few old men on the staff. No mathematical physics was taught and the century-old discovery of polarisation by Huygens was not in the curriculum.

Please remember this date : on January 1, 1815, Augustin Fresnel was still ignorant of most of the fundamental facts in Physical Optics. He had eight years left for work, and twelve years to live ; he was twenty-seven.

This 1814 burst of activity is no sign of better health and no coincidence : Bonaparte was falling ; regular work had stopped everywhere and was resumed but slowly after the accession of the new king and the departure of the Allies. Fresnel, for a few months, must have been a gentleman of leisure. Chance, war and politics saw to it that this would happen again, and shortly.

In the spring of 1815, Napoléon starts from Elba, lands at Golfe Juan, speeds to Paris. Bulky Louis XVIII is carted over to Ghent. Augustin Fresnel unhooks from the wall a sword he has never used and hastens to Toulouse, there to join the loyalist army. He seriously believes in this army which does not believe in itself. But this haste is too much for his frail body : what with the rough journey and the ironical welcome he receives from his general, he has to take to his bed almost as soon as he has reported for duty. After a few days, finding it useless to stay, he returns to Nyons, there to be received by a hostile crowd which breaks his windows and threatens him. Of course he is cashiered and has to report to police headquarters at regular intervals.

This is one of the two most important acts of Napoléon Bonaparte—the other being the appointment of Ampère in 1805. To this expulsion we most probably owe the whole of Fresnel's scientific work. Now he has no daily task to perform, no duties, no prospects, no close friends. Now is the time to think and to learn. In April 1815, he spends most of his days in bed. In May, he applies for permission to go back to his mother at Mathieu ; this is granted in June. We know that, by the end of July, he has arrived. In September 1815 we find him writing the famous letter which begins thus : " I believe I have found the explanation and the laws of the coloured fringes one is able to discern in the shadows thrown by opaque bodies illuminated by a bright point source. . . . "

The question at once arises : what did he know of Young's work at this stage ? The three fundamental papers of Thomas Young had been read before the Royal Society in 1801, 1802 and 1803 and printed in the *Transactions*. In spite of the recurrent war, copies of these were regularly received in Paris. Arago and Biot knew of Young's work and do not seem to have bothered much about it : Newton's glory still silenced every contradictor and Young's trend

of thought is not congenial to the French mind. Passing through Paris, our obscure and diffident engineer was granted a casual interview by busy Arago, already a great man. He spoke of a belief in the theory of vibration, of a work in hand concerning the theory of stellar aberration. He was told that he was rushing at open doors—"il enfonçait des portes ouvertes"—referred to classical works and sent away a little more silent and a great deal more unsure of his own powers. On July 12, 1815, Arago remembers enough about him to write and for the first time recommend perusal of Young's papers and treatise of 1807. This Fresnel cannot do: he does not know a word of English and the only copies available are in Paris, where he is not allowed to reside. But the thought of this extant and unknown work is a torment: "Please tell me if anybody has already determined this law of diffraction by exact experiment"—his dread of having broken down another open door is very evident.

Of course, the History of Science is full of mathematical feats performed in a few days by men of genius. But soft! We have here a piece of inductive reasoning, backed by careful experimental work; and proof that no experiments could have been performed prior to the end of July. How did he set about it? He had with him no instruments of any kind. But there was a locksmith in Mathieu, who made him a bench; out of wires and cardboard he managed to make a rough micrometer. The sun was his only available source; he owned no heliostat. But his mother kept bees: a drop of honey filling a small hole provided him with a short focus lens which gave almost a fixed image of the sun. You may appreciate better now the pop-gun story and recognise in this grown-up experimenter the child engineer of the Normandy hedgerows.

VI

September 23, 1815: Augustin Fresnel's single lucky day: the epoch-making letter was posted on this day to Arago, in Paris.

Arago was Fresnel's senior by two years. A wonderfully open mind, a bright wit, a facility for every kind of public speaking, proficiency in many arts, a knack of making important friends, a flair for scientific questions likely to become fashionable, much talent, a dazzling brilliancy: he had all these and was, at that time, at the height of his career. We owe him much since, having all these gifts, he had managed to remain essentially warm-hearted, free at that time from prejudice and envy; we owe him much, since

he welcomed Fresnel's letter and acted upon it. Also, the talented Arago was no genius and this fact may well be taken into account at this stage.

Maybe you feel this is harsh treatment of a celebrated man. Please understand I have no desire to disparage Arago; I only mean that it has probably been lucky for us that Arago was no specialist, had not gone too deeply into Optical Theories before he met Fresnel: when he read his first paper and saw him adopting the undulatory theory, he did not experience the reaction of the professional, of the man who "knows all about it": he encouraged Fresnel to send his paper to the Secrétaire Perpétuel de l'Académie, got himself appointed to examine it, together with Poinso, and, realising that the young engineer could do no really accurate experimental work in his present situation, urged him to come to Paris, there to resume his experiments with his (Arago's) own help. This was not to happen at once, however.

This first paper of Fresnel makes to-day curious reading. It consists of three parts. Fresnel begins by a critical examination of Newton's emission theory, insisting on the fact that the "fits" rather obscurely assumed by this extraordinary man are easily and immediately interpreted on a vibrational basis. The second part is experimental; there is a clear and correct quantitative description of the fringes; the law of the phenomenon is correctly stated. The third and last part is a theoretical account of the fringes, on the basis of the vibration theory of light: and this is practically all wrong. But Fresnel has gone astray in a fruitful way; his explanation is based upon the principle of interference, which is correctly understood, though incorrectly stated; and his account of reflexion and refraction at a plane surface, also based upon the idea of interference, is substantially correct: when we err, we seldom manage to make such interesting mistakes.

On November 10, 1815, Fresnel sent an addendum to this first paper, in which he gave an elementary theory of colours exhibited by striated bodies—diffraction gratings—the same elementary theory that is still expounded to-day in our first-year physics classes in France. Here, again, the principle of interference is alone employed.

By that time, Napoléon had been overthrown a second time and Louis XVIII was back in Paris. Fresnel had reported for duty and had been waiting at Mathieu for news of his reappointment. This he received in the autumn and communicated to Arago with the second paper we have just mentioned; incidentally, the text of that letter—November 12, 1815—shows that he had not yet read any

of Young's writings on Optics ; we may say at once that he will read them for the first time in Paris, in the spring of 1816.

Fresnel's new duties were at Rennes, in Brittany. Arago felt sorry ; with the arrival of the second paper his interest in Fresnel had increased ; it was disappointing to have this young man spend his time in filling up puddles on the Brittany roads ; he could not let that go on. I have been lucky enough to find the original of one of his letters on behalf of Fresnel, to M. de Prony, his colleague and a high official in the Roads and Bridges Service. It is most desirable, Arago says (December 19, 1815), that this young engineer should get leave to come to Paris, there to carry on experimental work much needed for the verification of important theoretical ideas ; and what can his colleague Prony do about it ?

I do not know, from direct knowledge, anything about the French Civil Service in those days ; but one may guess that many other letters had to be written, many official strings pulled before Fresnel was allowed the extreme favour of a few months' leave. We have a measure of Arago's " pull," tenacity and enthusiasm in the fact that Fresnel seems to have arrived in Paris early in March 1816.

Work immediately began in earnest : Young's papers were at last on hand, intercourse with Arago and with other celebrated physicists of the day was to be had daily ; thinking was allowed at all hours. It is quite true that none of the men he now met had given as much thought to the subject as he himself had ; quite true that Young's writings could not be of direct help to him since he had worked out for himself the principle of interference. But every man who has once worked alone will believe that this new familiarity with other minds and ways of thinking induced Augustin Fresnel to undertake a thorough revision of the system he had begun to develop. Indeed, we have almost conclusive evidence of this ; bear with me a little :

His first step was to try an experiment devised in such a way that, if fringes appeared, they could only be caused by interference between two single beams : hence Fresnel's mirrors mentioned by Arago in the *Annales* for March 1816, as soon as the experiment had been performed. Incidentally, the wording of Arago's note indicates that he has not yet clearly grasped the full meaning of the theory.

Fresnel must have, after that, gone back to his work on diffraction fringes in the neighbourhood of the boundary of the shadow thrown by a screen, compared it critically with Young's experiment and come to the conclusion that two distinct classes of phenomena were involved, since we find him giving, for the first time, in a paper

dated July 15, 1816, the first (and rather hazy) outline of the zonal postulate combined with Huygens' principle.

A long interval follows, the reason for which will be explained presently. On April 20, 1818, a sealed letter from Fresnel is put into the hands of the Secrétaire Perpétuel: the contents of this are a clear statement of what we now call the Huygens-Fresnel postulate, with a thorough outline of the procedure for computing the amplitude of vibration arising at any given point from any given portion of a wave surface; the calculations are carried up to the introduction of the integrals $\int \sin \pi v^2 dv$ and $\int \cos \pi v^2 dv$.

The last stage occurs three months later. On July 29, 1818, Fresnel sends to the Académie his memoir on diffraction and interference, where all his work prior to this date is summed up, corrected and completed. No mistake now subsists; interference and diffraction are dealt with separately, the zonal postulate applied to three experimental cases: diffraction caused by the edge of a large screen, by a slit and by a wire. The paper is a masterpiece and well deserves its rank in the Classics of Science. You probably know in what circumstances it was written: in spite of Arago's championship, Fresnel's papers had met with strong opposition at the Institut: Laplace, Biot, Poisson were upholders of the Emission theory. In 1817, at their instigation, the Academy proposed for the award of the 1819 Prize for Mathematics the study of diffraction, stating the subject in such terms as to leave no doubts that hopes were entertained, in some quarters, of finding a victorious knight for Emission in distress. This decision seems to have taken Arago by surprise and made him wary: it was Ampère's insistence, not his, that ultimately persuaded Fresnel to compete. The story is famous of how Poisson, examining Fresnel's calculations, became aware of paradoxical consequences; of how he suggested an experiment in which as much light should be found, if Fresnel was right, in the shadow of a circular screen, at certain points on the optical axis of a beam, as if there were no obstruction at all in the path of the "rays"; of how the experiment was performed, and Poisson silenced: later, much later, zone plates have illustrated this. We are also aware that the Lady Emission had indeed found a champion and know that the examiners of 1819 did not think fit to disclose the name of the "able physicist" whose rival work was dismissed in five sentences of their 30-page report.

Thus was fought and won the first battle of a new epoch in Physics, in the dusty and petty surroundings of the Quai Conti.

VII

You remember that Fresnel had arrived in Paris, on leave, in the spring of 1816. By October, duty had claimed him again and he had to report at Rennes where he was put in charge of the *Ateliers de Charité* (Government workshops for the unemployed) then being opened in the Rennes district: an emergency creation designed to cope with a difficult social situation. We may guess what it must have meant to this retiring young man to assume the role of workhouse master, to coax or bully into work a miserable crowd of journeymen, discharged soldiers and loafers: this was the only time in his life when fortitude may have been on the point of deserting him.

Arago was distracted: the term is not too strong. These few months had been enough to ripen acquaintance into warm friendship: a singular tie between these two men, one unknown, silent, royalist, valetudinarian—the other celebrated, hale, hearty, liberal (at least) in politics. They now met on a footing of equality. They had worked together, and there is nothing to indicate that the better-known man ever tried in any way to derive unmerited honour from this collaboration. This common work was most important, too: in fact, they had been preparing for the second battle of Modern Optics. When Fresnel left Paris in October 1816, the study of crystalline Optics, of the interference of polarised light rays had begun. And here was Fresnel in Rennes, almost in a state of nervous prostration, quite unable to do anything worth while!

The summer of 1817 had come before Arago succeeded in getting his friend back in Paris, on a second short leave of absence; he toiled seven months more to get him regularly appointed as Engineer on the Oureq waterway: a whole year had been wasted, and Fresnel's health further impaired.

Work was resumed and the immediate goal reached by the end of 1818 with another classical paper (on the interference of polarised rays) published in the *Annales* for March 1819: it is redundant to comment on it here, since the propositions it contains are reproduced word for word in our textbooks. But mark well that no allusion is made there to the very evident fact that the hypothesis of longitudinal vibrations will not fit the facts now established.

The explanation for that is simple: the two men did not agree on the expediency of publishing a second revolutionary statement. We have Fresnel's word for the following story:

As early as the autumn of 1816, when Fresnel published a paper on colours exhibited by crystalline plates in polarised light, Ampère

had been much interested and, in his timid and blustering way, had suggested that the results might admit of an interpretation on the basis of vibrations which had no component along the direction of propagation. "This idea," Fresnel says, "I rejected at the time." It was a scandalous one: to explain propagation *in vacuo*, some all-pervading medium had to be assumed and had been accordingly assumed to exist by Fresnel, with the extreme economy of words characteristic of his attitude of mind: an inductive one throughout his life, reluctant to make any but the most general assumptions. The properties of this ghostly medium Fresnel had supposed in an implicit way to be those of a fluid; but some sort of cohesion, of rigidity would have to be introduced if one was now to consider the propagation of transverse vibrations. Fresnel toyed from time to time with this repugnant idea.

Then came a letter from Young to Arago—April 29, 1818—a letter which is unfortunately lost. Young, whose interest in Optics had been rekindled by Fresnel's work, as of course you know, compared luminous vibrations to those of a "tautened string." It seems this was the last spark; Fresnel began to consider earnestly the new hypothesis. Observe what that means: "I cannot understand it; yet, I find no other way out; and only a few share my opinion." No wonder our diffident hero felt uncomfortable: this was a new attitude in scientific thought; we cannot feel like that any more, having grown accustomed to assume things which we do not understand.

Now, the colours of crystalline plates had been explained by Biot, on an emission basis; he had evolved for the purpose his complicated theory of moving polarisation; this he had cut and sewn to fit his own facts, and it fitted them very well; unhappily, it fitted nothing else, as Fresnel proceeded to show by changing the experimental conditions; these shattering experiments Fresnel published: a bitter controversy began between Biot and himself—a useless one, since the two men were unable to understand each other—and it was in the midst of this domestic war that Fresnel put before Arago his new theory and proposed a joint publication. Friendship does not always exclude caution: Arago quailed and the third great memoir, that of 1821, was published under Fresnel's single name. The end of this (under the heading: *Mechanical Considerations on the Polarisation of Light*) makes quaint reading: the author attempts some half-hearted reasoning designed to show that transverse vibrations may be propagated by a fluid, *provided this is endowed with a structure, some regular arrangement of molecules*. He does not carry it very far: obviously, he is not interested

in "explanations"; his sole aim is to make the hypothesis of transverse vibrations appear a little more palatable to his enlightened public—the ten physioists in the Académie.

This public, Arago felt, would find it preposterous: he was mistaken. At the same moment as he hung back, a greater man appeared at the bar for Fresnel. M. le Marquis de Laplace publicly owned that, long an upholder of the Emission theory, Fresnel's last paper had at last converted him: a new corpus, a new theory appeared to him to link henceforth in a homogeneous and satisfactory manner the phenomena of diffraction and of polarisation; he expressed a hope that Fresnel would go on. Thus was won the second battle, which was the last. True, the controversy raged on; when abandoned by Biot, it was carried on by Poisson and was still going on in 1824; when it perforce had to cease: Fresnel having no strength left to answer.

VIII

The tale is almost told. Fresnel has now complete command of his doctrine; he is fully aware of what he is doing and of what he still has to do. High time for him to be captain of his mind, since little more than two years are allowed him in which to finish his scientific work. The regularity and scope of this is truly amazing: discovery of double refraction in strained glass; of circular polarisation, with an explanation of the phenomenon; quantitative theory of the propagation of light in uni- and biaxial crystals: all this had been achieved by the end of 1823. Then silence comes. Fresnel will send one last note to the Académie, where it was read on June 13, 1825: it is an account of the first radiometer and the observed repulsion is clearly ascribed to differences of temperature. . . .

He never left the Roads and Bridges Administration. Even that body became aware, after some time, that it had in him no common servant. Director Becquey had him called in every time an optical problem had to be solved: thus it came about that he had to establish signals on the Ourcq canal, to find the means of lighting up the 8-foot clock dial of the Paris City Hall. He came to be appointed Secretary to the Commission for Lighthouses, and it was in that capacity that he invented, in 1820, the echelon lenses and began fitting the lighthouses of the French coast with toric and planes lenses of the new system: a work which was continued by his brother Léonor after his death. It always came first in his thoughts—no comparison with Newton's mysterious state of mind is here valid: this work was to him duty and as duty had to be done,

fully done before he felt himself at liberty to think of anything else. . . .

Year by year, his material life was lived with more difficulty and smaller scope. An appointment as examiner in the École Polytechnique, which he had diffidently accepted and which was to him an endless source of torment (he never felt he had awarded fair marks), had to be given up. Once, obeying a definite warning of his physician, we see him trying to break off his connection with the Roads and Bridges Service; he applies for a post at the École Navale; this application is turned down, on political grounds.

When the year 1824 began, he knew that he was nearing his end. He was told that, if he wanted to live a little longer, he must give up something of his activity. He said—once—"There was much more I should have done" and shut his scientific notebooks. Henceforth, all the energy he had left was given to his employers: he tried to finish his work on lighthouses.

This he could not do. The winter of 1826 found him too ill to go back to Mathieu for the last time. In the spring he was carried to Ville d'Avray, a small parish in a cleft of the hills between Saint-Cloud and Versailles, a delicate and soft village which later came to be a celebrated resort. There his mother came to him, and there he died on July 14, 1827, aged thirty-nine years, two months and a few days: the exact age of Blaise Pascal at his death.

This last name I have coupled with Fresnel's for good reasons: both are masters of inductive reasoning, both masters of style, both spiritualists; but the fire that consumed the otherwise normal body of Blaise was replaced in the weaker Augustin by the quiet fortitude of Vauvenargues. The Cartesian thought, flowing down with the current, is not for these men: their skiffs head upstream. On Fresnel's river were two successive water-gates. Standing on a hill, Thomas Young had seen over them a hazy and beautiful landscape. Fresnel opened the gates for everybody and went on, to the very spot where the river disappeared in the mountain-side. The map he drew remained unchanged with the advent of Maxwell, who changed the nature of the fluid flowing in the stream. That the river went on underground and came, perhaps, from Newton's own spring, Fresnel was not to know, as he could not guess that one of the two men who were to find this mysterious source would rise one day from the family his father had worked for: a curious coincidence.

I have already said that he was not concerned with "explanations." His aim was to avoid creating superfluous concepts and to fit the largest possible number of facts to the simplest and

hypotheses. He felt indeed reluctant to abandon the course of everyday logic and hesitated (a long time for such a man) before the necessity of transverse vibrations : but he eventually saw this necessity and faced it, with the result that in eight years he built a new science. That he was not satisfied in his mind is only too clear : his disciples, not he, fitted the mysterious ether with attributes and properties. His own thoughts were elsewhere : he seems to have had a glimpse of the difficulties which were to come from the consideration of moving sources and to the last, long after his polarisation theory was completed, we find him turning over in his mind again and again Newton's theory of fits.

His papers are masterpieces of the French language : no better teacher can be chosen by the scientific foreigner. His sincerity is complete : every mistake he has found he had committed he has owned up to and corrected ; every source of inspiration—Young, Ampère, Arago—he has fully acknowledged. Writing to convince and to be understood, he often adopts the deductive form. But this is, we may say, after the event and we find, here and there, traces of this fact : in a few cases, steps are missing, demonstrations are incorrect or insufficient : the equation of the wave surface in biaxial crystals is a celebrated example. No mean mathematician, he is not interested in pure mathematics : they were only one of the tools of his trade. When he met with $\int \sin \pi v^2 dv$, he did not waste

a moment in trying for a general integration : he was content with a numerical result, since he needed nothing else to carry on.

Recognition of his work came both late and early : he died too soon. Some of his French opponents never laid down their arms during his lifetime. It was in 1849 that the old Biot blurted out to his brother : " What a genius he had to see as soon as he did the transverse nature of light vibrations ! " But, in 1823, Fresnel entered the Académie des Sciences as a member and here, in Britain, Young's attitude was generosity itself, a fair measure of the greatness of the man who had guessed so much that Fresnel was to demonstrate. If Brewster's words were sometimes harsh, they were as nothing to what Young himself had to suffer at the hands of Henry Brougham ; and we remember that the Rumford Medal was awarded Fresnel in 1827 by the Royal Society, on the recommendation of Herschel. The poor Fresnel, lying in bed, could not even reply.

His bad luck survived him. His scientific papers had been assembled by Savary, who meant to publish a collected edition of his works. Savary died. De Senarmont succeeded him, and died also. Verdet came last and almost completed the work, Léonor

Fresnel revising the technical papers on lighthouses . . . and Verdet died too, before any printing could begin, before he could revise and correct his draft of the Introduction he had written for this edition. When the first of the three quarto volumes finally left the *Imprimerie Impériale* in 1866, Léonor Fresnel was 75 and Augustin had been dead thirty-nine years. . . .

Who was he ? He is hard to get at. His life has been most sad : since the curse of ill-health is probably the worst that can be put on a man. He never was well enough to be gay, never fit enough to afford the luxury of enthusiasm. He never rose on a sunny morning to feel spring in the air : since spring is never in the air, but in one's own bones. His mother seems to have been the only woman ever to approach him. Ordinary social intercourse he shunned with a timidity intensified by illness ; but he could be quietly daring when he was on his own ground. Laplace condescended, one day of 1818, to tell this young man that his theory of diffraction was impossibly complicated ; and this young man gently replied : " Nature does not shirk analytical difficulties."

" I like to search ; study bores me," he once wrote. Do not mistake his meaning : his short life was one long bout of strenuous work. All our human craving for beauty, all our striving for achievement, all passion and all joy he has vested in a single consummate feat of craftsmanship : a scientific theory to which he gave his own grave simplicity, his own taste for clever, generous and perfect designing. Son of an architect, cousin of Mérimée, I like to think of Fresnel as first and foremost an artist.

Who was he ? During the last war, I was living at Ville d'Avray and had one day to see the Mayor on business. Before leaving this worthy physician, it was only fitting to congratulate him on the many celebrities which the small town had some time known. I found this was his pet subject and listened with pleasure to anecdotes of Balzac writing endlessly in his small house, of Corot wandering along the lake, of Gambetta roaring in Les Jardies, of Mérimée strolling with great ladies in the Saint-Cloud woods. At last he paused for breath ; I chanced it : " And what of Fresnel ? "—" Who was he ? " said the Mayor.

INDETERMINISM AND THE WISH

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If a truth opposes no man's profit or pleasure, all men welcome it. This last sentence of Hobbes' *Leviathan* implies, rhetorically though not logically, that displeasing truth is not welcome. Bal-tasar Gracian, about the same time, notes the consequence: every man takes his views to suit his interests, and feeling usually perverts judgment. Earlier in the century, Sir Francis Bacon had realised how the wish can distort judgment, for, as he says, man always believes more readily what he prefers. In the nineteenth century Faraday knows how often the wish is father to the thought, and John Stuart Mill diagnoses this wish as the most frequent cause of bias. Wishful thinking is now a vogue phrase that rightly recognises an inveterate habit of the human mind.

Though, as Bacon puts it, feelings pervert understanding in innumerable, and sometimes in imperceptible, ways, the wish operates less in some domains than in others. If the equality between the three angles of a triangle and two angles of a square did prejudice the power of rulers, Hobbes remarks, authority would have tried to burn all books on geometry: the doctrine might be suppressed, but it could hardly be disputed. The wish has little direct sway over geometry, and less over physical science than over politics or economics. The *Novum Organum* seems to contemplate the possible distortion of even natural philosophy by preferences, and a pet hypothesis, for instance, or a reluctance to change or a desire for novelty may disturb even apparently rigorous science.

The disturbance may be great if the scientific doctrine touches deep interests or desires. Such powerful touch is evident in the Copernican or Darwinian offence to theology. The habits of the electron seem, at first sight, to be too remote from human wishes for physics to desire them to be other than they are, or to be what they are not. Indeterminism, however, seems to have disturbed such impartiality by allowing the wish to invade theory. Other "particles" are also concerned, but the electron is prominent in

the matter, and, by convenient *synechdoche*, can be used to represent them all. The modern situation has an interesting ancient parallel.

In the first cosmic moment of the Epicurean cosmology the atoms fell through infinite space, like showering rain-drops keeping their own places in the fall. Then, from time to time, some atoms swerved, collided with others, and their clashings begat the whole cosmic realm. These spasms of indeterminacy in which, as Lucretius sings, cause does not perpetually succeed cause, but swerves begin new starts, are old-world analogues of indeterministically conceived electrons. The analogy is more complete since the Epicurean atoms swerve casually from time to time through their whole cosmic course after their initial swerves—as Lucretius presents them. The movement of a flying arrow, as usually conceived, is the inevitable result of its initial direction, its initial speed, the pull of gravity upon it, the resistance of the air, and of any other relevant causes: the arrow moves as it must. In indeterministic physics there is no complete causal must for the electron, as there was not for the Epicurean atoms. The parallel between swerving Lucretian atoms and the indeterminism of electrons is still closer.

When a woodman fells a tree his arms swing the axe to make the keen edge bite through bark into trunk until the tree falls prone: these are recognisably physical events. He also selects the tree, plans his hews, and directs his strokes: these mental judgments and choices are discernible through the physical aspects of the whole causal complex. According to rigid universal determinism every flying chip inevitably flies as it does, and every intention or directive choice is unalternatively bound to be what it is. A metaphysical woodman might argue that he is a complex of inevitably acting causes. The normal woodman believes that he can, and often does, choose independently of these causes, to some extent at least. This is the protest of free will against determinism.

Epicurean atomism protested against the deterministic rigour of Democritean atomism. Man's will, Lucretius explains, can give a start, just as the casually swerving atoms can. The modern "atom" is not the Epicurean, and modern indeterministic physics does not compose the soul of special atoms as Epicurus did, but the modern indeterministic physicist looks to the electron much as Lucretius looked to the casually swerving atoms of Epicurus. The felt power to choose constantly conflicts with the sophisticated deterministic version. The physicist, like most men, dislikes being as much a strictly caused automaton as his motor-car. If the deterministic physics of the past does not prove him to be a rigidly deter-

mined run of causes, it does nothing to suggest that he is not. Indeterminism in physics does, for if the electrons in his brain do not suffer complete causal compulsion, his mind, which is intimately connected with his brain, need not suffer it either. Brain and mind are closely enough related for freedom from rigid causation in one domain to suggest a similar freedom in the other. Indeterminism in electrons does not demonstrate free will; it does suggest the likelihood. This seems reasonable enough in spite of demurs from some philosophers, and it has obviously seemed so to indeterministic physicists.

In 1927 Heisenberg's Principle of Uncertainty seems to Sir Arthur Eddington to have made science more sharply hostile to the predetermined future of strict causality, and ready to drop its "moral opposition to free-will." In 1931 he calls it a "gratuitous complication" to presume determinism in the mental when there is indeterminism in the physical. In 1935 he cautiously claims that physics has opened the "door of human freedom" by expelling determinism from physics, but only slightly. Sir Edmund Whittaker reveals the operation of desire on indeterministic physics in 1943: there is true indetermination in the emission of alpha particles from radium, and the world does thus contain fresh creations. In 1933 Max Planck tries to prevent the wish from perverting determinism by explaining that strict causality and free will belong to totally different domains. In 1942 Sir James Jeans notes how many look hopefully to the "indeterminacy of atomic motions" though, he comments, this does not touch the fundamental problem of free choice. A "capricious indeterminism" involves neither the freedom we want nor the free will we think we have. Free will may be only a name for "unconscious determinism"—a determination which seems to be freedom because it is not felt. The power of the wish is evident when Jeans admits that the new physics may disclose the desired freedom in a universe fit for free men.

The wish to confirm free will clearly pervades the indeterminism of physics; it seems also to be strong enough to compel many physicists to reject determinism too easily. The uranium, or radium, atom in particular seems to be made a quite unwarrantable guarantee of indeterminism.

Since radioactive disintegration reduces any bit of radium to half in 1600 years, only 50 per cent. of its atoms remain after that time. As of two women at a well one may be taken and the other left, or as of two men at one moment one may die and the other live, so of any two radium atoms at any moment one may pop and

the other remain. The two atoms behave differently either because they are different or in spite of being alike. Determinism, the inevitable causal outcome presumed in the former alternative, is, of course, a postulate. So is the indeterministic presumption of the latter alternative that different chances befall two like atoms. As the dying man is naturally presumed to differ from the one who lives, so the two atoms are naturally assumed to behave differently because they are differently caused. In 1931 Eddington quite arbitrarily treats the break up of a radium atom as a "presumably undetermined phenomenon," and in 1943 Whittaker, equally arbitrarily, again treats the emission of an alpha particle from radium as a true indetermination.

When a man is hanged his death is causally determined, but, according to indeterminism, the disrupting radium atom pops off by pure chance. The hazards and strains of human life help to decide its length, and so does physiological make-up. The varied interplay of physiological forces, which predisposes one man to die young and another to die old, is the natural analogy for the interplay of nuclear forces which predisposes one radium atom to disrupt before, often long before, its fellows. Varying forces are quite likely to be the determining causes of atomic disruptions in so complex a thing as the atom of radium, and they do not become unlikely or irrelevant by being unknown. When Eddington ignores them because they are unknown he exposes his *preference* for the indeterministic presumption.

Chance disruption covering an unknown play of causes does not infringe determinism; if it denies deciding causes it does. The arbitrary choice of the second indeterministic alternative is plainly a preference, for there is no need to abandon the usual postulate of determinism. Whittaker intimates the power of the preference by connecting fresh creations with alpha particles casually emitted from disrupting radium atoms.

According to one set of figures 50 per cent. of the babies born in England and Wales live to be seventy; 50 per cent. of the radium atoms in any present set will survive for sixteen hundred years. Babies differ very greatly from radium atoms, but the two statistical results, though they are differently derived, present a useful parallel. Each baby has a one to one chance of living to be seventy; each present radium atom has the same chance of surviving for sixteen hundred years longer. The restriction of prediction about the baby to a mathematical probability does not justify any indeterministic assumption about the length of its life; neither does the similar restriction for the radium atom justify its indeterminism.

The statistician only needs records of births and deaths to obtain his result. He need not know that they are births and deaths, for X's beginning and ending at recorded times will give the same statistical result. The statistical independence of operating causes warrants no indeterministic assumption about radium atoms, any more than it does about the babies. Statistics, Schrödinger notes in 1932, prudently and systematically ignores details. It is, therefore, by natural consequence, very independent of physical (or physiological) reality. The statistical probability of living to be seventy, as such, covers two alternatives equally well. Either many casual chances befalling many human beings present the 50 per cent. statistical result, or determinate causes, varying in each human being, present it. Analogously, two alternatives cover the 50 per cent. survival of the radium atoms for sixteen hundred years equally well: it results from an interplay either of chances or of varying determinate causes. The radium regularly halves in sixteen hundred years. This apparent determinism may conceal an indeterminism among many atoms that results in a constancy. Schrödinger notes a kinship between human and physical statistics. Any regularities in vital statistics would be very arbitrarily ascribed to an underlying indeterminism—to many deaths by many purely casual chances. It seems equally arbitrary to infer that the varied interplay of determinate causes, which is reasonably supposed for human beings, does not apply to radium atoms.

Schrödinger notes the exemplification by the "so-called disintegration of radioactive matter" of the "general law arising from a mass of chaotic events." The laws of chemistry and physics are "statistical throughout," he affirms in 1944, and these statistical laws depend on very many co-operating atoms. In 1871 Clerk Maxwell had commended the statistical method of dealing with groups of atoms because the course of each atom cannot be known. Gas pressure was one stimulus to the statistical version of scientific laws. Many constantly colliding molecules, moving in various directions with varying speeds, have a constant average impact on the containing walls. The total statistical consequence is independent, or relatively independent, of the actual behaviour by any single molecule, and any deduction about that behaviour, so far as it is possible, is only a probability. Schrödinger manifests the power of the preference in deciding for "The Law of Chance," for indeterminism against determinism.

The molecules may present a constant statistical average of either undetermined chances or of varying determined behaviours. Quantum theory raised this issue differently and widely after

1926. In 1919 Exner had inclined Schrödinger to the "thoroughly statistical character" of "Nature's laws." *Thoroughly* because "acausal" molecules of the gas, for instance, correspond to "undetermined chance" being "primary." This preference for genuine indeterminism, Schrödinger says in 1929, involves no decision about the real nature of natural events, but simply a choice between two predispositions, according as one or the other is judged to be the "more useful and convenient." Inherited custom, he had said in 1922, predisposes us to "*thinking causally*"; he himself prefers to discard the "rooted predilection for absolute Causality." He manifests a protestive preference against the customary postulate.

After 1926 quantum or wave mechanics steadily reconstructed notions of the electron and other "particles." In 1927 Heisenberg's Uncertainty Principle stimulated indeterminism in physics, and wave mechanics incorporated it. Electrons seem to behave now like particles, now like waves; they behave like particles, as Mott notes in 1947, when they hit matter, and like waves when they hurry about. Neutrons, protons, and their congeners also present this double nature. Wave mechanics is based on this doubled, and apparently contradictory, behaviour. According to Heisenberg's principle the exact path of an electron is fundamentally undetectable and unpredictable, for the position and velocity of the electron cannot be simultaneously known. In wave mechanics the waves expressing the *wave*-aspect of the electron indicate its probable position as a *particle*. The "probable" indicates the statistical element, and, as Jeans notes in 1942, the new physics replaces the classical concepts by "an assemblage of statistical laws." "Probability waves," mathematical expressions of probabilities, are not waves at all in any physical sense. Neither are the "particles" whose probable positions are assigned, particles in any physical sense if, as Whittaker affirms in 1942, "they can be represented only by mathematical expressions."

As Mott affirms in 1947, "quantum or wave mechanics" is a "system of equations." This describes the behaviour of electrons very effectively—how they interact with light, for example, or bind atoms into molecules. The equations do not even try to explain the double wave and particle aspects of electrons and their microscopic kin. The inevitably mathematical form of Heisenberg's results, writes Jeans, could not disclose the "true nature of physical processes or entities."

On May 26, 585 B.C., a solar eclipse stopped a battle between the Lydians and the Medes. The Chaldeans had discovered a cycle of 223 lunations in which eclipses, lunar and solar, repeat themselves

after approximately 18 years and 11 days. Thales predicted the darkening of the sun, somewhat luckily, from this Saros, without knowing what caused the eclipses. He did not need to know: periodical black-outs, mathematically formulated from observations, were enough. If he had supposed a monstrous crow to spread its obscuring wings over the sun at each solar eclipse, or presumed any other fantastic hypothesis, he could still have calculated from the Saros. Simple mathematics was very independent of physical reality then; as Jeans implies, the more complex mathematics of today is still very independent of it.

In 1930 Dirac can deal with "states corresponding to negative energy" mathematically, though they are physically nonsensical, and hope to discover a physical meaning. In the same year Dirac, in effect, drops a warning: mathematics is a tool, symbolic, well suited to abstract concepts, and physical ideas should be considered without too much deference to mathematical form. The wish seems to press indeterminism hard when this leans on an indeterminate electron which is not physically intelligible, and on a probability wave which is a mathematical equation. Quantum mechanics, Einstein once remarks, only claims to describe the probabilities of some contemplated reality, not "physical reality itself." Heisenberg's principle does seem to him to forbid the complete localisation of particles in "a theoretical model," but a "model of reality," "representing things themselves and not merely the probability of their occurrence," may still be possible. A persistent indeterminism based on statistically probable occurrences, also, does well to ponder a remark by Einstein: "our notions of physical reality can never be final." Physicists must be very wishful to base an insistent indeterminism on radioactivity, on statistical probabilities that are notoriously independent of actual operating causes, and on mathematically conceived "particles" located by probability waves. Indeterminism did not seduce Planck, though he disturbed determinism by plunging the quantum into physics.

When an athlete runs his 100 yards along a straight track, his speed can be measured by timing his start and then his finish on the tape. He cannot be accurately localised during his run, for as soon as he reaches one spot he leaves it. He must be stopped to be localised; and then his speed cannot be measured. This does not justify any indeterminism in his motions. Any such macroscopic analogy is imperfectly applied to the electron, but it seems to be a warning against one rash assumption of indeterminism in the microscopic sphere.

High frequency radiation of big quanta locates the electron well,

but makes it move differently. Low frequency radiation alters the velocity less, but the location is indefinite. Heisenberg's equation indicates the size of the error in determining the velocity when the electron is well located, and vice versa. The Uncertainty Principle seems to indicate a fundamental impossibility of accurately determining the velocity and position of an electron. This impossibility depends upon a necessary *interference* with the particle during the attempt. It seems arbitrary to assume that the electron's track is actually indeterminate because the interference makes it so for the physicist. Every measurement, Planck comments in 1933, disturbs the measured event to some degree. By implication, the electron is a particularly serious case of interference, but here, as elsewhere, measurements disturbed by interference do not justify any presumption of indeterminism.

The vogue of indeterminism in 1933 seems to Planck to have a psychological explanation. "Psychological" can cover many predispositions. For Planck it covers the prompting by the success and value of "quantum theory" in applying probabilities to found physics ultimately on the "concept of probability." It also covers Schrödinger's preference for Exner's indeterministic rebellion against regnant determinism. The desire for the new pervades his claim to put the "burden of proof" on the upholders of "absolute causality." "Those who question it," those from whom he removes the burden, must surely give good reasons for abandoning the deterministic postulate. There is, of course, no question of *proof*, as Schrödinger himself recognises elsewhere. The "psychological" seems very clearly to cover a wish when a disrupting radium atom is arbitrarily supposed to be too like its persisting fellow not to be the sport of undetermined chance. Wishfulness also seems to control the deduction of indeterminism from mathematical probabilities—the conclusion that there are no interplaying causes because statistical probability is able to ignore them.

The power of the wish is directly evident when indeterminists connect incomplete causal control with free will, and indirectly when they accept indeterminism too easily.

The probability of a tossed coin falling head or tail is 1 in 2—ignoring further refinements in the calculation. A varied interplay of determinate causes, not of casual chances, enforces this probable inference, for a coin-tossing machine, by always projecting the same coin from the same position in the same way, can keep the causes constant, and may throw 100 per cent. heads or tails. Analogously, a die could probably be thrown to obtain a six more frequently than the probable 500 in 3000 times. Schrödinger calls

the score by "two dice cubes" in a single throw "a pure matter of chance." The "pure" misleads if taken too literally, for some determinate causes do operate. Varying interplays of determinate causes, also, produce varying scores much more probably than indiscriminate chances do. The coin-tossing machine conforms to this by getting constant results from constant causes.

Coins and dice are not electrons, but they warn indeterminists against misusing probabilities to identify varying interplays of determinate causes with casually undetermined chances.

The laws, the statistical laws, of physical science are said to hold within a probable relative error of $1/\sqrt{n}$, where n is the number of co-operating "molecules." This involves departures from strict statistical accuracy, especially when the participating units are few, as Schrödinger notes when he states the principle. Any actual variations from strict statistical uniformity are at least as likely to derive from varied interplays of determinate causes as from quite indeterminate events. The regularities involved within all the irregularities underlying statistical uniformities, indeed, seem to be rather curiously referred to purely casual chances. The wish constantly seems to be the father of indeterminism.

THE ELECTRONIC STRUCTURE OF SOLIDS

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SOLIDS AND ATOMS

THE study of phenomena of the solid state of matter forms one of the oldest branches of physics. Most of the experimental facts were indeed established long before the advent of the atomic era, yet their bewildering diversity and complexity had prevented any coherent explanation on classical lines. Perhaps more than any other field it needed the knowledge of the electronic structures of atoms to provide the essential clue for solving the jig-saw puzzle of the solid state.

The last two decades, following the final consolidation of atomic theory in quantum mechanics, have thus seen the rise of a consistent and well-founded theory of crystalline solids. This theory, dealing almost exclusively with the behaviour of electrons and their interaction with the crystal lattice, has proved to be a real step forward towards a full understanding of all aspects of the solid state. The theoretical developments bound up with the names of Bloch, Brillouin, Mott, Peierls, Seitz, Wilson and many others have gone hand in hand with considerable progress on the experimental side. Among the main advances in this direction have been the successful application of modern research tools, such as X-ray diffraction and soft X-ray spectroscopy and, perhaps of even greater importance, the use of physically and chemically well-defined materials, in many cases single crystals of very high purity and perfection, as specimens for the study of the properties of solids.

This article is an attempt to present the general pattern of the solid state as it now unfolds itself above the rugged background of the individual effects. Electric conduction and other typically electronic phenomena have been selected to illustrate the general scheme. This choice is dictated by the available space and the special interests of the author and does not in any way reflect a restriction of the modern electron theory to these topics.

THE BAND THEORY OF SOLIDS .

The theoretical model of a solid is based on the concept of electronic energy bands most conveniently introduced by recalling certain essential features of atomic theory. A large number of energy levels is available for the atomic electrons. The Pauli principle does not allow more than two electrons (one of each "spin" direction) to occupy a given level. This leads to an unambiguous allocation of the available electrons to the various levels by filling up the levels from that of lowest energy. Successive groups of levels are associated with electron "shells" which, when completely filled, have a negligible interaction with outside electric and magnetic fields. Atoms having only filled or "closed" shells are therefore physically and chemically inert, the simplest example being helium having two electrons to complete the lowest "K" shell. The supernumerary or "valency" electrons occupying levels beyond those of the closed shells are responsible for the physical and chemical properties of the atoms. The gradual filling up of the various shells is expressed in the periodic table of the elements (see p. 626). The horizontal rows or "periods" correspond to the different incompletely filled shells, the vertical columns or "groups" to the number of valency electrons. The position of an element in the periodic table is related closely to the "ground state," i.e. the state of lowest total energy. Absorption of energy in the form of a quantum of light or by a collision of the second kind leads to a transition of the atom to an "excited state." The energy differences between the various excited states and the ground state determine the possible energies of the quanta of light which can be emitted or absorbed and hence the frequency of the spectral lines.

In solids the energy problem is approached by considering the assembly of atoms in a macroscopic crystal as a whole. In a collectivistic treatment, very characteristic of quantum mechanics, the crystal is regarded as a kind of giant atom consisting of a very large number of atom rumps, i.e. atoms stripped of their valency electrons, interacting with the total number of valency electrons derived from all the atoms of the crystal. The atom rumps are assumed to occupy the crystal lattice positions, whereas the electrons are not necessarily localised. In this model the possible energy values occur in distinct groups or "energy bands," each band consisting of a very large number (of the order of that of the atoms in the crystal) of very closely spaced levels. The Pauli principle plays the same fundamental rôle as in the case of free

atoms in governing the filling up of the energy bands with the available electrons. The actual number of the energy levels in a band and its relation to the number of electrons decides the physical and, in particular, the electrical properties of the solid. The application of the Pauli principle leads, for example, to a distinction between metals and insulators.

Metallic conduction and metallic properties are obtained whenever the total number of electrons is smaller than twice the total number of energy levels (Fig. 1a). Then one or more energy bands

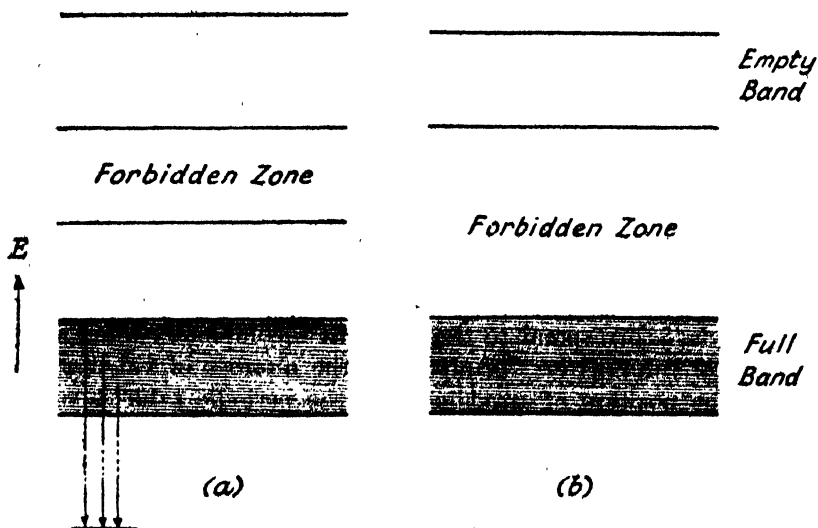


FIG. 1.

are only partly filled and electrons can move into higher unoccupied levels under the influence of an external electric field. This means physically that the electrons can be accelerated and acquire a drift in the direction of the electric field equivalent to an electric current.

An entirely different situation resembling that in a closed atomic shell arises if the number of electrons is just sufficient to fill one or more of the lowest bands and if the next band is separated by a large energy gap or forbidden zone (see Fig. 1b). Unless energy corresponding to the gap width, which is usually of the order of several electron volts (1 e-volt = energy of a light quantum of light of wavelength 12,340 Å), is introduced from outside, the upper bands will be empty and the electrons will be tightly packed in the filled bands. Under normal conditions they cannot acquire additional energy, since there are no neighbouring empty levels.

Hence, they cannot be accelerated in an electric field and so cannot contribute to an electric current. Solids of this type are, therefore, insulators, or, more correctly, non-metals, since it will be seen below that non-metallic conduction may arise in a different way.

COHESIVE FORCES AND SOLID TYPES

The full significance of the energy band scheme is brought out very clearly through a discussion of the binding forces responsible for the formation of the different solid types. Very weak interactions occur between stable electronic structures such as rare gas atoms, or the saturated molecules of the typical diatomic gases, hydrogen, nitrogen, etc. Van der Waals type forces, due to the mutual deformation of the electron clouds of these structures, are responsible for the deviation from the ideal gas state at normal temperatures and lead, at very low temperatures, to solidification. The energy bands of these van der Waals or "molecular" solids have positions closely related to those of the energy levels of the isolated atoms or molecules. Their widths are small compared with their separations: the drawing out of the atomic levels into bands can in fact be correlated with the presence of the interacting fields in the same way as the splitting of atomic levels in the Stark and Zeeman effects. Since each constituent atom or molecule contributes its filled levels to fill the energy bands, the solids are insulators.

The same holds for ionic or polar solids such as salts, oxides, sulphides, etc. Here cohesion is produced by the electrostatic forces of attraction between the ions of opposite polarity forming the crystal lattice. In the typically ionic alkali halides the constituent ions have a very stable rare gas structure; in rock salt (NaCl), for example, the sodium ion (Na^+) has the structure of neon and the chlorine ion (Cl^-) that of argon. Due to the very strong interionic electric fields the correlation between the ionic energy levels and the band positions of the solid is here not quite unique and the width of the bands is greater than that of van der Waals solids.

The binding forces of the remaining solid types can be understood on the basis of quantum mechanical exchange interaction. In covalent solids the bond is of the same nature as in the hydrogen molecule, where it is produced by the sharing between the two nuclei of the two electrons derived from the original atoms. Covalent insulating crystals are usually obtained from atoms with high valency, diamond (C), carborundum (SiC) and silica (SiO_2) being representative examples. In the crystal structure of diamond each

carbon atom is surrounded tetrahedrally by four others with which it shares its four valency electrons so that an unending interlinkage results. In all covalent crystals the valencies are completely used up or "saturated" and the electrons localised in particular bonds between two neighbouring atoms. In the energy scheme this saturation property is reflected as a complete filling of the bands.

The recognition that the binding electrons need not necessarily be localised, but can lead to "conjugated double bonds" by a kind of super-sharing between a larger group of atoms, has been most fruitful for the theory of aromatic compounds. In the modern model of the benzene ring the six valency electrons not used up in the ordinary covalent C—C and C—H bonds are shared by all the six carbon atoms and have a synchronised motion within the ring which gives the benzene molecule its characteristic stability. In graphite, where the C—H links of benzene are replaced by further C—C bonds, successive hexagonal rings join to form a flat honeycomb-like layer structure. The layers are held together by weak van der Waals forces, the ease of glide parallel to the layers being related to the special mechanical properties as shown by the ordinary graphite pencil or the lubricating action of graphite powder. The electrical properties, on the other hand, are entirely due to electronic effects within the layers. The sharing of the double-bond electrons extending from ring to ring leads to a nearly free motion of electrons through the whole layer and thus accounts for the typical anisotropic conductivity observed in graphite crystals.

Pauling [1] has recently applied a similar idea to explain the metallic state. In the alkali metals, the metals with the simplest electronic structure, the metallic interaction is derived from the molecular form of these elements. The diatomic form of Lithium, for example, is well known; the Li_2 molecule, in which two electrons are shared by two He-like ions, has a structure very similar to that of molecular hydrogen. The condensed metallic phase is assumed to arise from a lining up of the linear molecules in a closely spaced array resulting in a continual interchange in all directions of the bonding electrons between adjacent pairs of atoms. This change-over process, indicated in Fig. 2, is non-synchronised, which means the metal atoms are not permanently neutral but may accommodate an additional electron when bonds extend momentarily in two directions or may be ionised when temporarily freed from bonds. The bonding is thus spread throughout the crystal, stretching out from any atom to all neighbouring lattice points. Further work will have to show how far these ideas fall into line with the established wave-mechanical theory of the metallic state. They

provide at least a very vivid physical picture of the two functions of the metallic electrons. In the first place they can act as the cement holding the crystal together, and, secondly, they have sufficient freedom to move from point to point in the lattice and so act as the carriers of the electric current. In the alkali metals, the cohesion is exclusively of this non-synchronised "resonance" type and this explains the high conductivity and the characteristic softness of these metals. The different electrical and mechanical properties of metals in higher groups of the periodic table, particularly in iron and other transition metals, are, in Pauling's theory, associated with the fact that in these solids the typically metallic bond supplies only a small contribution besides strictly covalent bindings to the total cohesion.

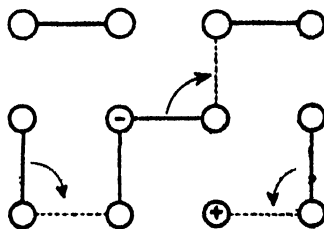


FIG. 2.

FERMI-DIRAC STATISTICS AND METALLIC CONDUCTION

The view that electrons in metals are essentially free to move and are responsible for electric conduction is supported by the results of the ingenious experiments of Tolman and Stewart [2]. They identified the current pulse observed in a metallic conductor when suddenly stopped from high-speed rotation as an inertia effect of the carriers of electricity, and established by careful measurements that the charge to mass ratio of these carriers is of the same order as that of free electrons.

It is possible to explain many features of metallic conduction by the theory of a free electron gas, applying statistical methods to obtain the distribution of the electrons on the almost infinite number of energy levels available in the conduction band. The quantum statistics of electrons, or Fermi-Dirac statistics, is again dominated by the Pauli principle. The number of electrons occupying the energy states as a function of temperature is given by the characteristic "Fermi Function" shown in Fig. 3a. In marked contrast to classical statistics, where at absolute zero of temperature all particles have zero energy, the distribution of a highly condensed or "degenerate" Fermi-Dirac "gas" of particles shows the phenomenon of zero point energy, all energy states up to a limiting value given by the Fermi energy E_{max} being filled by two electrons. E_{max} increases with the density of the electrons in the metal. At higher temperatures the drop at E_{max} is rounded off

but even at room temperature the extent of the rounding off, the "Maxwellian tail" of the distribution, is small. This means that a rise in temperature will lead to the moving up into higher energy

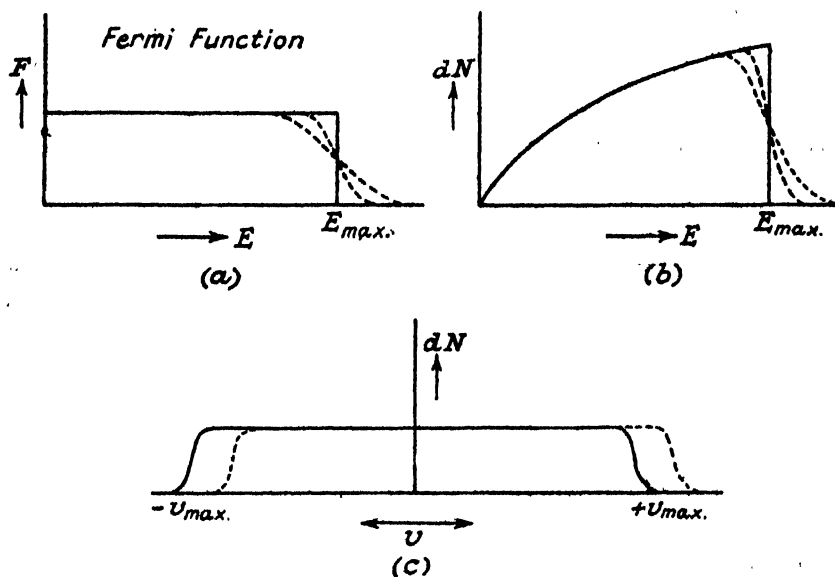


FIG. 3.

states of only a small number of electrons, so that the total energy content of the electrons increases but slightly. Fermi-Dirac statistics explains, therefore, why free electrons show only a negligible contribution to the specific heat of solids, a fact which was one of the major stumbling-blocks of classical electron theory.

In the free-electron theory the number of energy states in a given energy interval, the density of states, rises as $\sqrt{E}$, so that the energy distribution, giving the number of electrons in a small energy interval, has the simple shape shown in Fig. 3b, again showing the sharp drop at the Fermi energy.

Soft X-ray spectroscopy [3] affords direct experimental evidence of the Fermi-type energy distribution of metals and of the band structure of the energy scheme of solids in general. In the emission method developed mainly by Skinner in the Bristol Laboratories, the metal investigated forms the target of a low-voltage X-ray tube. The bombardment of the primary electron beam knocks electrons out of the inner closed shells of the metal atoms and thus creates vacancies in the normally filled very narrow bands below the valency electron bands (see Fig. 1a). The filling of these vacancies by

transition of electrons from the practically continuous valency electron band gives rise to a continuous low-energy X-ray spectrum, which is analysed and detected by a crystal or grating spectrograph contained in the same vacuum system. The intensity distribution of this soft X-ray band is mainly determined by the number of electrons in the various levels of the solid conduction band, i.e. by the energy distribution. Fig. 4 shows the actual microphotometer

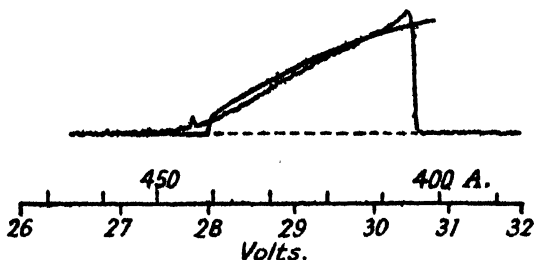


FIG. 4.

(From SKINNER, *Reports Progr. Physics*, 5, 207, 1938.)

curve of the X-ray band of sodium. The intensity distribution of the X-ray emission due to the transition into the L-shell of sodium shows very clearly the sharp drop at the high-energy end corresponding to the Fermi energy. The shape on the low-energy side fits the theoretical $\sqrt{E}$ relation for free electrons (indicated by a full line) except at low and high energies. The smooth rise at low energies is explained by X-ray excitation levels below the valency electron band. The hump just before the drop at the Fermi energy agrees satisfactorily with the modification of the distribution curve arising from the electron-lattice interaction and the resulting finite width of the conduction band. The soft X-ray method has also been successful in verifying the main features of the band theory for metals other than the good conductors of the first group of the periodic table, and even for certain non-metals, although in these cases where the free-electron approximation is not valid and complex wave-mechanical methods have to be used, the quantitative agreement between experimental and theoretical energy distributions is so far not very good.

The results of Fermi-Dirac statistics are essential for the theoretical interpretation of electric phenomena in metals. By considering the velocity distribution of electrons in the presence of an electric field (indicated schematically in Fig. 3c), it can be shown, for example, that only electrons with velocities corresponding to the Fermi energy contribute to the electric current in a metal,

This result very much simplifies the calculation of the electric resistance determined by the probability of collision or, in wave-mechanical language, by the scattering of electron waves. Wave-mechanics shows that electrons can move unhindered through a perfectly periodic lattice. The actual deviations from perfect periodicity by lattice imperfections on the one hand, and by lattice vibrations whose amplitudes increase with temperature on the other, very satisfactorily account for the residual or impurity resistance of metals and the temperature-dependent portion of the resistance respectively. The latter forms the major contribution of the total resistance in very pure specimens and vanishes at the absolute zero of temperature, increasing with a high power of T at very low temperatures, whereas at room temperatures the dependence is nearly linear. The total disappearance of resistance, both of the residual and temperature-dependent part, at a low but finite temperature in super-conductivity is an entirely different phenomenon for which an acceptable theory has been proposed only very recently.

ELECTRON TRANSFER EFFECTS

The very interesting group of phenomena relating to the escape of electrons from a metal surface or the transfer of electrons from one metal to another is determined by the existence of a surface potential. This rise of potential at the metal boundary can be explained as a surface polarisation, the electrical equivalent to the surface tension, produced by the asymmetry of the binding forces at the surface. Since all electrons contributing to conduction effects have energies equal to the Fermi energy E_{max} , the energy necessary for escape from the surface, the work function X , is given by $U - E_{\text{max}}$ (see Fig. 5a). In the surface photoelectric effect this energy is provided by the absorbed quantum. In thermionic emission it is supplied by thermal agitation, the Maxwellian tail of the Fermi energy distribution extending above the edge of the potential hill.

When bringing two different metals into contact, electrons flow across the boundary due to the initial difference in height of the highest occupied energy levels on the two sides (Fig. 5b). The resulting contact potential difference due to the equalisation of the Fermi levels is essentially given by the difference between the two work functions. A more rigorous application of the equilibrium condition requiring a detailed balancing of the electron densities on both sides leads to a small gap between the final position of the two Fermi-levels. The energy required or developed when an electron current flowing from one metal to the other passes over

this very small potential step is the Peltier heat. The direction and magnitude of the Peltier coefficient (of the order of a few milli-volts at room temperature) obviously depends on the lengths of the Maxwellian tails in the energy distributions of the two metals.

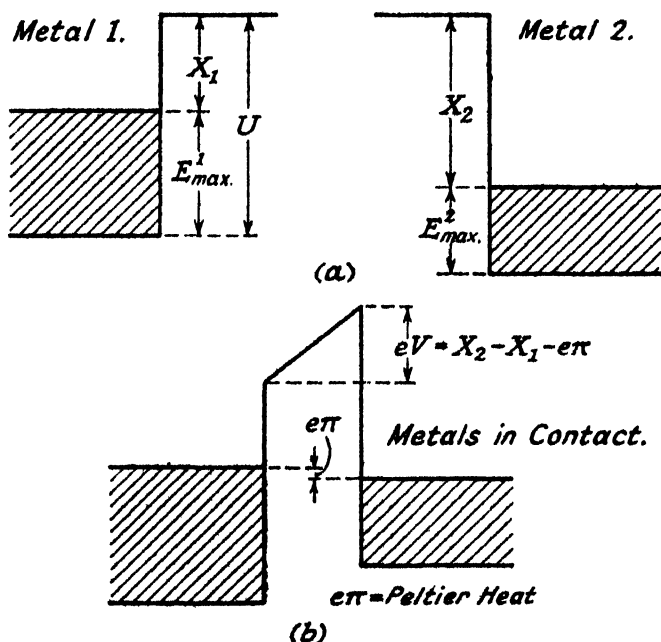


FIG. 5.—Work function and contact potential.

Since this increases with decreasing values of $E_{max.}$, the Peltier effect between normal metals is in such a direction that electrons will have to rise, i.e. heat will be absorbed, when moving from the metal with smaller $E_{max.}$ to that with higher $E_{max.}$ The Thomson effect, i.e. the liberation or consumption of heat due to electrons moving in a conductor through a temperature gradient, is explained in a similar way. The value of the thermo-E.M.F. (Seebeck effect) is most conveniently derived from the Peltier coefficient Π using the thermodynamic relation between the two effects, $\frac{\partial V_{Th}}{\partial T} = \frac{\Pi}{T}$.

The actual physical process even in an idealised thermoelectric circuit is rather complex. Peltier heat is developed and consumed at the cold and hot junctions, Thomson heat is developed and consumed along the two temperature gradients. From a purely electronic model it is thus not easy to see how the various heat gains and heat losses balance to yield energy to the external circuit. The

whole system is essentially a heat engine and the use of thermodynamics is very appropriate.

HALL EFFECT AND POSITIVE HOLES

An important feature of the thermoelectric effects is their linear dependence on the charge of the carriers of the electric current so that the polarity of the carriers is decisive for the direction of the effects. The same holds for the Hall effect, which concerns the potential difference observed at right angles to an electric current in a thin conducting plate placed normal to a magnetic field. The effect is explained by the bending of the electron paths due to the magnetic field, and for monovalent metals and many other metallic and non-metallic conductors its direction agrees with that expected from the negative sign of the electronic charge, the quantitative results being usually expressed by a negative value of the Hall coefficient. In a large number of multivalent metals and certain conducting non-metals, however, the potential difference is found to have the opposite direction, so that the Hall coefficient is positive. The mysteries of this anomalous Hall effect and corresponding anomalies in the direction of the thermoelectric phenomena, seemingly inexplicable on the grounds of electron theory, have found a very satisfactory solution in the positive-hole theory of wave-mechanics. The anomalies are due to the strange behaviour in an electric field of electrons occupying the top levels of an energy band. They will, therefore, occur whenever an energy band is nearly filled, *e.g.* when all energy levels are occupied except for a small number of vacancies. In this case, the total electrical effect is shown to be equivalent to that produced by a number of positive carriers equal to the number of vacancies or holes.

In metals, the occurrence of nearly filled bands and positive-hole conduction is bound up with the existence of overlapping bands. It is difficult to realise how energy bands can overlap and yet be distinct without considering the detailed interaction of the electrons with the crystal lattice. The theory leads to the idea of "Brillouin zones" which are related to the selective "Bragg" reflections of the electron waves at the various sets of lattice planes. For the purpose of this article it must be sufficient to accept as a result of this theory that the band structure of all multivalent metals has a complex character of overlapping regions. In this way the available electrons spread over into two bands leaving vacant levels in one and partially filling the other. Both the holes in the lower band and the electrons in the higher band take part in the conduc-

tion effects. The sign of the Hall coefficient depends on the relative strength of the two contributions.

PERIODIC TABLE AND SOLID TYPES

It is very illuminating to survey the incidence of the different types of metallic conduction and of the various kinds of solids in general in the periodic table of elements. The table reproduced on p. 626, an up-to-date version as given in an article by Paneth in the August issue of *Discovery* [4], shows the metals and the sign of the Hall coefficient. The relatively simple properties of the elements in the first group of the table, already discussed above, are due to the fact that all electrons can be accommodated in a single band whose width is considerably larger than the Fermi-energy. The elements of the second group are potential insulators, since their two valency electrons would be sufficient to fill the energy bands completely. The pronounced metallic character actually shown by all elements of this group is due to the overlapping of the bands, anomalous conduction existing here in at least three cases. The third group starts off with boron, an insulator, forming typically covalent crystals of great hardness but is followed by the well-conducting aluminium. The following three elements in the "b"-section of the third group, corresponding to the second branch of the long periods, Ga, In and Tl, show a very small electrical conductivity although definitely metallic in character. They are often classed as semi-metals. Their peculiar conduction effects are explained by the very small degree of overlapping between adjacent bands and a consequent small number of positive holes and free electrons. As, Sb, and Bi, in the fifth group of the table, are even better-known examples of this class of solid, Bi being distinguished by very large galvanomagnetic and thermoelectric effects.

Proceeding backwards from the right-hand end of the table, the rare gases solidifying as van der Waals solids are followed by the halogens whose electronic structure, being one electron short of closed-shell type, has a positive-hole character. The diatomic molecules formed by the halogens are, therefore, in a way complementary to those of the alkali metal vapours but solidify as insulating molecular crystals.

The placing of hydrogen in the same group as the halogens signifies that it lacks one electron to complete the K-shell of He. From the more familiar classification of hydrogen as the parent of the alkali metal series it would appear eminently qualified to form a metallic phase. Theoretical calculations have shown, however, that pressures of the order of half a million atmospheres would be

PERIODIC TABLE OF THE ELEMENTS

semi-metals; + - indicates sign of Hall coefficient where known; (+) (-) defect or excess semi-conductor.

Period.	Group I.		Group II.		Group III.		Group IV.		Group V.		Group VI.		Group VII.		Group VIII.	
	a	b	a	b	a	b	a	b	a	b	a	b	a	b	a	b
I													1H			2He
II	3Li —		4Be +			5B	6C —			7N	8O		9F			10Ne
III	11Na —		12Mg —			13Al —	14Si (+)(-)			15P —	16S		17Cl			18Ar
IV	19K —	20Cu —	20Ca	30Zn +	21Sc	31Ga —	22Ti —	32Ge (+)(-)	23V —	33As +	24Cr —	34Se (+)(-)	35Br	26Fe +	27Co +	28Ni —
V	37Rb —	47Ag —	38Sr	48Cd +	39Y	49In —	40Zr —	50Sn —	41Nb —	51Sb +	42Mo +	52Te (+)	53I	44Ru —	45Rh —	46Pd —
VI	55Cs —	79Au —	56Ba	80Hg	57-71 Rare Earths* 81Tl +	72Pb +	73Bi —	74Po	75At	76Og	77Ir +	78Pt —	79Au	80Hg	81Tl —	82Pb
VII	87Fr —		88Ra —		89Ac	90Th —	91Pa —	92U —	93Np —	94Pu —	95Am —	96Cm —	97Bk —	98Cf —	99Es —	100Fm —

required to free the electrons from the diatomic bonding and make them available for collective metallic interaction. In terms of the Pauling picture, the high pressures would be necessary to get the linear hydrogen molecules sufficiently near each other for the process of bond jumping to take place. It is thus very unlikely that metallic hydrogen will ever be observed under terrestrial conditions, although its possible existence in the interior of certain stars has been seriously considered.

In the sixth group of the periodic table, sulphur and its homologues Se and Te are of particular interest. All are decidedly non-metallic in their chemical properties and form complex molecular crystals. In the case of sulphur the crystal structure is based on a ring-shaped molecule of 8 sulphur atoms. But, whereas sulphur is a very good insulator, a considerable electrical conductivity is observed in the solid forms of Se and Te. A similar situation arises in the carbon series of the fourth group, carbon, silicon and germanium. The last two, forming strictly covalent crystals essentially non-metallic in character, show complex conduction effects in contrast to diamond, the covalent form of carbon, which is insulating under normal conditions. From the previous discussion it is clear that the graphite modification of carbon belongs to a different category. Since recent theoretical calculations have shown that its electronic band structure consists of directly adjacent bands, the lower one being practically filled, graphite can be considered as the extreme form of a semi-metal.

SEMI-CONDUCTORS

The conduction effects in non-metallic element solids, essentially different from the properties of semi-metals, are examples of the general phenomenon of semi-conduction observed in many solids, particularly those of the ionic type. Their interpretation in terms of the band theory rests on the assumption that ideally pure and perfect crystals of these solids would be insulating and their behaviour correctly represented by the energy scheme of Fig. 1b. Crystal imperfections and foreign inclusions give rise to impurity centres and lead to the appearance of additional levels in the basic energy scheme.

There are two types of impurity centres. In excess semi-conductors, such as zinc oxide or aluminium oxide, the impurity centres are electron donors leading to normally filled impurity levels just below the empty or conduction band (see Fig. 8). In defect semi-conductors, *e.g.* cuprous oxide, the impurity centres are electron acceptors leading to additional levels just above the filled band.

Thermal activation by means of the lattice vibrations lifts electrons from the donor levels into the conduction band or creates holes in the full band by lifting electrons into the acceptor levels. Electric conduction can, therefore, take place by the normal and positive-hole conduction processes respectively, the sign of the Hall coefficient providing a distinguishing criterion between the two types of semi-conduction. The number of carriers or holes depends on the tem-

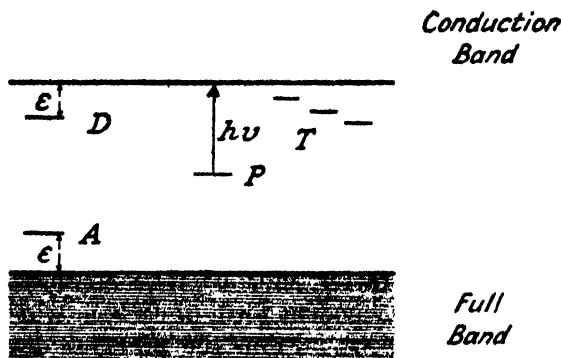


FIG. 6.

perature and on the activation energy ϵ , i.e. the energy separation between the impurity levels and the bands, as $\exp\left(-\frac{\epsilon}{2kT}\right)$. Hence the electrical conductivity of semi-conductors increases exponentially with temperature in sharp distinction to metallic conduction which decreases with increasing temperature, the resistance of metals rising with temperature.

Semi-conductors have acquired an immense technical importance, particularly in view of their use in metal rectifiers and photo-voltaic cells [5]. These are based on boundary effects at the interface between a metal and a semi-conductor whose theoretical interpretation is still far from satisfactory. A comprehensive theory will have to take account of additional surface states or "Tamm levels" for whose existence at metal semi-conductor contacts experimental evidence has lately been forthcoming.

COLOUR CENTRES IN ALKALI HALIDES AND IMPURITY PHOSPHORS

Many fascinating phenomena are associated with impurity levels whose energy separation from the conduction band is large compared with the available thermal energy. The properties of coloured

alkali halides, studied very extensively by Pohl and his school, provide ideal illustrations of such cases. Alkali halide crystals, which are normally transparent in the whole visible and ultra-violet spectrum, acquire a pronounced colour, due to a single symmetrical absorption band, when exposed to X-rays or a stream of electrons or when heated in the vapour of an alkali metal. The absorption occurs at a wavelength specific for each alkali halide (at -190°C ., NaCl 4500 Å, KCl 5400 Å, see Fig. 7) and is explained by the presence of colour centres or F-centres (German: Farbzentrum). A large amount of experimental material is now available to support the view that F-centres consist of excess electrons trapped at points in the crystal lattice where a negative halogen ion is missing. The production of centres thus becomes linked up with order-disorder phenomena [6] concerning the distribution and diffusion of vacancies in the regular crystal structure. In the energy scheme the F-centres are represented by normally filled levels between the filled and conduction bands (see Fig. 6). Absorption of light raises electrons from these levels into the conduction band. This accounts for the photoelectric current observed in an illuminated coloured crystal when placed in a strong electric field. The spectral response curve of the photoconductivity corresponds in position and width to the absorption curve for the colour centres.

Of even greater interest are effects resulting from an intense illumination, with light corresponding to the centre of the F-absorption, of crystals coloured by the alkali vapour method. The crystals are found to bleach, the F-absorption decreases and a new absorption band is formed in the near infra-red region (see Fig. 7). The new absorption band is associated with trapping centres, F'-centres, represented by energy levels considerably closer to the conduction band than the original F-levels (see T, Fig. 6). The bleaching is explained by assuming that the electrons raised into the conduction band drop into the F'-levels so that the number of filled F-levels or absorption centres is gradually decreased. The original absorption curve can be completely restored by infra-red irradiation or by heating the crystal to a moderately high temperature. This restoration process corresponds to the release of electrons from the trapping levels by the infra-red or thermal activation leading to their return into the F-levels. The electrical conductivity which can be observed during the restoration period is proof that the electrons pass on their return journey again through the conduction band.

The energy lost by the electrons when they return to the F-levels and thus restore the original or ground state of the colour centres

is dissipated in the form of lattice vibrations. In the luminescence of "impurity phosphors," on the other hand, at least part of the energy supplied in an original excitation process is emitted in the form of light. The impurity centres in phosphors are formed by a small concentration of foreign activator atoms artificially introduced into transparent base crystals. The possibility of a radiative return of electrons to the ground state of the centres is related to

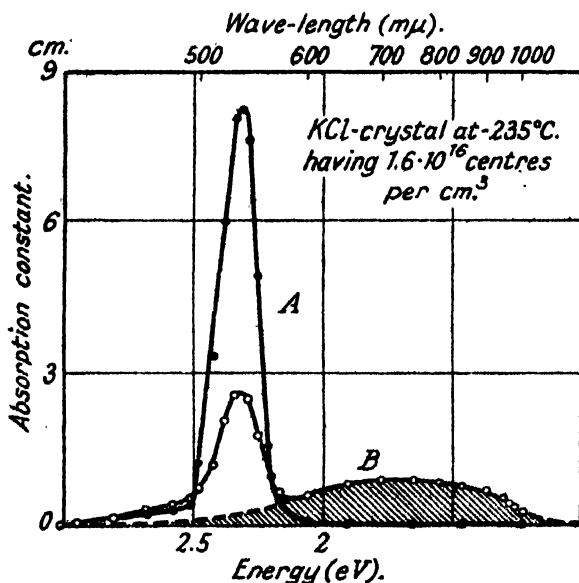


FIG. 7.

(From POHL, *Proc. Phys. Soc.*, 49 (extra part), 11, 1937.)

the peculiar electronic structure of the activators (e.g. rare earths or transition elements like Mn and Cu) in which a stable outer electron shell surrounding the valency electrons affords protection against interaction with lattice vibrations. The brilliant experimental investigations of Randall and his collaborators [7] have established that, apart from this fundamental difference in behaviour regarding the final distribution of the excitation energy, the mechanism of temperature-dependent phosphorescence runs closely parallel to the processes in alkali crystals just described. Electrons produced in the primary excitation by absorption of light in the colour centre pass through the conduction band leading to photoconductivity, and drop into shallow trapping levels (Fig. 6, T). Thermal agitation releases them from the traps so that light is emitted on their return to the centres. The delay between emission and excitation given by the time spent by the electrons in the traps

determines the decay constant of phosphorescence and depends on the temperature and the depth of the traps below the conduction band. The trap depth can be estimated by the ingenious method of observing thermal glow curves. A phosphor excited at liquid-air temperature is allowed to warm up very slowly. The weak radiation emitted as the traps are emptied by the gradually increasing thermal agitation and electrons sent back into the impurity

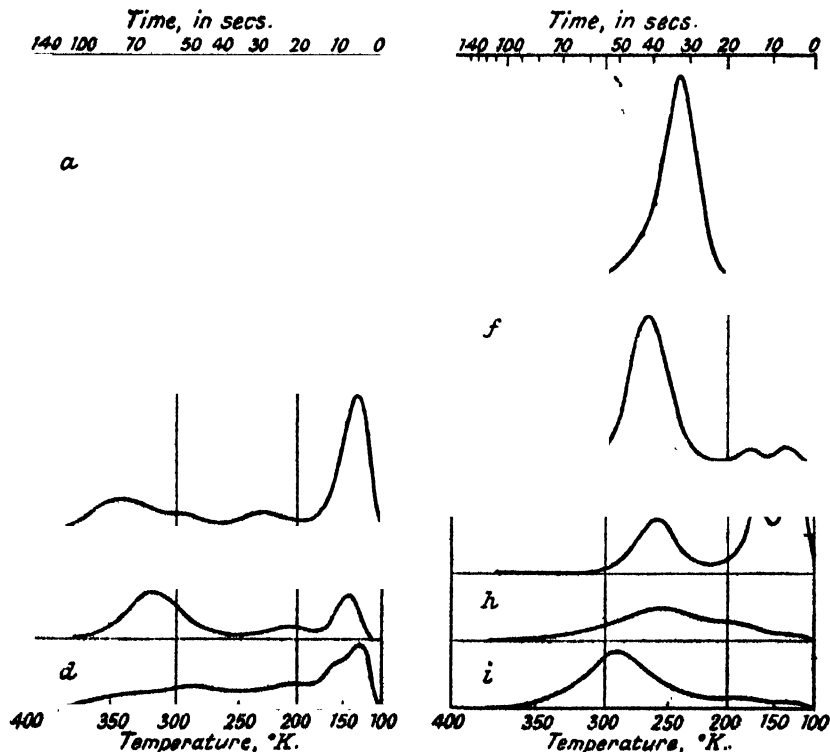


FIG. 8.—Thermal glow curves for phosphors activated by manganese.

a, cadmium borate; b, cadmium chlorophosphate; c, zinc mesodisilicate; d, cadmium silicate; e, green zinc silicate 0.5 per cent. Mn; f, zinc silicate 1 per cent. Mn; g, zinc silicate 0.1 per cent. Mn; h, zinc beryllium silicate Mn; i, zinc beryllium silicate 2 per cent. Mn.

(From RANDALL *et al.*, *Proc. Roy. Soc., A.*, 184, 376, 1946.)

centres, is observed by means of a very sensitive electron multiplier photo tube. Examples of the resulting glow curves are reproduced in Fig. 8. The appearance of distinct peaks indicates that the thermal energy has to reach a definite value before it can lead to the release of electrons from the traps, and it is thus seen from the curves that in some phosphors the traps are of almost uniform depth, in others there is a small number of different trap depths.

The way in which the very perspicuous effects in alkali halides have served as a model for reaching a fair understanding, if not a complete theoretical description, of the very complex luminescence phenomena is characteristic of many modern developments. The great value of the electronic theory of solids lies in this function of providing models of simple processes which will no doubt lead to future progress in many fields.

The author is indebted to the Physical Society for permission to reproduce Figs. 4 and 7, and to the Royal Society for permission to reproduce Fig. 8.

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THEORETICAL ASPECTS OF CLAY CONSOLIDATION

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WHEN a building is constructed above a stratum of clay bounded above and below by more porous strata, its foundations may undergo appreciable settlement as the water is forced from the clay by the weight of the building. The term "consolidation" is used in soil mechanics to refer to this change in moisture-content and volume of the clay under pressure. The rate at which the settlement occurs depends on the nature of the clay, and the problem is that of predicting from laboratory experiments the magnitude of the effect to be expected and the time necessary for it to become complete. Standard methods of laboratory testing of clays have been recognised (Gilboy, 1936). The apparatus used is known as an "œdometer" (Cooling and Skempton, 1940), and consists essentially of a cylinder within which the practical conditions may be copied in principle by compressing a sample of the clay between porous pistons.

The conventional approach of soil mechanics to the theory involved is due to Terzaghi (1925) and, following Taylor and Marchant (1940), proceeds as follows:

Under the applied pressure the flow across any plane, by Darcy's Law, is $Q/A = Ci$, where Q is the rate of discharge across area A , i is the hydraulic gradient, and C the coefficient of permeability. The hydraulic gradient for uni-dimensional flow is written $-\partial h/\partial x$, h being the pressure head, and is put equal to $\frac{1}{\gamma} \frac{\partial p}{\partial x}$ where p is the "intergranular pressure," and γ is a conversion constant. Thus the flow per unit area across an elementary plane is $\frac{C \partial p}{\gamma \partial x}$, and the differential flow across a layer of thickness δx is written $\frac{C \partial^2 p}{\gamma \partial x^2} \delta x$.

The rate of loss of water per unit volume in terms of the voids

ratio e (ratio of volume of voids to volume of solids) is $-\frac{1}{(1+e)} \frac{\partial e}{\partial t}$ and in this way is derived the fundamental equation

$$\frac{C}{\gamma} \frac{\partial^2 p}{\partial x^2} = -\frac{1}{(1+e)} \frac{\partial e}{\partial t} \quad (1)$$

This is not capable of solution as it stands, and the assumption is made that, for a small increment of load, the slope of the pressure-voids ratio curve is a constant a . The standard technique involves comparatively thick samples of clay, and, since large times would be required, equilibrium values of e are not measured, but it is argued that the value of a is but little affected by the time for which each increment of pressure is applied.

By substitution,

$$\left[\frac{C(1+e)}{a\gamma} \right] \frac{\partial^2 p}{\partial x^2} = \frac{\partial p}{\partial t} \quad (2)$$

The term in the bracket, the "coefficient of consolidation" c , being assumed constant, this is the familiar heat flow equation which can be solved by standard methods. The case of greatest interest is that outlined above, the determination of the relative movement of the top and bottom planes, originally distant H , of a stratum under pressure. The solution is required in terms of the "degree of consolidation" μ , the ratio of the change in overall dimension at time t to the total change at infinite time, whereas (2) gives an expression in terms of p , a pressure. The previous assumption that the slope of the p - e curve is constant must again be made so that

$$\mu = (e_t - e_0)/(e_\infty - e_0) = (p_t - p_0)/(p_\infty - p_0)$$

whereupon, by Fourier's Series,

$$\mu = 1 - \sum_{m=0}^{m=\infty} 2 \left[\frac{2}{(2m+1)\pi} \right]^2 e^{-\left[\frac{(2m+1)\pi}{2} \right]^2 \frac{C(1+e)t}{\gamma a H^2}} \quad (3)$$

Experimentally, it is found that (3) fails exactly to represent the data obtained (*cf.* Cooling and Skempton, 1940). In the majority of cases, the actual time taken to reach a given value of μ is greater than is indicated by (3). The deviation is well known and has been called the "secondary time effect" or "secondary compression." According to Taylor and Marchant, it "may be of the nature of a plastic adjustment of the clay structure wherein all or practically all the shearing stresses are eventually dissipated." Cooling and Skempton say that the mechanism is not completely understood.

The theory that part of the applied load is absorbed in plastic deformation in a manner somewhat similar to the creep of a metal

is of particular interest from the rheological point of view, as it appears to be opposed to the known properties of plastic clay under stress. Most materials deform more under a slowly applied stress than under sudden loading, but plastic clay is outstanding in that the reverse is the case, and the strain for a given stress increases with the rate of application of that stress. This, incidentally, is an important factor in the cracking of a clay article which can occur if the rate of drying is too great. Under the comparatively low rate of application of the stresses produced by the differential drying and shrinkage between the surface and interior, the surface layer will crack, although it may still be plastic to the more rapid stresses of handling and moulding. The conception of creep is therefore foreign to the nature of clay.

Explanation on the basis of plastic deformation is rendered even more difficult by the fact that the secondary compression may be negative, and equilibrium under load between porous pistons reached in less time than is indicated by theory. Now the above theory makes three assumptions at different stages of its development, (i) that the coefficient of permeability C is constant, irrespective of moisture-content or x , (ii) that the slope of the $p-e$ curve is constant, and (iii) that the combination of these two terms with a third variable $(1 + e)$ into the "coefficient of consolidation" is again constant. It is therefore relevant to enquire whether the secondary compression, which is found as the result of the failure of these assumptions, does in fact exist, for which purpose an entirely different approach may be made from first principles.

If a thin test-piece of plastic clay be subjected to a pressure P between porous pistons, it assumes equilibrium in a reasonable time (a few hours) and the equilibrium moisture-content M is related to the pressure by the expression

$$P = \alpha e^{-\beta M} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where α and β are constants for the batch of clay. The moisture-content M is expressed as the percentage on the basis of dry weight of clay and is thus directly equivalent to the voids ratio e . It is thus apparent that assumption (ii) above, that the slope of the $p-e$ or $P-M$ curve is constant, is of very limited validity.

The applied pressure may be used to control the moisture-content of a sample while water is passed through it, whereupon the permeability or "aqueous conductivity" C is found to be a similar exponential function of the moisture-content,

$$C = \theta e^{\gamma M} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

θ and ϕ being again constants. In assumption (i) above, C is assumed to be independent of M .

These experimental findings are subject to the following conditions :

(i) All moisture-contents are above the stage known as "leather-hard," where shrinkage ceases and the clay, on drying, changes colour. Change in moisture-content is therefore accompanied by a change in total volume equal to the volume of water lost. (At moisture-contents below leather-hard the total volume remains practically constant and any water removed by evaporation from the pores is replaced by air.)

(ii) All changes in moisture-content are negative, i.e. reductions. A large hysteresis exists, and (4) no longer holds for increases in moisture-content. (This and (i) are, of course, necessary in the previous approach.) (iii) Owing to the very large values of pressure required at the lower moisture-contents, (4) and (5) can only be directly established over a limited range. This range will, of course, cover all pressures likely to be encountered in consolidation problems, while the validity of (4) and (5) can be indirectly demonstrated down to the leather-hard condition (Macey, 1940). (iv) The form of (5) holds for all moisture-contents, but two sets of values of the constants θ and ϕ are necessary for each clay, applicable respectively above and below a certain, fairly high, moisture-content. This critical point is, of course, different for each clay, but is normally sufficiently high to prevent this complication entering into the present arguments.

The problem of the flow of water through a plastic clay, due either to evaporation at the surface or to a pressure applied, is analogous to that of heat in a non-uniform conductor with the added complication that the rate of flow is not proportional to the temperature (moisture-content), but is exponential therewith. The flow across an elementary plane of unit area is $C\partial P/\partial x$. Since the moisture-content is defined as a percentage on dry weight, we now have, as the basic differential equation :

$$\frac{\partial}{\partial x} \left[C \frac{\partial P}{\partial x} \right] = - \frac{\partial}{\partial t} \left[\frac{M}{S + M} \right] \quad \dots \quad (6)$$

where S is the volume of 100 grams of clay minerals in the plastic mixture, and $(S + M)$ is therefore the total volume of the plastic mix containing 100 grams of dry clay. The constant S , which can be evaluated by shrinkage measurements, normally has a value of about 40 c.c.

Substituting from (4) and (5) we have :

$$\frac{\partial}{\partial x} \left[e^{(\phi-\beta)M} \frac{\partial M}{\partial x} \right] = \frac{\partial}{\partial t} \left[\frac{M}{S+M} \right] \quad (7)$$

which contains no *a priori* assumptions, and makes due allowance for the variation of P and C with M .

Unfortunately, no strict mathematical way of handling (7) is known, and approximation has to be used. Experiment has shown, Fig. 1, that the constants ϕ and β tend to be equal, and, with this

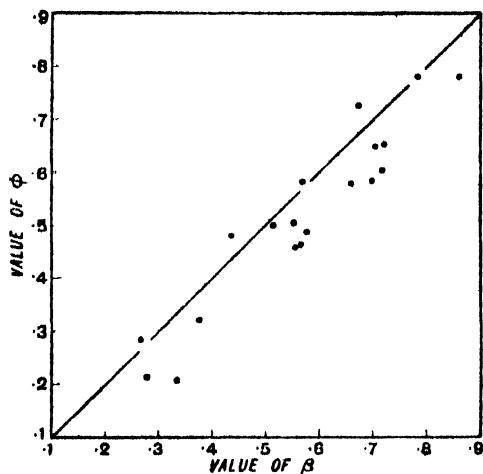


FIG. 1.—Illustrating approximate equality of the constants ϕ and β for different clays.

knowledge, (7) may be reduced to the heat-flow equation by writing :

$$\alpha\beta\theta e^{(\phi-\beta)M_0}(S+M_0)\frac{\partial^2 M}{\partial x^2} = \frac{\partial M}{\partial t} \quad (8)$$

This is equivalent to putting $\phi = \beta$, and assuming $C = \theta e^{\beta M}$, an erroneous value, so that the correction factor $e^{(\phi-\beta)M}$ is required. In other words, it being impossible mathematically to give C its true value at all moisture-contents, it is assumed to have the same variation as P (which by Fig. 1 is nearly true), and is then given its true value at the moisture-content M_0 . Obviously the accuracy of the result depends in large measure on the judicious choice of M_0 , which should be such that equal error is made in both directions. M_0 must therefore be the average moisture-content through which the clay has passed between its initial and final condition in the particular problem and at the particular time with which we are concerned. It will vary with time as the moisture-content of the system decreases. The initial moisture-content M_0 is introduced in

the expression $(S + M)$ as it is then that the dimensions of the system are defined.

Using (8), an equivalent form of (3) may be obtained as the solution of the problem in consolidation with which we are here concerned, but other methods are preferable. For simplicity we write $c = \alpha\beta\theta e^{(\beta-\theta)M_0}(S + M)$ and then eliminate it from (8) by putting $ct = \tau$, so that

$$\frac{\partial^2 M}{\partial x^2} = \frac{\partial M}{\partial \tau} \quad (9)$$

By the method of Hartree (1936), or by the use of the Laplace Transformation (Carslaw and Jaeger, 1947), a solution in terms of integrals of the complement of the error function may be obtained. The boundary conditions are equivalent to the well-known case of a finite slab of initially uniform temperature of which the faces are raised to and maintained at another temperature at $t = 0$. If the thickness of the clay layer is $2l$ and the initial moisture-content M_0 , the application of a pressure P produces a reduction of moisture-content at the top and bottom boundaries to a value $(M_0 - A)$ such that $P = \alpha e^{-\beta(M_0 - A)}$. Furthermore, $\partial M / \partial x = 0$ for all values of t at the mid-plane of the layer, $x = l$.

The value of the moisture-content M at any point distant x from the surface at a time t is given by:

$$M = M_0 - A \left[\chi_0 \left(\frac{x}{2\tau^{1/2}} \right) + \chi_0 \left(\frac{2l - x}{2\tau^{1/2}} \right) - \chi_0 \left(\frac{2l + x}{2\tau^{1/2}} \right) - \chi_0 \left(\frac{4l - x}{2\tau^{1/2}} \right) + \dots \right] \quad (10)$$

where χ_0 is the complement of the error function, defined as

$$\chi_0(w) = \frac{2}{\sqrt{\pi}} \int_w^\infty e^{-t^2} dt$$

Change of linear dimension is directly proportional to change of volume on a constant area, and therefore to moisture-content. The degree of consolidation μ is given by:

$$\mu = \frac{\text{Original volume} - \text{Volume at time } t}{\text{Original volume} - \text{Final volume}}$$

which reduces to

$$\mu = (M_0 - \int_0^l M \cdot dx) / A$$

By integration of (10) and substitution we have:

$$\mu = \frac{2\tau^{1/2}}{l} \left[\chi_1(0) - 2\chi_1 \left(\frac{2l}{2\tau^{1/2}} \right) + 2\chi_1 \left(\frac{4l}{2\tau^{1/2}} \right) - 2\chi_1 \left(\frac{6l}{2\tau^{1/2}} \right) + \dots \right] \quad (11)$$

where χ_1 is the first integral of the complement of the error function, defined as

$$\chi_1(\omega) = \int_{\omega}^{\infty} \chi_0(\xi) \alpha \xi$$

values of which are listed (Hartree, 1936, Carslaw and Jaeger, 1947).

This solution has several advantages over (3), notably in that the series is highly convergent, and therefore very easily evaluated, and in that it is a comparatively simple function of $l/2\tau^{\frac{1}{2}}$. Cooling and Skempton note that for small times, μ is proportional to $t^{\frac{1}{2}}$. This follows immediately from (11), holding until $\tau^{\frac{1}{2}} = 0.5l$ or μ is roughly 0.5, with an accuracy of about 0.5 per cent.

In (11) $\tau = ct = \alpha\beta\theta e^{(\phi-\beta)M_e}(S + M_0)t$ and M_e is the average value of the moisture-contents through which the whole system has passed, i.e. $M_e = \frac{1}{2}(M_0 + \int_0^l M dx)$. As already explained, it varies with the time, and therefore with the degree of consolidation. For some clays ϕ is greater than β , and the value of c decreases as the consolidation proceeds. The time taken for the clay to reach any given degree of consolidation is therefore greater than would be the case if c maintained its original value, and this appears as a deviation of the observed data from the calculated, the "secondary compression." In other clays β is greater than ϕ . These will reach a given degree of consolidation in less time than the theoretical and show a negative secondary compression.

In general terms, the constant β determines the rate at which the force P , to which the flow is proportional, changes with moisture-content. Similarly, the constant ϕ determines the rate of change of the permeability or aqueous conductivity C . The balance between the two determines how the rate of flow will vary with the moisture-content, $\partial M/\partial x$ being constant, and therefore how the experimental data for μ will vary from that calculated on the assumption that c is constant.

By evaluating (11) for various values of $w = l/2\tau^{\frac{1}{2}}$ it is possible by interpolation to obtain from the experimental data the value of c appropriate to any given change of moisture-content between the initial and final state considered. This has been done with a set of data on London Clay due to Cooling and Skempton, and the resulting figures, Table 1, show that the variation of c is appreciable. They may be compared with the value of 7.4×10^{-4} obtained by Cooling and Skempton from the same data and the portion of the curve linear with $t^{\frac{1}{2}}$. The degree of consolidation μ is, by definition, linear with the integrated moisture-content at the time t , and therefore with the average M_e . On the above theoretical arguments,

TABLE I
CALCULATION OF VALUES OF c FROM DATA BY COOLING AND SKEMPTON

Experimental Data.		Value of $c \times 10^{-4}$.
μ	μ	
0.509	5.10	7.56
0.542	5.53	7.29
0.580	6.01	7.13
0.628	6.52	7.13
0.667	7.06	6.97
0.738	8.03	6.81
0.796	9.02	6.61
0.847	10.06	6.40
0.883	11.06	6.15
0.901	11.72	6.25
0.964	14.32	5.91
0.969	15.03	5.62 (?)
0.975	16.05	5.24 (?)
0.982	17.02	5.15 (?)
0.985	18.04	4.80 (?)

$\log \bar{c}$ is linear with μ , and this is confirmed by Fig. 2. In this particular set of data, the experimental curve of μ against time is asymptotic, not to $\mu = 1$, as theory demands, but to a slightly smaller value. This unexplained divergence accounts for the lack of agreement of the points in Fig. 2 at very large values of μ .

It should be noted that it is impossible directly to calculate from (11) the degree of consolidation at any given time, since the appropriate value of c depends on M_v , which in turn depends on the

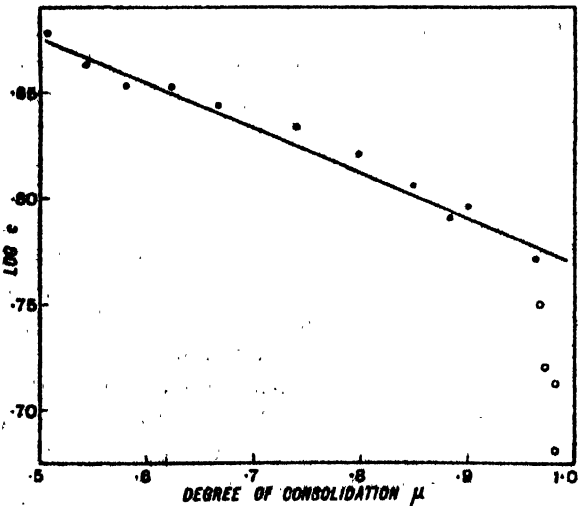


FIG. 2.—Relation between coefficient of consolidation c and degree of consolidation μ .

average moisture-content at that time, i.e. the degree of consolidation. This vicious circle may be avoided by working with tabulated values of (11) in terms of w , from which w may be interpolated for a given μ . Experimentally the variation of c with M_v may be established, when the appropriate value for a given degree of

consolidation is known, and the time required for that consolidation may be calculated. Since (11) is a function of $w = l/2\tau^{\frac{1}{2}}$, the time of consolidation for a thick stratum of clay may be predicted directly from experiment on a thin specimen of the same initial moisture-content and under the same applied pressure, M_e and c being common to both in this case.

In broader terms, the interesting feature of problems of water flow in plastic clay is the relationship (5) between the permeability C and the moisture-content, for which no completely satisfactory explanation has yet been found. With water obeying the normal fourth-power law of flow through capillaries, the calculated change in permeability with moisture-content is far less than that observed. An improvement in the situation is possible by stipulating a bound water layer on the clay surface, so reducing the effective radius of the capillaries. When, however, this bound layer is of sufficient thickness to account for the variation of the permeability over the experimental range of determination, the free water capillaries disappear, and the permeability becomes zero at a moisture-content which is still above the leather-hard, and when the vapour-pressure of the mixture is still practically equal to that of free water at the same temperature. Which is, of course, absurd. The author has suggested that, since the atomic lattices happen to be an exact fit, an ice structure forms on the silicate surface, and is slowly dissipated by thermal agitation as the distance from that surface increases. This appears to offer a possible explanation of the rough equality of the experimental constants ϕ and β , but the distances involved are large, while there is some evidence that the water at the silicate surface has a density greater, and not less, than that of free water. There can be little doubt but that some special relationship exists between clay and water, for no other substance will form a plastic mass in the same way, but its exact nature still remains to be elucidated.

The data of Cooling and Skempton used above were taken from large-scale reproductions of diagrams in their published paper, kindly supplied by them.

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RECENT ADVANCES IN SCIENCE

ASTRONOMY. By A. HUNTER, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

STELLAR ABSOLUTE MAGNITUDES.—The relation discovered by Hertzsprung and Russell in 1913 between the luminosities of the stars and their spectral types was necessarily based on scanty data, for few good parallaxes were then known for the derivation of accurate absolute magnitudes. The ensuing 35 years have, however, confirmed the general form of the relation, and it has become more and more firmly established that early-type stars with surface temperatures of $10,000\text{--}30,000^\circ\text{K.}$ are, with few exceptions (the "white dwarfs"), high-luminosity stars of absolute magnitudes averaging about -2 , whereas stars with lower surface temperatures may be either giants or dwarfs, the distinction in luminosity between the two classes becoming more marked as cooler and cooler stars are considered. The reddest stars of all, whilst all at a surface temperature of about 3000°K. , comprise both the common red dwarfs of absolute magnitude $+11$ and the much rarer though more conspicuous red giants some 10^4 times brighter. The Hertzsprung-Russell diagram shows these two branches: the giants, blue, white, yellow and red, all of about the same absolute magnitude, and the dwarfs or main-sequence stars decreasing steadily in luminosity towards later spectral types. On the basis of this diagram have been built all the modern theories of stellar evolution.

Recent work has, however, tended first of all to question the adequacy of the Hertzsprung-Russell diagram in representing the distribution of stellar luminosities by means of two isolated branches; and secondly, and even more seriously, to dispute its validity elsewhere than in that part of the Galaxy near the sun. The first objection is primarily an observational one. It has always been realised that selection can play havoc with theory if its effects on observation go unrecognised. In this case the trouble is that stellar luminosities depend in the last resort on measurements of distance, and trigonometric parallax measurements have in the

past been made chiefly on bright stars (because they were easy to observe) and on fast-moving stars (because they were likely to be close neighbours and therefore rewarding objects for the parallax observer). These criteria automatically pick out the giants and the dwarfs respectively. It has always been known that such observational selection was present, but its extent and its far-reaching consequences to theory have perhaps not always been realised to the full. It is in fact uncertain to what extent the absence from the Hertzsprung-Russell diagram of late-type stars intermediate between giants and dwarfs is to be attributed to their real infrequency of occurrence in space rather than to the fact that such stars are excluded from parallax observing lists by a selective process—a process no doubt imposed on observers by the exigencies of their work, but nevertheless an arbitrary one which can lead to misleading results. Martin (*The Observatory*, 66, 82, 1945) has suggested that amongst K0 stars the gap between giants and dwarfs may not be as wide nor as empty as was at one time believed, and Jackson (*Monthly Notices of the Royal Astronomical Society*, 106, 245, 1947) has followed this up by expressing similar doubts about the much wider gap amongst M stars.

But it is not contended that any filling of these gaps will be more than partial. The total elimination of observational selection would scarcely alter the character of the Hertzsprung-Russell diagram fundamentally. It is another matter, though, when we turn to the relation between stellar luminosity and colour in systems other than our own Galaxy. It was a natural working assumption that the Hertzsprung-Russell diagram would represent equally well the distribution of luminosity in (say) the extra-galactic nebulae. The first doubts, set in train when the globular clusters were examined, were strengthened by the discovery of the star clusters in Sculptor and Fornax with equally peculiar absolute magnitude distributions, and were confirmed by Baade's resolution of M32 and NGC 205, the elliptical companion galaxies to the Andromeda spiral. It was the circumstances attending this observational feat (*SCIENCE PROGRESS*, 35, 269, 1947) that led Baade to distinguish between stellar populations of type I and those of type II. The former, it will be remembered, are characterised by the presence of blue supergiants and open clusters (as in the solar neighbourhood); the latter by the presence of short-period Cepheids, which are rare in type I, and by the fact that their brightest stars are red giants. In type II the number of stars in a small magnitude interval remains roughly the same to a definite upper limit of brightness (that of the red giants) and then drops precipitately. The Hertzsprung-

Russell diagrams are thus quite different for the two populations. Nor is it yet certain that the future will not bring populations III, IV, etc., to light. Meanwhile the mere existence of type II is a standing challenge to the theorists, whose accounts of stellar evolution have hitherto been constructed so as to satisfy the observational requirements summarised in the Hertzsprung-Russell diagram for type I alone. The first attempt to take up this challenge comes, appropriately enough, from the veteran astrophysicist H. N. Russell himself (*Publ. Astr. Soc. Pacific*, **60**, 202, 1948).

Russell points out that the brightest stars in the Galaxy, blue supergiants such as α Cygni, are radiating energy at a rate high enough to exhaust all their known internal sources of nuclear energy in a time short compared with the 2×10^9 years which so many lines of approach indicate as the age of the universe. If such stars do indeed live on the principle of a short life and a merry one, those which we observe at present must be young relative to the stars as a whole. It has several times been remarked, for example, that supergiants tend to be associated with regions of obscuration by diffuse material, an observation which would lend support to the idea that these stars are recent condensations from interstellar clouds whose remnants are still visible in their immediate neighbourhood. Conversely, populations of type II, which are noteworthy in not comprising supergiants, also lack obscuring clouds. Russell therefore suggests that if, in the various stellar systems accessible to observation, there still exists, in quantities that differ greatly from system to system, pre-stellar matter from which stars may yet in favourable circumstances condense; and if the condensation process is capable of producing stars of large mass as well as small; then at least two kinds of population may be expected *a priori* to result.

A system in which large quantities of diffuse material still remain unused will now contain supergiants. One in which the present amount of interstellar matter is negligible may have contained supergiants initially, but these will have burned themselves out long ago. At any given time after an assemblage had its origin it will contain stars of small mass and low luminosity destined to continue their pedestrian course far into the future, stars of medium mass and luminosity and rather more spendthrift habit ready to radiate for some little time yet, and stars of a limiting mass and luminosity on the point of using up their last resources of nuclear energy before bringing their radiative careers to an end. Beyond this upper limit of luminosity, nothing. The actual frequency of different luminosities for masses below the limiting value would

doubtless depend on the initial distribution of mass and the particular evolutionary course followed by the stars of various luminosities; the latter being controlled, no doubt, by the chemical constitution and physical characteristics of the matter in the deep interiors of the stars. But the general resemblance of the state of affairs pictured by Russell to that actually observed is independent of any particular hypothesis concerning stellar evolution. Stellar systems containing diffuse matter currently giving birth to stars will be of type I; populations with little or no content of pre-stellar matter will be of type II, with a limiting luminosity depending on the time which has elapsed since the assemblage ceased to be capable of creating new stars. Where (as in the middle regions of our own Galaxy) mixtures of populations I and II occur, the pre-stellar material was doubtless limited in amount or non-uniformly distributed.

It remains to be seen whether the other features of type II systems—the redness of the brightest stars, the frequency of cluster-type variables, etc.—are necessary concomitants to the conspicuous lack of supergiants which this suggestion of Russell's so neatly explains; and if so, whether his idea can be amplified to account for them also. It may be, too, that extensions of his hypothesis in the future will explain, for example, how the stars of population I in the solar neighbourhood came to have such a relatively high galactic rotation. The suggestion is bound in any case to stimulate the efforts of those theoretical astrophysicists whose work has been directed to investigating in detail the conditions in which interstellar matter can condense to form stars.

THE SPECTRA OF CLOSE BINARIES.—A large part of the spectrographic work of one of the biggest telescopes in the world has consisted during the past decade of detailed examination of those spectroscopic binaries which are also eclipsing variables, i.e. close double stars whose orbits lie in the line of sight. The separate investigations have never been of a character suitable for reporting in these pages, but latterly a pattern has been emerging from the work as a whole. The award of the 1948 Bruce Gold Medal of the Astronomical Society of the Pacific to Dr. Otto Struve, till lately director of the Yerkes and McDonald Observatories and the astronomer responsible for these researches, has given the medallist an opportunity of summarising (*Publ. Astr. Soc. Pacific*, 60, 160, 1948) the work and interpreting in general terms the results achieved.

To appreciate fully the purpose of Struve's investigations it will be necessary to go back into astronomical history a little. Almost exactly 300 years ago the first visual binary star, Mizar,

was discovered with the newly-invented telescope. Not till 150 years later did the elder Herschel announce that many such double stars were physically and not merely optically connected. That double-star catalogues now contain well over 20,000 pairs does not mean that telescopes can show the real extent of duplicity amongst the stars: only those binaries that are bright enough, that are close enough to the sun, and that have components far enough separated to appear double in the telescope will reach the catalogues at all. Sixty years ago, the spectroscope was added to the telescope as a means of detecting double stars. The first binary detected by the variable duplicity of the absorption lines in its spectrum was, strangely enough, one component of Mizar. Rapid extension of the observations to other stars showed that many of them were binaries far too close to be separated telescopically, but comprising components moving rapidly enough round their common centre of gravity to show, if not always lines which periodically become double as the components reach their maximum line-of-sight velocity, then at least lines due to the brighter component which oscillate in position as that star revolves in its orbit. Examination of the curves connecting line-of-sight velocity with time soon showed examples of elliptical orbits of every conceivable variety, both as regards ellipticity and size and as regards inclination and orientation to the line of sight.

The next step, when a sufficient number of these "spectrographic orbits" had accumulated, was to collect them and search systematically for any suggestive regularities. There was the simple sine curve characteristic of a circular orbit, the unsymmetrical saw-tooth curve corresponding to an elliptical orbit with the major axis in the line of sight, the sharply peaked curve shown by an elliptical orbit with its major axis across the line of sight, and all manner of curves intermediate between these extremes. It was as long ago as 1908 that Barr pointed out that in the 30 or so spectroscopic binaries for which orbits were then known, there seemed to be a tendency for the point of periastron to lie at that point of the orbit most distant from the earth. Now, since Copernicus, astronomers have been properly wary of any conclusion which smacks of the geocentric. There might conceivably be some physical regularity in the orientations of the major axis of binary orbits (perhaps along or across the direction to the galactic centre), but that such a regularity should be related to the position of the solar system would be altogether too much to swallow. So for 40 years desultory disputes have continued on the validity of Barr's conclusions. As recently as 1935 Aitken decided that amongst

the longitudes of periastron ω of the 120 low-eccentricity orbits then available to him no fewer than 40 per cent. occurred in the quadrant 0° – 90° , whereas each of the other three quadrants had to be content with about 20 per cent. Though there can of course be no certainty that this inequality of distribution is real (and Aitken counselled caution in accepting it), this excess is far larger than should occur from sampling error in a random distribution.

In these circumstances only more observations could be expected to clear up the tangle, and Struve and his collaborators, working largely with the 82-inch reflecting telescope of the McDonald Observatory in Texas, have in the past decade investigated the orbits of about 100 spectroscopic binary systems, giving preference to those already known to be eclipsing variables (i.e. to have orbits of inclination 90° to the "plane of the sky"). Perhaps the most striking result of the work is a remarkable confirmation of Aitken's conclusion. The observations show a large preponderance of unsymmetrical velocity curves with narrow pointed maxima and steeper descending branches than ascending branches. These curves are typical of orbits with $0^\circ < \omega < 90^\circ$. Here is Barr's quandary again, then, refined somewhat and posed the more imperatively now because of the weight of data behind the observed asymmetry in the distribution of ω .

Anthropocentric astronomy is no less unpopular now than it was in 1908, however, and Struve examines several possible loopholes which provide some hope of escape from the dilemma. Can it be, for example, that lack of balance in the terrestrial distribution of observatories—lack of observations of the southern Milky Way, perhaps—will transform a real asymmetry based on some preferred direction in the Galaxy into an observed asymmetry apparently depending on the line of sight? No, says Struve: his latest observations do not support such an explanation. Nor can the accidental inclusion of pulsating stars such as Cepheids, under the mistaken impression that the periodic line displacements in their spectra are due to orbital motion, contribute largely to the effect. Rotational distortion and the so-called reflection effect, which increases the brightness of those hemispheres of each component which radiate to each other across the common focus, can both contribute to the peculiar distribution of ω , but to an extent which is quite inadequate for a quantitative explanation.

One significant observation, however, holds the key to the mystery. The star U Cephei is a spectroscopic binary and also an eclipsing variable, and it is possible to check the spectrographic orbit against the photometric observations. The spectroscopic

evidence indicates an unsymmetrical velocity curve with shallow, broad minima, peaked maxima and descending branches steeper than ascending ones. All this points to an orbit of appreciable eccentricity ($e \sim 0.2$) and small periastron angle ($\omega \sim 40^\circ$). The photometric observations, however, give a period of 60 hours with equally spaced (but not equally deep) eclipses 30 hours apart. But symmetrical eclipses indicate either a circular orbit or one in which the line of apses coincides with the line of sight, i.e. $\omega = 90^\circ$ or 270° . Neither of these is consistent with the spectrographic results, to satisfy which the secondary eclipse would have to occur 12 hours later than it actually does.

Now when, as in this case, it comes to direct conflict between photometric and spectroscopic data, it will always be the latter which are thrown overboard. The brightness estimates are inferences from primary sensory impressions; they are direct observations in a sense in which the velocity observations are not. The spectrographic orbit is deduced from a velocity curve showing asymmetry of a particular kind. This curve is itself derived from periodic displacements of spectrum lines: shifts that might be otherwise interpreted—*have* indeed been otherwise interpreted in the parallel case of Cepheid pulsation, where the geometry of the problem demanded an explanation different from the standard one of orbital motion. No resort to pulsation is necessary (or indeed possible) in this case, however. As Struve shows, it is sufficient to drop the hypothesis that the observed velocities are entirely those of the binary components executing their orbital motions, and to suppose instead that some of the absorption occurs in streams of gas divorced from the stellar components of the binary and moving independently of them. Once it is admitted that the velocity curves of close binaries may be seriously distorted by extraneous absorption, the explanation of the particular statistical asymmetry found in the distribution of ω becomes a matter for the exercise of sufficient ingenuity. Struve first of all satisfies the case of U Cephei. The photometric data demand a real velocity curve that is symmetrical, with the radial velocity zero at the times of principal and secondary eclipse. If the observed curve is corrected so as to comply with these conditions, there remains a large excess of recessive motion before the principal eclipse begins, and a large excess of motion of approach after totality. Struve explains this by postulating that we are seeing, projected on to the primary star itself, a mass of gas enveloping the whole binary system and rotating in the same direction as, but faster than, its components move. Shortly before mid-eclipse the receding stream is seen in the foreground

of the primary and its redward-shifted absorption lines blend with those of the primary to give an excessive velocity of recession. After mid-eclipse the secondary occults this half of the stream, and the approaching half is seen projected on to the primary, producing a spurious shift to the violet in the combined spectrum. It is to be regarded as important confirmation of this hypothesis that the profiles of the lines during the critical phases are in fact unsymmetrical in just the way that would be expected if sharp low-pressure lines were superposed on the Stark-broadened lines due to the primary star itself.

Were this suggestion of a sort of whirlpool of gas surrounding the components of U Cephei purely an *ad hoc* one it would be interesting but perhaps no more than that. But when it is found to account, qualitatively as it stands, and even quantitatively with relatively minor modifications, for such puzzling objects as SX Cassiopeiae, RW Tauri and many others, it begins to assume general importance. In the case of RW Tauri, there is an interesting further confirmation of the physical reality of the gas streams. When the total light of this binary is reduced during the partial phases of the eclipse, *emission* lines are observed flanking the absorption lines. As the eclipsing star gradually obscures the approaching stream the violet component is quenched; after mid-eclipse the receding stream is obscured, the red component disappears and the violet one reappears.

In all these binaries the effect of the gaseous whirlpools is, as required, to produce spuriously high eccentricities and a spatial distribution of periastra with a fictitious asymmetry which appears to orientate the major axes nearly along the line of sight. No difficulty arises about the amount of matter needed for the streams: the observed absorptions call for a density so low that no dynamical perturbations of the orbital motion would be expected. (In any case, the whirlpools are in rotation in the same sense as the components themselves, so they would not act as the classical "resisting medium.") The origin of the streams is still a matter of speculation. Whether they are debris from the initial catastrophe which caused the birth of the binary, or whether they are coronal in nature and derive from the component stars themselves, perhaps by tidal or prominence action, are questions that will not concern the observer. He will breathe a sigh of relief at being able to write off at least one of the outstanding puzzles of astrophysics.

His relief is, however, destined to be short-lived. It is pleasant to be able to account satisfactorily for discrepancies between spectrographic and photometric orbits in those close binaries whose

orbits happen to lie in or near to the line of sight, and thus lend themselves to a photometric check. It is less pleasant to realise that the type of distortion produced by these gaseous streams exactly simulates certain perfectly normal orbital features which, though not in fact present, might well be present. If we cannot trust the velocity curve for a spectroscopic binary when the light curve is there to check it against, can we trust it in the many more cases where no such help is available? So far we have no idea how many non-eclipsing spectroscopic binaries may have quite fictitious spectrographic orbits; and even if further work on eclipsing variables gives us in the future an idea of the proportion affected, it will only be a statistical idea—we shall still not know whether or not to suspect any particular orbit.

"The camera cannot lie." No, but in astronomy the indications it affords have to be looked at very, very carefully if the unwary observer is not to be deceived.

PHYSICS. By F. A. VICK, O.B.E., Ph.D., F.Inst.P., The University, Manchester.

ULTRASONICS.—1.—Ultrasonics (or supersonics) is concerned with the generation and propagation through solids, liquids and gases of vibrations having frequencies higher than the audible limit, that is frequencies above 20 kilocycles and extending to hundreds of megacycles per second. Such vibrations have been studied for many years, but their importance and interest have greatly increased recently, following more efficient methods of generation, particularly at the higher frequencies. It has been shown that the classical theory of acoustics is unable to account quantitatively for the propagation of ultrasonic waves. The waves themselves are found to have numerous uses in various branches of science and technology.

2.—Earlier work is summarised in the standard textbooks (*e.g.* L. Bergmann, *Ultrasonics and their Scientific and Technical Applications*, Bell, 1938; E. G. Richardson, *Sound*, 4th ed., Arnold, 1947, Chap. X), and the Physical Society Progress Reports in Physics. On the classical theory, the absorption coefficient for any waves, sonic or ultrasonic, passing through fluids is made up of two terms, one related to viscosity and the other to thermal conductivity. The first, calculated by Stokes, is

$$\alpha_1 = \frac{8\pi^2\nu^2\eta}{3v^3\rho} \quad (1)$$

where α_1 is the coefficient of absorption (of particle velocity), ν the

frequency, v the velocity, and ρ the density of the medium. For the second process, the expression due to Kirchhoff is

$$\alpha_2 = \frac{2\pi^2 v^2 (\gamma - 1) K}{v^3 \rho C_p} \quad (2)$$

where γ is the ratio of specific heats and C_p the specific heat at constant pressure for the medium, and K its thermal conductivity. For gases the absorption is found to be many times that given by $\alpha_1 + \alpha_2$. An explanation has been found (as originally suggested by Jeans) in the relatively slow transfer of energy between the translational and internal degrees of freedom of molecules subject to high frequency vibrations. Detailed theory (Kneser, *Ann. der. Physik*, **32**, 277, 1938) shows that the total absorption coefficient first varies as v^2 (as formulæ 1 and 2 indicate), but then rises rapidly to a maximum as the period of the ultrasonic vibration becomes equal to the relaxation time of the energy interchange between the degrees of freedom. Strong dispersion of velocity also occurs in this region. At still higher frequencies, the theory predicts a very low absorption. For gases, the theory is quite well confirmed experimentally.

3.—A similar explanation has been attempted for liquids, where the absorption coefficient may be hundreds of times greater than given by expressions 1 and 2. In liquids, the closeness of the molecules would lead one to expect a much shorter relaxation time than for gases, and therefore maximum absorption at higher frequencies, with little departure from the classical values at lower frequencies. While for some pure liquids, α was found experimentally to vary as v^2 as predicted, up to frequencies of the order of tens of megacycles, a number of anomalies appeared. For most liquids, α_2 should be much smaller than α_1 , and is neglected, but equation 1 does not appear to be obeyed even at frequencies well below the region where the relaxation process should be important. For example, R. Lucas (*J. Phys. Rad.*, **8**, 41, 1937) made measurements with pure benzene and hexane and mixtures of the two in different proportions. He found the absorptions in the mixture to be up to ten times those in the pure liquids, but the viscosity had changed by only a small fraction of this amount. Lucas suggested there might be some form of "sonic turbidity" due to fluctuations of density and the grouping of molecules. Following up this work, J. Quinn (*J. Acoust. Soc. America*, **18**, 185, July 1946) set out to test another suggestion, attributed to Debye, associating the anomalies with delays (relaxation times) in the molecular rearrangements in the liquid during the passage of the supersonic waves. These delays might be expected to be dependent on the state of order

or disorder of the liquid molecules. Quinn therefore made measurements with pure benzene near its freezing-point, and at higher temperatures where the order should be less. The experimental method was that commonly used, in which the ultrasonic waves are generated by a quartz crystal in the liquid and connected to a suitable oscillator. The plane waves generated travel through the liquid to a plane metal reflector parallel to the quartz face. The current through the quartz crystal is affected by the energy received from the liquid after reflection by the metal plate. A plot of current against distance from quartz to reflector shows (in the ideal case) sharp minima when the distance is a multiple of $\lambda/2$. Successive minima are less and less deep, by amounts depending on the absorption coefficient, α , in the liquid. It was found that for pure benzene α was not very temperature dependent, it being sufficiently accurate to hold each temperature to 0.1°C. during measurement. Very few crystals vibrate in a single mode and uncertainties in the current-distance curves lead to errors of the order of 25 per cent. in α . It was found that the mean values of α increased by about 30 per cent. as the temperature was increased from 7.2°C. to 73.5°C. , contrary to what would be expected if the orderliness of the liquid molecules is important. The actual value of α/v^2 was about 0.8×10^{-14} , whereas equation 1 predicts 0.9×10^{-16} . Between 7.2° and 73.5°C. , η/ρ decreases by 50 per cent., but α increases. It thus appears that η is not a predominant factor, and there is an unknown mechanism of absorption in addition to that leading to equation 1.

4.—C. E. Teeter (*J. Acoust. Soc. America*, 18, 488, Oct. 1946) has recorded preliminary measurements at 10 and 15 megacycles per sec. for various liquids. He found that the absorption coefficients were up to 1000 times as large as predicted by Stokes' formula, (1), and that in some cases the temperature coefficient was high.

5.—Another method of measuring ultrasonic velocities and absorption coefficients in liquids is provided by the well-known diffraction of light passing through a liquid at right angles to the direction of propagation of the train of compressional waves. The diffraction angles, θ , are given by

$$\sin \theta = \frac{n\lambda}{\lambda_s}$$

where n is a positive integer, λ the wavelength of the incident light in the liquid, λ_s the wavelength of the ultrasonic waves. A recent example of the use of such diffraction is described in a paper by C. J. Burton (*J. Acoust. Soc. America*, 20, 186, March 1948), who studied velocities and absorptions in binary mixtures of water and

alcohols, glycols, and glycol ether in the range 5–25 megacycles per sec. A constant deviation spectroscope selected a wavelength of light which passed in a parallel beam through a glass cell containing the liquid. The diffracted light was focussed on to a photographic plate. An x-cut quartz crystal having a fundamental frequency of 5 megacycles per sec. generated the ultrasonic waves, which were absorbed at the opposite end of the cell by several layers of fine-meshed stainless steel gauze to prevent the formation of standing waves. The ultrasonic velocities were determined by measuring the spacing of the various diffraction orders to obtain λ_s . No dispersion was found in the region 5–25 megacycles per sec. Absorption was measured by the intensity of transmitted light in a given diffraction order (e.g. the first) after passing through the cell at different distances from the quartz crystal, using a multiplier-photocell in place of the photographic plate. For small values of sound intensity, the intensity of the first-order diffracted light is proportional to the intensity of the ultrasonic waves causing the diffraction. It was verified that the intensity of ultrasonic waves fall off exponentially with distance from the quartz crystal. The ultrasonic intensities used were 0.006 watt/sq. cm. or less. The results showed peaks of absorption, higher than for the pure liquids, at intermediate concentrations of acetone, ethanol, propanol, isopropanol, tertiary butanol, monoethyl and monobutyl glycol ethers in water. No absorption peaks were found in mixtures of methanol, glycol, monomethyl glycol ether or dioxane with water. Velocity peaks at intermediate concentrations were found with all the mixtures listed above. The observed absorptions were in all cases greater than predicted by the existing theories.

6.—Before a complete explanation can be expected for mixed liquids, it is reasonable to suppose that one will be necessary for pure liquids. As has been mentioned above, the relatively slow interchange of energy between different degrees of molecular freedom accounts satisfactorily for the higher absorption of ultrasonic waves in gases but not in liquids. Further evidence of this is provided by the work of F. E. Fox and G. D. Rock (*Phys. Rev.*, 70, 68, 1946), who measured the absorption coefficient for water over a range of temperatures. They found that the coefficient was still large at 4° C., where the specific heats at constant volume and constant pressure are equal, the ultrasonic propagation isothermal, and thus the relaxation phenomenon between degrees of freedom not applicable. Possible mechanisms for absorption in water are discussed by L. Hall (*Phys. Rev.*, 73, 775, April 1948). He examines in particular a structural mechanism; the rearrangement of molecules during

compression, involving the breaking of molecular bonds and other energy interchanges which occur at a rate finite compared with the period of ultrasonic vibration. Two states of configuration are assumed for each molecule: State 1 characterised by larger volume and lower energy and State 2 by smaller volume and higher energy. Compression is associated with the increase in number of molecules in State 2. State 1 is identified with an "ice-like" structure and State 2 with a close-packed liquid structure. The theory of reaction rates is applied, and an expression for the relaxation time between the two states obtained as

$$\tau = \frac{h}{kT} \cdot \frac{e^{\Delta F_1/RT}}{(1 + e^{\Delta F/RT})}$$

where h is Planck's constant, k Boltzmann's constant, T the absolute temperature, ΔF_1 the activation energy per gm. mol. required for the transition State 1 to State 2, ΔF the difference in free energy per gm. mol. between States 1 and 2. For water at normal temperatures, τ is of the order of 10^{-12} sec., and ultrasonic absorptions calculated from it agree with the experimental values to within about 10 per cent. It therefore seems very probable that structural relaxation processes are important in ultrasonic absorption in liquids like water.

7.—The development of radar techniques during the war has made possible the use of short pulses to measure velocity and absorption of ultrasonic waves in liquids. An interesting example is the work on liquid helium by J. R. Pellam and C. F. Squire (*Phys. Rev.*, **72**, 1245, Dec. 1947). As in the work described in paragraph 3, a quartz crystal generator and a plane metal reflector are mounted parallel to each other in the liquid so that their distance apart may be varied. In this case, however, electrical pulses of short duration are converted by the quartz crystal to high frequency sound pulses which are reflected and re-excite the crystal to give a piezo-electric E.M.F. after a time dependent on the ultrasonic velocity and distance between crystal and reflector. The delay is measured by the use of a cathode ray oscillograph just as in the determination of range in radar. The amplitude of the reflected pulse gives a measure of the attenuation and therefore of the absorption. In practice the size of the transmitted pulse is increased by a calibrated attenuator to bring back the reflected pulse amplitude to a standard value, so that the attenuation may be read off directly. Since there is practically no dispersion in liquid helium at the frequencies used, the pulse method gives the same results as the continuous wave method. The wavelength of the ultrasonic disturbance in liquid

helium at about 15 megacycles per sec. is about 10^{-3} cm., so with crystal and reflector about 1 cm. in diameter, the pulses travel out in a very narrow beam. Measurements were made over the temperature range 1.57 – 4.5° K. For liquid He I, i.e. above the λ -point, the observed absorptions are accounted for completely by the classical theory as expressed in equations 1 and 2 above. This presumably means that for simple monatomic liquids like helium there are no relaxation phenomena of the two kinds already discussed. As the temperature approaches 3° K. the experimental values of α rise appreciably above those predicted by the classical theory, and at the λ -point itself there is a sharp peak in the α – T curve. The absorption falls rapidly as the temperature is reduced below the λ -point, and then begins to rise again at the very low temperatures. The interpretation of the results in this neighbourhood is naturally complex. No appreciable discontinuity in velocity was observed at the λ -point.

8.—An interesting application of the pulse method outlined in paragraph 7 is in the construction of acoustic delay lines, used in radar and calculating machines. The two uses are examples of measuring short time intervals and storing information. They are made possible by the sharp pulses which can be sent through liquids at high ultrasonic frequencies. If the velocity through the liquid at a definite temperature is known, then the distance between quartz crystal and reflector gives a measure of the time interval between the generation of the pulse in the liquid and its arrival back again at the quartz after reflection. Several papers have appeared recently describing practical applications of this principle (T. K. Sharples, *Electronics*, p. 134, Nov. 1947; N. B. Huntingdon, A. G. Emslie and V. W. Hughes, *J. Franklin Inst.*, **245**, 1, Jan. 1948; A. G. Emslie, H. B. Huntingdon, H. Shapiro and A. E. Benfield, *J. Franklin Inst.*, **245**, 101, Feb. 1948; M. V. Wilkes and W. Renwick, *Electronic Engineering*, **20**, 208, July 1948). The last paper describes the "memory unit" for EDSAC, the Electronic Delay Storage Automatic Calculator under construction in the University Mathematical Laboratory, Cambridge. As in most electronic calculating machines, a scale of two is used, in which the presence of a pulse indicates a "1" and the absence a "0." The series of pulses required to be "remembered" for a subsequent operation is passed into a quartz crystal and converted into a corresponding series of ultrasonic pulses in a liquid (mercury in this case). After a time longer than the pulse train they appear at a crystal at the other end of the tube, where they are converted back into electrical pulses. These are amplified and pass into an electrical gate, which sends

a pulse back to the input of the delay line for every pulse received at the gate. This device avoids cumulative distortion of the pulse wave form which would occur if the same pulses were kept in circulation through the delay tube. Each pulse lasts 0.9 microsec. and the interval between pulses is 1.0 microsec.. Thirty-four binary digits are provided for each number, so that with a pulse for the sign of the number and a space at the end 36 pulses (or pulse spaces) are required for each number. Very interesting technical problems have to be solved in designing and constructing the tubes and associated circuits. These are described in the papers.

9.—Though most delay lines are liquid, solid lines have been used. An investigation of these is described by D. L. Arenberg (*J. Acoust. Soc. America*, 20, 1, Jan. 1948). The paper contains a useful discussion of the theory involved. Fused quartz was the best material, and for delays longer than 300 microsec. multiple reflection paths in two and three dimensions in the quartz have been employed.

10.—It is a fairly obvious extension of the principles described above to use ultrasonic pulses in solids for "echo sounding," to determine thicknesses and to detect flaws such as cracks which reflect the pulses. Recent papers include F. L. Firestone and J. R. Frederick (*J. Acoust. Soc. America*, 18, 200, July 1946), W. S. Erwin and G. M. Rasswieler (*Rev. Sci. Instr.*, 18, 750, Oct. 1947), N. G. Branson (*Electronics*, p. 88, Jan. 1948).

CRYSTAL COUNTERS.—In the previous article in this series (*SCIENCE PROGRESS*, 36, 471, July 1948), it was stated that further work was needed to test the view expressed by Hofstadter that Type I diamonds have a mosaic structure. More recently Dr. Kathleen Lonsdale has published a letter (*Phys. Rev.*, 73, 1467, June 1948) stating that Type I diamonds are neither mosaic nor laminated, but are nearly perfect crystals. Good Type II diamonds do appear to be laminated, and Dr. Lonsdale suggests that the electrons counted move along the boundaries and not through the crystals.

GENERAL AND PHYSICAL CHEMISTRY. By J. W. SMITH, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic.

SWELLING AND SHRINKING.—No branch of chemistry has undergone more profound developments during recent years than colloid chemistry. This has been due partially to the application of methods such as those of X-ray and electron diffraction and the use of the electron microscope for its study. Such means of investigation have brought it from the stage of being a mass of empirical facts, for which at best only a qualitative explanation could be given, to, in certain phases, at least, something approaching

an exact science. The interest in this branch of science has received an additional impetus through the ever-increasing applications of plastics, many of which, in common with natural rubber, have the character of yielding typical gel systems.

The most characteristic feature of gel structures is their swelling in suitable dispersion media and their shrinkage when this medium is removed from them again, and, therefore, through study of this behaviour it might be expected that considerable light would be cast on the nature of such gel-forming materials. Early studies in this direction were made principally upon materials, particularly gelatin, which swell in water, together with a few isolated examples of swelling in non-aqueous solvents, especially of rubber in benzene and other liquids. For swelling in aqueous media it was known that Donnan's theory of membrane equilibria held for the distribution of ions between the gel and the liquid phase, whilst for all cases the swelling process was attended by a very considerable swelling pressure. The phenomena, however, cannot be explained by mere capillary action or on the simple theory of osmosis, and it is only through the investigations of the last few years that a picture is beginning to be built up of the series of processes which occur when a gel swells or shrinks. The more recent aspects of the problem formed the subject for a General Discussion held by the Faraday Society in September 1946 (*Trans. Faraday Soc.*, 1946, **46**, B, 1-304). This review is based principally upon some of the topics discussed at that meeting and references quoted by page number only are drawn from the above-cited publication.

As was pointed out by Bernal (p. 2) in his introductory address the definition of a gel is very loose. It has been taken to include any fluid-containing system with much rigidity, but of structure which cannot be elucidated by the optical microscope. The electron microscope and X-ray methods, however, reveal evidence of a definite structure in most cases. This structure is often of a crystalline type, but sometimes is confused and irregular, suggesting that in certain cases the particles are arranged in an orderly manner parallel to one another, whilst in other cases there is a certain degree of randomness.

Marsden (p. 29) lays down three criteria which such a system should satisfy in order that swelling can occur. Firstly, there must be large units which can separate from each other while remaining definite entities themselves not undergoing any breakdown. These units may be aggregates of small molecules as in soaps, large molecules, linear as in thermoplastic polymers or

laminar as in graphitic-type compounds, or even small crystals. Secondly, agencies must exist causing localised cohesion between the units, and thirdly the solvent must be able to release all the other forces which hold the large units together.

These requirements are met by a wide range of substances, ranging from synthetic polymers and regenerated cellulose to natural products such as wood and other forms of cellulose, starch grains, rubber, gums, and proteins of various types and in different forms. It is evidently impossible to expect one or two simple laws to express the behaviour of such diverse systems, but many close analogies can be observed between the behaviours of systems which appear to be very different from one another, and the general picture of the processes occurring during swelling and shrinking is being gradually evolved.

Owing to the fact that the swelling of a gel to produce a very viscous concentrated solution proceeds more rapidly under stagnant conditions than the subsequent dilution of the swollen mass by diffusion, and that a visible boundary is observed between the solid and the swollen mass and also between the latter and the dilute solution, this swelling process has frequently been regarded as a distinct phenomenon. Hartley has pointed out, however (p. 6), that the whole process can occur only through the local thermal changes of molecular positions which constitute diffusion, and therefore the process should be capable of description in terms of the basic diffusion equation.

Rate of diffusion of one component across a section = $-D \cdot \partial c / \partial x$ where D is the diffusion coefficient of the component and $\partial c / \partial x$ indicates its concentration gradient in the direction of diffusion. The existence of the visible boundaries, owing to relatively abrupt changes of refractive index, between the solid and viscous swollen mass, and between the latter and the dilute solution, indicates that the diffusion coefficient is much greater for intermediate concentrations (i.e. in the swollen mass) than in the dilute solution inside or in the unswollen material. Also, owing to the great discrepancy in the intrinsic mobilities of the two species, the transfer of polymer in the swollen region occurs largely by real movement of the whole solution, which can be demonstrated by the movement of foreign particles in the swollen mass.

This state of affairs leads to the circumstance that, if the swelling is restricted to one direction only, the polymer will undergo a stretching, and therefore the polymer molecules would be expected to be partly oriented and extended in the direction of diffusion. Such an effect has been observed by Robinson (p. 12).

In connection with this phenomenon, Hermans pointed out (p. 29) that a very sharp boundary line results in some cases when reaction occurs between the diffusing substance and the medium into which it is diffusing, and always results when the diffusing molecules are precipitated from the medium and hence withdrawn from the diffusion process, as in the case of copper ions diffusing into a cellulose xanthate gel or of sodium thiosulphate diffusing into an iodine-containing gel.

The behaviour of the type discussed by Hartley, however, is not restricted to examples such as these, but is of a much more general type. Scott has shown similar behaviour in the case of ebonite, which can be regarded as a three-dimensional network of polyisoprene molecules, cross-linked by sulphur. Liquids which have a strong swelling action, like benzene and its homologues, chlorobenzene, carbon disulphide, carbon tetrachloride, and nitrobenzene, are absorbed at a rate which soon becomes practically constant and remains so until the ebonite becomes saturated, when absorption ceases suddenly. This uniform rate of absorption is interpreted as representing the steady inward advance of the boundary between swollen and unswollen material, leading to the inference that the diffusion constant of the liquid is greatly increased when the solid is in the swollen condition (*Trans. Inst. Rubber Ind.*, 1937, 13, 109; *J. Rubber Res.*, 1941, 10, 123).

A good deal of attention has centred on the nature of the actual processes which occur when the solvent penetrates and swells a solid material. Nitrocelluloses are particularly interesting in this respect, since they can be regarded as typical examples of polymers with which a great variety of reactions can be effected owing to the stability of the cellulose chain and the reactivity of the hydroxyl and nitrate groupings. Petitpas and Mathieu (p. 17) have investigated the action of various solvents on the nitrocelluloses, and find that these fall into three categories. Solvents such as ketones attack the molecular chains, and the cohesion of these chains in arrangements based on an ordered crystal lattice, where it exists, offers only a negligible resistance to the fixation of the solvent. Hence absorption occurs throughout the whole mass of the fibres, including the regions of regularity as well as those of disorder, and the surface of the micelles. On the other hand, liquids such as methyl nitrate cause practically no change in the structure as revealed by X-ray methods, so it is inferred that they can penetrate only the regions of disorder, free portions of the chains, and the surfaces of the micelles. Esters such as methyl and ethyl acetate and methyl propionate show an intermediate behaviour, in that

they cause the structure, as revealed by the X-ray method, to change at the beginning of the absorption, but not in such a definite way as in the case of ketones, and it is only when the ester concentration in the gel is high that a clear modification of the structure occurs. Camphor shows a third distinct form of behaviour, in that it can penetrate into nitrocellulose fibres only in the presence of small molecules such as those of methyl alcohol, which seem to open up the road to the larger molecules.

A comparison of the swelling properties of various types of protein fibres in such organic solvents as alcohols, acids, formamide, and *m*-cresol has been made by Jordan Lloyd, Dempsey, and Garrod (p. 228). Extruded nylon and elastin (from ox ligament) resemble gelatin in that the swelling occurs both in length and diameter, the behaviour being attributable to disorientated molecular structure. Swelling of elastin fibres is usually accompanied by coiling, attributed to the release of strains caused by uneven compression of the individual fibres during growth. On the other hand, the swelling of cold-drawn nylon, silk, keratin (horsehair), collagen (from rat tendons), and reticulin (from beef suet) fibres occurs almost exclusively radially. Under certain circumstances this is accompanied in the cases of silk, collagen, and reticulin fibres by a marked axial contraction, and it is interpreted as due to orientated molecular structure. Fibres in the axially contracted state show rubber-like extensibility, and this state is interpreted as a half-way, but stable, stage on the way towards dissolution. By contrast, nylon fibres pass directly into solution without passing through this intermediate stage. The change from the non-extensible to the rubber-like form is reversible, but is accompanied by an irreversible deformation of structure. If the reagent which produced contraction is washed out with alcohol, new lateral bonds can re-form whilst the fibre is in the fully contracted or in the loaded condition. Many solvents seem to possess a critical temperature below which they cannot enter the fibre. Neither the size nor the polarity of the solvent molecules explains the interaction between the solvent and the fibre, and it is inferred that chemical interaction between the solvent and localised groups in the fibre must occur.

The investigators suggest that the arrangements of the polymeric molecules in the protein fibres fall into three different types. Firstly, the molecules may be disorientated, as in extruded nylon, elastin, and gelatin; secondly, they may be orientated in parallel alignment and held by cross-bonds, as in cold-drawn nylon, silk, collagen, and reticulin; and thirdly, they may be orientated in parallel but regularly folded alignment, and again held by cross-

bonds, as in keratin. When all the cross-bonds are of the same type, as with extruded nylon, any solvent which can open this bond will dissolve the fibre, whereas when the cross-bonds are of different types a solvent may be able to open one type but leave the others intact. If these remaining bonds are widely separated the chains will crumple, leading to a reduction in the length of the fibre and the development of rubber-like extensibility.

In the case of collagen the interrelation between the swelling and the hydrothermal stability has been considered in detail by Theis (p. 244). Collagen comprises long polypeptide chains held together partially by electrovalent linkages between the carboxyl and amino groups and partially by hydrogen bonds. The chains are not fully extended as in the case of silk fibroin, but are slightly curled, and the packing is not so close as in fibroin and wool keratin. The shrinkage temperature is greatly reduced when the *pH* value is decreased below 4.0 or increased above 9.0, indicating structural breakdown under such conditions. Analysis of the behaviour shows that at *pH* 3.0–11.0 these changes are attributable principally to the effect of the hydrogen and hydroxyl ions on the electrovalent (salt-like) linkages, their effects upon the hydrogen bond linkings becoming more evident outside this *pH* range. The swelling of collagen is also a minimum in the *pH* range 5.0–10.0, and increases to much higher values outside this range. A reduction in swelling and thus dehydration of the collagen occurs again, however, as the *pH* value is reduced from 4.0 to 0.5, this being attributed to the strengthening of the hydrogen bonds.

Studies of the hydrothermal stability and swelling of collagen in salt solutions give further information. At *pH* 5.5 dilute solutions of sodium chloride decrease the structural cohesive forces of collagen, doubtless by acting on the electrovalent linkages, whereas, due to dehydration forces, concentrated solutions cause decreased swelling and subsequent increased hydrothermal stability. Even very dilute solutions of calcium chloride reduce the structural cohesion and more concentrated solutions weaken the intermolecular network to such an extent that the collagen structure collapses. At such concentrations, therefore, this salt must act on both types of linkage. The importance of the electrovalent linkages in the collagen structure is also demonstrated by the fact that the stability of the structure is markedly reduced, particularly in alkaline solutions, after treatment with nitrous acid, which will reduce the number of amino-groups in the collagen.

The observation of dimensional changes during drying, followed by measurements of double refraction and X-ray observations on

the dried fibre has also been applied to this problem by P. H. Hermans and Vermaas (p. 155). The filaments used were of regenerated cellulose, produced by stretching threads of cellulose xanthate of various degrees of polymerisation and then decomposing the filaments into cellulose before drying. They infer that the behaviour conforms to that expected from a molecular network structure of which the degree of coiling in the primary gel is independent of its degree of swelling, and of which the orientation remains almost constant upon drying in the stretched state.

From these same measurements J. J. Hermans (p. 160) finds that the behaviour indicates that the statistical number of chain elements between successive junction points is about one in cellulose xanthate and in freshly regenerated cellulose. In reswollen cellulose the number of chain elements is somewhat larger, the average value being about two between each junction point.

Behaviour which appears at first sight to be of quite a different type is encountered during the swelling of starch in water. The structure of starch grains, and the processes involved when they swell and at certain temperatures, or in the presence of appropriate electrolytes, pass into the familiar starch paste has long been a problem which has excited considerable attention. The view that two chemically distinct compounds exist in the starch grains has passed, and Meyer (*Adv. Coll. Sci.*, 1942, 1, 143) has shown that both amylopectin and amylose are formed of the same chemical units. The amylopectin, however, comprising the majority of the starch molecules, has a ramified structure, and these are admixed with single chains of amylose.

According to the views of Badenhuizen (p. 255), the fact that the starch grain appears in optical cross-section to consist of a series of layers, in each of which lamellæ can be distinguished and of which the birefringence decreases from within outwards, is to be attributed to the mode in which it is built up. The periodic supply of carbohydrates during the growth of a starch grain causes the latter to be built up in a system of layers, all layers being similar. Contrary to earlier conceptions, it is not necessary to suppose that a special membrane surrounds the grain. During the swelling in water all layers swell at the same time, but since the outer layers have more swelling power in the tangential direction than the inner layers and the layers stick together, the inner part is stretched outwards. A small cavity is thereby formed in the centre of the grain, which enlarges as swelling proceeds, whilst the volume of the grain increases with consequent decrease in the thickness of the wall of the growing vesicle. So long as the wall of the

vesicle is smooth it is very elastic, in accord with the conception that the starch molecules are held together by a network of cross-linkages. A certain force is needed to break the lateral cohesive forces, this causing the swelling of the starch grain to start suddenly.

Goldenberg, on the other hand (p. 272), gives rather a different picture of the swelling of starch grains. He supposes that in the granules the amylose and amylopectin macro-molecules are held together, presumably by hydrogen bonds between neighbouring hydroxyl groups, in a structure comprising crystalline and amorphous regions, and similar to that obtaining in cellulose. The energy of activation needed to overcome the cohesive forces arising from this cause can be obtained either by raising the temperature or by the presence of certain electrolytes. Below a certain temperature the molecular framework does not break up, and hence the granules retain their microscopical appearance, birefringence, and X-ray diffraction pattern, and show a limited reversible swelling. Above this temperature the network is broken, and many hydroxyl groups previously tied to one another are freed to combine with water molecules, the starch macro-molecules become heavily hydrated, and the granule swells considerably to give the normal starch paste. Goldenberg also regards it as uncertain whether it is justifiable to accept a layer structure in the original granules when relatively drastic treatments, which may have produced such a structure, have been used to demonstrate its presence.

Finally it may be mentioned that Seifriz (p. 262) discussed the swelling and shrinking of protoplasm, the fibrous structural units of which form a continuous rigid but labile framework which has a great affinity for water, but is not miscible with it. Protoplasmic swelling and shrinking involve the extensibility and contractibility of the protoplasm, which in turn implies the adjustment of a structure which can extend and contract without loss of continuity between fibrous units. These properties lead to the conception of a structure in which the units are linear, but in which molecular folding can occur. In view of the present chemical views on the structure of proteins this leads to the idea that the contractibility of protoplasm is due to the supercontraction of its structural proteins through the folding of molecular fibres symmetrically aligned and joined to one another by side-chains to form a three-dimensional lattice. Conversely Seifriz considers that whenever contractibility occurs in animate nature, whether in protoplasm or in highly specialised tissue, it is brought about by the shortening of protein fibres by molecular folding, the energy for this work being supplied by oxidative processes in the cell.

The selected aspects of the subject discussed here illustrate the general picture which is being built up of the structure of gel-forming materials, and the mechanisms of their swelling and shrinking. The essential basic structure involves long macro-molecules, which are linked together by cross-linkages into a three-dimensional lattice. The cross-linkages may be true covalent bonds, electrostatic linkages, or hydrogen bonds, and in many of the systems there are at least two different forms of cross linkage present. The mechanical properties of the gel-forming material depend to a considerable extent on the frequency of the cross-linkages between neighbouring fibrillar molecules. The polymer chains are generally folded so as to give an elasticity to the structure.

When a liquid is imbibed, it may simply cause the stretching of the structure or it may cause partial disruption of the cross-linkages, each of which effects increases the overall volume of the gel. The imbibition and swelling are naturally inseparable phenomena, and it has been found, for instance, that in the case of cellulose-laminated phenol-formaldehyde plastics the hygroscopicity of the components appears to be reduced, most probably owing to the mechanical restraint operating mutually on the resin and cellulose preventing their swelling (Barwell and Pepper, p. 275). If, however, the solvent has the effect of breaking all the cross-linkages, the macro-molecules pass into the sol state.

The most orderly arrangement of the macro-molecules in a lattice is observed in the cases of natural fibres and of synthetic fibres which have been subjected to considerable elongation so as to draw their chains into a parallel arrangement. Regions of the structure where the cross-linkage is less perfect are more readily attacked by swelling agents, and in certain cases there are macro-molecules present which are scarcely cross-linked at all. Such molecules will pass into solution more readily than those which fall into the general network.

The facts that these properties are possessed by natural plant and animal tissues as well as by synthetic products, and that swelling and shrinking play important rôles in physiological processes, render this aspect of colloid chemistry one of the most important branches of physical science.

"CLATHRATE" COMPOUNDS.—It has been known for many years that, besides forming the normal types of molecular compounds associated with phenols, quinol gives complexes with volatile compounds such as hydrogen sulphide, sulphur dioxide, formic acid, hydrogen cyanide, hydrogen chloride, and hydrogen bromide. These compounds, which contain three or four molecules of quinol

to each molecule of the other component, are stable under ordinary storage conditions and have no smell of the volatile compound, but are decomposed when heated to near their melting-points or when dissolved in water. These compounds have always been somewhat of an enigma, and Mylius in 1886 came to the conclusion that no ordinary chemical combination occurs and suggested that during crystallisation the quinol molecules were in some way able to lock the volatile compound in position. This conclusion has just been vindicated in a remarkable manner by Palin and Powell (*J. Chem. Soc.*, 1947, 208; 1948, 61, 571), who have restudied some of these compounds by examination of their crystal structures. They find that the quinol molecules link together through hydrogen bonds to form infinite three-dimensional complexes of trigonal symmetry. The resultant giant molecule has a form which is so open that two such molecules are built up completely interpenetrating each other but having no direct attachment. The molecules of the volatile component of the complex are enclosed in the cavities between two giant molecules in such a manner that they cannot escape from their surroundings. The composition of the complex is determined by the number of spaces available for the enclosed molecules. Such substances have been called by Powell "Clathrate" compounds (Latin *clathratus* = enclosed or protected by cross-bars of a grating).

This explains the fact observed by Mylius that when quinol was heated with formic acid in a sealed tube a product was obtained which liberated carbon monoxide on treatment with water or on fusion, but of which the composition was such as to preclude its being an addition compound of quinol and carbon monoxide. It can be inferred that it was a clathrate compound where the enclosed molecules were partly formic acid and partly carbon monoxide.

Similar structures occur with the complex $[\text{Fe}(\text{CNCH}_3)_6]\text{Cl}_3 \cdot 3\text{H}_2\text{O}$ (Powell and Bartindale, *J. Chem. Soc.*, 1945, 799), where the CNCH_3 arms of the complex ion project into the spaces between three of the arms of another ion. Forces of interionic attraction then bind the complex ions and chloride ions so as to form a strong cage structure which encloses not only the chloride ions but also the water molecules, which cannot escape. This accounts for the fact that the water in these crystals can be removed to only a very small extent. Prolonged heating at a temperature slightly below 100° causes only slight dehydration, whereas at higher temperatures decomposition occurs with loss of methyl chloride.

The so-called β -form of quinol obtained by Caspari (*J. Chem. Soc.*, 1926, 2944; 1927, 1093) by crystallisation from methyl

alcohol has now been shown by Palin and Powell by chemical analysis, molecular weight determination from unit cell dimensions, and detailed crystallographic structure to be $3C_6H_4(OH)_2 \cdot CH_3OH$, similar to that of the sulphur dioxide addition product.

In each of these cases the essential condition that the cage shall form around the second component is achieved by its being built up bit by bit from small components capable of separate existence in the solution, which also contains a large concentration of the molecule to be enclosed. Thus, for instance, the quinol-sulphur dioxide compound cannot be formed from solid quinol and sulphur dioxide gas, and hence the formation is restricted to second components with appropriate solubility. Increased pressure can be used, however, to increase the solubility, so the formation of such compounds with, for example, rare gases as the enmeshed component can be visualised.

Other known compounds which are probably of the same type, but of which the structures are yet to be proved, include the addition product of quinol with carbon dioxide, of variable composition but containing not more than one molecule of carbon dioxide to each six molecules of quinol. Probably similar, too, are the compounds containing two molecules of orcinol or phloroglucinol to one molecule of hydrogen bromide, the addition compounds of *p*-aminophenol, *p*-methylaminophenol, and *p*-phenylenediamine with sulphur dioxide. The compounds of the choleic acids with fatty acids, hydrocarbons, etc., may be essentially of the same type, but with the second components contained in extended channels formed by a structure of deoxycholic acid rather than in closed cavities of limited dimensions.

BIOCHEMISTRY. By W. O. KERMAK, D.Sc., LL.D., F.R.S., Royal College of Physicians' Laboratory, Edinburgh.

PERNICIOUS ANÆMIA, FOLIC ACID AND RELATED TOPICS

IN 1926, Minot and Murphy made their famous discovery that patients suffering from pernicious anæmia were greatly benefited, and sometimes restored to normal health, by feeding with liver. The defective bone-marrow regained the power to manufacture healthy blood cells and in the course of a few weeks, or even days, the red blood-cell count would increase from the gravely low level of perhaps about a million to something approaching the normal value of about five millions per cubic millimetre. Potent commercial liver preparations soon became available for the treatment of pernicious anæmia, and the therapy was in general so effective that

researches on the nature of the active factor have been seriously handicapped because of the lack of patients upon whom active preparations could be assayed. Nevertheless, strenuous efforts have continued to be made to concentrate and purify the "anti-pernicious anaemia" factor present in liver, and during the past two or three months two reports have appeared which indicate that important success has been achieved in this field. However, before referring in detail to the two announcements, the one from the Merck Laboratories in the United States, the other from the Glaxo Laboratories in Britain, which give preliminary details concerning this new advance, it is desirable to recount briefly recent discoveries which have an important bearing on the problem of blood-cell formation. These concern the group of substances known loosely as "folic acid"; a very large amount of work on this subject has been done during the past ten years, mainly in the United States. The following brief account merely indicates some of the main results of biochemical interest. The reader who wishes to pursue the subject may be referred to some of the very useful review articles which have appeared; for example a chapter on "The Use of Pteroylglutamic Acid (Liver *L. casei* factor, folic acid) in Clinical Studies," by T. D. Spies in the *Annual Review of Biochemistry*, 1947, 16, 387-400, and an article entitled "Pteroylglutamic Acid and Related Compounds," by T. H. Jukes and E. L. R. Stokstad in *Physiological Reviews*, 1948, 28, 51-106. Reference may also be made to a concise and readable survey of "Anti-anaemia Factors," by J. R. P. O'Brien in the *Annual Reports of the Chemical Society*, 1946, 43, 296-306.

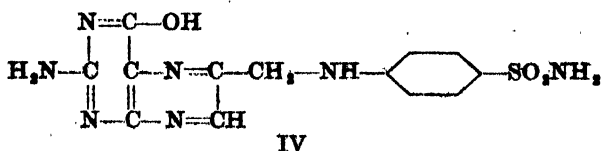
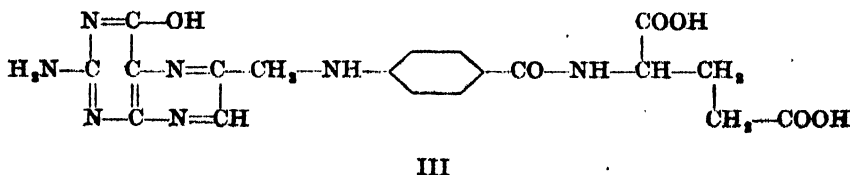
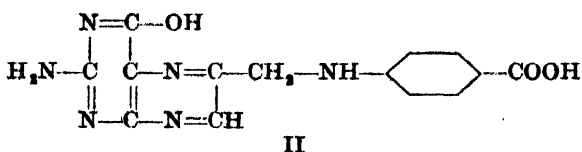
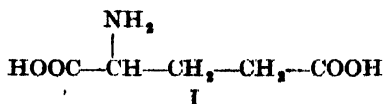
FOLIC ACID.—Folic acid, as originally defined (Mitchell, Snell, and Williams, *J. Amer. Chem. Soc.*, 1941, 63, 2284), denoted the factor present in spinach which stimulated the growth of cultures of *Streptococcus lactis R* (also known as *S. faecalis R*). However, the name has been somewhat loosely applied to a group of compounds, all closely related, and characterised by their ability to promote the growth of *Lactobacillus casei*, another organism often used for microbiological assay of chemical compounds, and, like *S. lactis R*, stimulated by the original folic acid from spinach.

Three crystalline *L. casei* factors have been obtained, one from liver, one from yeast and one from the products of fermentation by a diphtheroid bacterium; they are called respectively the liver, yeast and fermentation *L. casei* factors. Of these factors, the first is the simplest in structure; its constitution has been completely elucidated and the compound synthesised by Angier and his team of the Lederle Company, U.S.A. During the past two or three

years, this "synthetic folic acid" has become commercially available and has been used in many clinical investigations. •

The relation between this liver factor and the crystalline factors isolated from yeast and fermentation residues is best understood in terms of their structure.

All three compounds are formed out of the well-known amino-acid glutamic acid (I) and the much less familiar pterioic acid (II). This acid may be regarded as a derivative of *p*-aminobenzoic acid, recently become so familiar because of its connection with the sulphonamide drugs. One of the hydrogen atoms of the amino group in this acid has been replaced by a somewhat complex cyclic structure belonging to the pterin series. The pterins are so called because they were first encountered as pigments in insects; the wings of the cabbage white butterfly contain leucopterin, whilst the yellow xanthopterin is found in the yellow markings on wasps' bodies, and it has been shown that pterin derivatives are widely distributed in animal tissues. Thus it is not surprising that these compounds should turn out to have important biochemical functions.



The crystalline *L. casei* factor (III) is simply pteroylglutamic acid, the carboxyl group of the pterioic acid uniting with the amino group of the glutamic acid through an amide linkage. The fermentation *L. casei* factor is pterioic acid united to three molecules of glutamic acid, the latter presumably forming a polypeptide chain

held together by amide linkages. Yeast *L. casei* factor is still more complicated; it contains no fewer than seven glutamic acid molecules and so is sometimes called pteroylheptaglutamic acid. Both the yeast and fermentation factors, especially the former, are often referred to as folic acid "conjugates," the simple folic acid or pteroylglutamic acid being combined or "conjugated" with the additional glutamic acid molecules. It is possible that further conjugates may yet be discovered, containing various numbers of glutamic molecules other than three or seven, and even other amino-acids as well.

The structure of the original "folic acid," the *S. lactis R* factor from spinach, does not seem to have yet been conclusively proved, but it is very probable that it is simply pterioic acid itself. Though pterioic acid is not active when tested on *L. casei*, it is converted into an active compound by the action of *S. lactis R*. The latter organism evidently has the power of synthesising pteroylglutamic acid from pterioic acid and glutamic acid, while *L. casei* has not.

So far, we have been discussing the folic acids in terms of their action in stimulating bacterial growth. It has become very clear, however, that pteroylglutamic acid is also very important for the avian and mammalian organisms.

Vitamin B₁₂ was originally defined as a factor present in liver and yeast, needed for the proper growth and health of chicks. In the absence of vitamin B₁₂ the birds develop a macrocytic anaemia and the feathering may also be defective. Rats on a purified diet to which sulphasuxidine has been added to inhibit bacterial growth in the intestine also develop a disturbance of blood-cell formation. Another animal disease is observed in monkeys fed on a basic diet of polished rice. The symptoms include anaemia, leucopenia, ulceration of gums, diarrhoea, oedema and susceptibility to bacillary dysentery; the illness, which has a distinct resemblance to human sprue, usually terminates fatally within 26 to 93 days. The conclusion was reached that this malady was due to the absence from the diet of a hypothetical vitamin, the so-called vitamin M. It turns out that pteroylglutamic acid has a dramatic therapeutic effect on all three diseases. It will be observed that in each of these three diseases a prominent symptom is a disturbance of the power of the bone-marrow to form blood cells. The administration of pteroylglutamic acid is rapidly followed by the return of the bone-marrow to normal and the restoration of the power to manufacture healthy blood cells.

The power of pteroylglutamic acid to act in these deficiency diseases of chicken, rat and monkey is shared by its conjugates,

pteroyltriglutamic acid and pteroylheptaglutamic acid. Many animal tissues contain an enzyme which splits off glutamic acid from these conjugates and converts them into pteroylglutamic acid itself. This special polypeptidase, usually called conjugase, is found in mammalian kidney, liver, pancreas and intestine; other sources are chicken pancreas and almonds. It seems probable that the conjugates, pteroyltri- and heptaglutamic acid do not exert their therapeutic action directly, but are first hydrolysed in the animal body by the widely distributed conjugase to yield the vitamin proper, pteroylglutamic acid. It should be added that many vegetable materials contain a conjugase inhibitor. As a large proportion of the pteroylglutamic acid present in the diet appears to be in the form of its conjugates, it is possible that, under certain conditions, this anti-conjugase may conduce to the development of deficiency symptoms.

It seems justifiable, then, to identify vitamin B₉ and vitamin M with pteroylglutamic acid, that is, the liver *L. casei* factor.

The crystalline pteroylheptaglutamic acid from yeast is sometimes referred to as vitamin B₉ conjugate. It will be remembered that originally this was called the yeast *L. casei* factor. It is stated, however, that pure vitamin B₉ conjugate has no action on *L. casei* until after it has been hydrolysed by conjugase, and so it would seem that the original impure "yeast *L. casei* factor" contained pteroylglutamic acid or some lower active conjugate, as well as the heptaglutamic acid; this other factor may either have been present in the yeast or may have been produced by hydrolysis during the process of extraction and working up. The point is mentioned, as it illustrates the complexity of the subject and the difficulty of obtaining an absolutely clear picture.

FOLIC ACID AND HUMAN ANÆMIAS.—In view of the action of pteroylglutamic acid in improving the bone-marrow and correcting the blood picture in animals suffering from anæmias of nutritional origin, it is not surprising that it should have been tested clinically on a large scale in cases of human anæmia of various types. Broadly speaking, the result has been to show that it has a potent action on macrocytic anæmias associated with a megaloblastic bone-marrow. Here, treatment with doses of pteroylglutamic acid of the order of 20–100 milligrams daily usually brings about a rapid improvement in the condition of the bone-marrow and the regeneration of healthy blood cells in much increased numbers. Amongst the anæmias in which pteroylglutamic acid has been found to be useful therapeutically are pernicious anæmia, megaloblastic anæmias of pregnancy and infancy, pellagra and sprue, and macrocytic

nutritional anæmias in general. The vitamin—for pteroylglutamic acid may justifiably be so described—may be administered either orally or parenterally, the dose varying to some extent according to the route of administration and the condition of the patient. Clinical investigations in this field have been summarised and discussed in a recent lecture on "Folic Acid," by Dr. J. Wilkinson, published in the *British Medical Journal*, 1948, 1, 771-6, 821-7).

The effect of pteroylglutamic acid in pernicious anæmia is of particular interest. There is general agreement that usually the administration of the compound results in a rapid improvement of the condition of the bone-marrow and a rise in the red blood-cell count. The patient feels much benefited and at first sight it might seem that pteroylglutamic acid was a satisfactory substitute for liver extracts. Indeed, when it is remembered that pteroylglutamic acid was originally isolated from liver as the liver *L. casei* factor, the possibility naturally suggests itself that it might be the anti-pernicious anæmia factor postulated as the source of the efficacy of liver concentrates. It was soon realised, however, that this identification of pteroylglutamic acid with the anti-pernicious anæmia factor was untenable. It could be shown that certain highly potent, purified liver extracts were almost devoid of pteroylglutamic acid. As we have seen, this compound has to be given in doses of the order of some tens of milligrams in order to be effective; the liver extracts were active in doses which definitely could not contain more than a few micrograms of pteroylglutamic acid. Then, the acid is active when given orally, whereas the purified liver extracts have to be injected. Though crude liver extracts contain pteroylglutamic acid, it turns out that this is largely removed in the concentration and purification of the commercial preparations for use in pernicious anæmia, and the conclusion was inevitable that liver contains some other compound, probably of unusually high potency, responsible for its anti-pernicious anæmia effect.

This conclusion was borne out and its importance underlined by another set of considerations. Besides the disturbances of blood and bone-marrow, patients suffering from pernicious anæmia tend to develop nervous disturbances. The lesions are described as sub-acute combined degeneration of the cord and form a characteristic feature of the later stages of the disease. The question at once arose as to whether pteroylglutamic acid would prevent these nervous afflictions as well as improve the blood and bone-marrow. As the months and years have passed, it has become increasingly evident that the answer is definitely in the negative. It appears

to be generally agreed that the neurological lesions appear in patients treated with pteroylglutamic acid with at least as great severity as in untreated cases. This failure of pteroylglutamic acid to prevent the nervous symptoms of pernicious anæmia is in contrast with the efficacy of liver preparations in that respect, and it confirms the conclusion that the liver factor and pteroylglutamic acid are different compounds.

THE ANTI-PERNICIOUS ANÆMIA FACTOR OF LIVER.—We come back now to the recent work, referred to at the beginning of this article, which has resulted in the isolation of what is apparently the true anti-pernicious anæmia, or as it is sometimes called the "erythrocyte regeneration" factor of liver. The preliminary communications on the subject are in the form of a letter to *Nature* (1948, 161, 638, April 24) by Lester Smith, and in an article in *Science* (1948, 107, 396, April 16) by Rickes, Brink, Koniuszy, Wood and Folkers.

Lester Smith purified the crude extract from ox-liver by the methods employed by Emery and Parker (*Proc. Biochem. Soc.*, 1946, 40, iv) followed by repeated chromatography. A proteolysed liver extract gave better yields of a product differing slightly in chromatographic behaviour, and of higher activity. Preparations of both types were separated by partition chromatography into two red fractions; the proportions suggest that one arises from the other by proteolysis. Barely 1 g. of red materials has been obtained from 4 tons of liver. One preparation from proteolysed liver was active in a single dose of 0.3 mg. (in pernicious anæmia) and it is of particular interest that this preparation was potent in three cases of subacute combined degeneration of the spinal cord. There is evidence that further purification may be achieved. The products are amorphous solids having a colour resembling cobalt salts. Diffusion measurements indicate that the molecular weight of both proteolysed and non-proteolysed material is about 3000. It is concluded that both preparations are forms of a single factor effective in pernicious anæmia, and that the effective dose of the pure compound may be as low as about 20 micrograms daily (possibly only about 2.5 micrograms).

The American workers, Rickes, Brink, Koniuszy, Wood and Folkers have evidently succeeded in carrying the purification of their product still further and report the separation of a compound crystallising in the form of small red needles, which, on the micro stage, darken to black at about 210°–220°, but do not liquefy below 300°. Unfortunately, no chemical details are yet available concerning this new compound, which the authors call vitamin B₁₂.

Not even its analysis is published and so we are in complete ignorance as to its structure. According to West (*Science*, 1948, 107, 398), vitamin B₁₂ has produced positive hæmatological responses in three patients suffering from pernicious anæmia, following single intramuscular injections of 3, 6 and 150 micrograms respectively. Four patients receiving single injections of impure amorphous concentrates containing probably 2-4 micrograms gave strong or maximal responses, while three receiving 1 microgram or less gave weak or negative responses. Evidently the product is active in doses of a few micrograms; this means that it has about 2000 times the activity of pteroylglutamic acid.

The relation of vitamin B₁₂ to Lester Smith's products is not altogether clear, though it seems very probable that the latter is vitamin B₁₂ in a still impure form, and there is some evidence to suggest that it may contain vitamin B₁₂ combined with amino-acids in the form of a conjugate. In a second letter to *Nature* (1948, 162, 144, July 24) Lester Smith announces that the pure pernicious anæmia factor, which the Glaxo workers too have obtained crystalline as red needles, contains one atom of cobalt and three atoms of phosphorus per molecule. The molecular weight on the basis of chemical and X-ray analysis is about 1500 (anhydrous material). Though cobalt has been known for some time as a trace metal important in animal nutrition and apparently concerned in hæmopoiesis, this is the first case of the isolation of a naturally occurring organic compound containing cobalt. The American workers are stated to have obtained similar results.

In their isolation of vitamin B₁₂ the American workers were guided by a method of microbiological assay which reduced their dependence on the troublesome use of cases of pernicious anæmia (Shorb, *Science*, 1948, 107, 397). It had previously been found that *Lactobacillus lactis* Dorner (L.L.D.) required for its growth two factors, one of which, present in liver concentrates, appeared to be closely associated with the anti-anæmic potency. Further work has left little doubt that this L.L.D. factor is identical with vitamin B₁₂. A simple microbiological method of assay of this type saves much time, labour and material as compared with the troublesome process of testing on patients whose clinical condition may be variable and uncontrollable.

RELATIONSHIP OF THE VARIOUS ANTI-ANÆMIA FACTORS.—It may be useful to summarise very briefly our present knowledge of the various anti-anæmic factors which have come to light during the past twenty years. The work of Castle indicated the existence of what he called an extrinsic factor present in beef muscle and

certain other foodstuffs, and an intrinsic factor present in gastric juice and membrane. The intrinsic factor, sometimes called hæmopoietin, appears to be an enzyme, and its function is to convert the extrinsic factor into the active hæmopoietic principle, needed for the proper maturation of red blood cells in the bone-marrow. This active hæmopoietic compound is stored in the liver and it is this which has now been purified by Lester Smith and by Rickes and his colleagues. The fundamental defect in pernicious anæmia is supposed to be defective production of the intrinsic factor, with a secondary deficiency of what has now been named vitamin B₁₂. Thus a pernicious anæmia patient may be treated either by supplying the intrinsic factor in the form of hog's stomach or normal gastric juice, or by injecting active liver preparations.

So far the situation is fairly clear, but if we ask where pteroylglutamic acid fits in, we have to admit ignorance. It is evident that pteroylglutamic acid is normally needed for proper blood-cell formation. The various macrocytic nutritional anæmias in man and animals, as mentioned above, may be cured promptly by administration of this compound and are evidently due to lack of it. In pernicious anæmia, the administration of pteroylglutamic acid improves the bone-marrow and the blood picture. But pernicious anæmia is definitely not due to lack of folic acid (used in the broad sense of pteroylglutamic acid and its conjugates) in the diet, for pernicious anæmia patients are not normally found to have been on a deficient diet, and, besides, pteroylglutamic acid, though it improves the anæmia, does not prevent other symptoms of the disease, such as the nervous manifestations. Pernicious anæmia, in fact, is the result of a lack of vitamin B₁₂, due primarily to deficiency of the intrinsic factor in the stomach. This liver factor corrects the disorder, and, without special administration of pteroylglutamic acid, restores the bone-marrow and blood to health.

It seems clear, then, that both vitamin B₁₂ and pteroylglutamic acid are needed for health, and in the absence of one or the other a macrocytic megaloblastic anæmia develops. But clearly it is not just a question of these two factors being both required and the hæmopoietic mechanism breaking down when either is absent; otherwise why should pteroylglutamic acid administered to pernicious anæmia patients, who are devoid of vitamin B₁₂, bring about something like normal blood-cell formation by the bone-marrow?

Somehow or other the two factors are related in their function. The suggestion has been made that patients suffering from pernicious anæmia are deficient in the enzyme conjugase, and that in consequence they are unable to utilise the vitamin B₁₂ conjugate. Now,

it would appear that the greater part of the folic acid in ordinary foodstuffs is in the form of conjugate, and so in pernicious anæmia, on this theory, the disturbances in bone-marrow and blood would be really due to lack of available pteroylglutamic acid. Vitamin B₁₂ would be necessary for the activity of conjugase, possibly being required for its synthesis. Though some evidence has been put forward to support this view, it is far from conclusive, and other observations seem to disprove it.

Another possibility is that vitamin B₁₂ might be necessary to prevent the transformation of pteroylglutamic acid into some inactive form. For example, in the complicated hæmopoietic process within the bone-marrow, the acid might function in some mechanism which involves its transformation into a second compound, X, which under the action of an enzyme was reconverted into pteroylglutamic acid. Suppose now that vitamin B₁₂ was part of this enzyme. Then, in its absence, X would accumulate and pteroylglutamic acid would be used up. The administration of extra pteroylglutamic acid would overcome the induced deficiency and allow hæmopoiesis to proceed. If the substance X happened to be toxic to nervous tissues, the nervous symptoms would be explained. It is of interest that there is some suggestion that pteroylglutamic acid treatment in pernicious anæmia may actually aggravate the nervous disorder (see Wilkinson, *loc. cit.*, 825); this result would be consistent with the above suggestion. However, the idea is put forward here simply to illustrate how the puzzling and seemingly inconsistent facts might be reconciled.

PTEROIC ACID SYNTHESIS AND SULPHANILAMIDE.—The presence in pterioic acid of the *p*-aminobenzoic acid grouping cannot fail to arouse special interest in view of the part which *p*-aminobenzoic acid plays in connection with the action of the sulphonamide drugs. It is a generally accepted view that sulphanilamide exerts its bacteriostatic action, in part at least, by interfering with some biochemical mechanism in which *p*-aminobenzoic acid is concerned (see SCIENCE PROGRESS, 1948, 36, 285). The possibility suggests itself that sulphanilamide may interfere with the synthesis of pterioic acid; some facts seem to support this view. Thus bacteria which require pterioic acid (or pteroylglutamic acid) for their growth are not sensitive to sulphonamides in presence of this acid. As they are not capable of synthesising pterioic acid, there is no synthetic process to interfere with. In certain bacteria which can dispense with preformed pterioic acid as a growth factor and are sulphonamide sensitive, the action of the drug may be counteracted either by *p*-aminobenzoic acid or by pterioic acid. The

evidence strongly suggests that man and other mammals cannot synthesise pteric acid in their tissues, and this may be the reason why they are not sensitive to the action of sulphanilamide; why in fact this drug, though it acts on certain bacteria, has no corresponding action on the mammalian host.

There are two distinct ways in which sulphanilamide might interfere with pteric acid synthesis. It might act through substrate competition, that is to say, it might displace *p*-aminobenzoic acid from the surface of the enzyme which brings about its incorporation into the pterin molecule; the enzyme reaction would thus be completely blocked. Alternatively, the sulphanilamide might actually take the place of *p*-aminobenzoic acid in the synthetic process, so that the product, instead of being pteric acid (III), would be the sulphonamide derivative (IV). This compound would be unable to combine with glutamic acid and so would be devoid of the biochemical activity of pteric acid. This second explanation differs from the first in that it does not involve "substrate competition"; it depends on "chemical competition" in the wider sense (see SCIENCE PROGRESS, *loc. cit.*).

Some experiments described by Forrest and Walker (*Nature*, 1948, 161, 722) are interesting in this connection. They report that pteric acid derivatives may very readily be obtained from *p*-aminobenzoic acid, a suitable pyrimidine derivative, and "reductone," a three-carbon atom compound of the formula $\text{CH}(\text{OH})\text{:C}(\text{OH})\text{.CHO}$, probably available in the cell as a product of carbohydrate metabolism. Forrest and Walker, in fact, find that the condensation product of reductone with the ethyl ester of *p*-aminobenzoic acid reacts in ethyl alcohol with 2:4:5-triamino-6-hydroxypyrimidine to give an excellent yield of ethyl pteroate. When the reductone-*p*-aminobenzoic acid ethyl ester compound is replaced by reductone-sulphanilamide the sulphonamide analogue (IV) of pteric acid is obtained.

This result is of great interest for it may indicate the route whereby pteric acid is synthesised by the organism. Further, it strongly suggests that the chemically similar sulphanilamide may take the place of *p*-aminobenzoic acid in this synthesis and so produce the biochemically inactive compound (IV).

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

PETROLOGY—THE GRANITE PROBLEM.—T. F. W. Barth has provided a long and penetrating review of recent works by Backlund, Perrin and Roubault, Niggli, Reinhard and Raguin, in "Recent

Contributions to the Granite Problem" (*Journ. Geol.*, **56**, 1948, 235-40). Some of these memoirs, which the writer has not seen, are noticed in the present Advances, with comments that are taken from Barth's review. Papers by Perrin and Roubault, and by Niggli, were noticed in a previous contribution (*SCIENCE PROGRESS*, XXXV, 1947, 120-8).

A considerable book, *Géologie du Granite* (Paris : Masson et Cie., 1946, 211 pp.) by E. Raguin, assembles the facts and analyses the theories that have been advanced to explain the origin of granite. But from Dr. W. C. Campbell's long review (*Geol. Mag.*, LXXXIV, 1947, 254-6) it seems that, while the problems have been faced, they have by no means been solved. Anatectic granites with gradational margins are distinguished from the sharply intrusive bodies. Ionic diffusion in the solid state as advocated by some authors is not accepted as a *vera causa*. Raguin believes that granite has passed through a liquid phase and is not a metamorphic rock.

In his Rectorial Address to the University of Basle "Über die Entstehung des Granits," M. Reinhard (*Basler Universitätsreden*, **16**, 1943, 38 pp.) takes the view that most granites are of mixed origin, and the main problem is to determine the extent to which they are magmatic or metamorphic. He postulates three modes of origin for granites : (1) by fractional crystallisation of a basaltic magma ; (2) residual granitic magmas rich in volatile constituents, and their pegmatitic successors rich in alkalis and silica, react not only with the already crystallised minerals, but with adjacent sandstones and shales, changing them into rocks indistinguishable from granite ; (3) the deeper parts of orogenic zones, dominantly granitic in composition, are subjected to a differential anatexis, with the formation of a granitic ichor or "migma" which permeates the whole rock complex.

In his later contribution to the granite problem, entitled "Die leucogranitischen, trondhjemitischen und leucosyenitischen Magmen und die Anatexis," P. Niggli (*Schweiz. Min. Petr. Mitt.*, **26**, 1946, 34 pp.) relaxes somewhat from his former uncompromising magmatic position (*SCIENCE PROGRESS*, XXXV, 1947, 123), and inclines to much the same views as Reinhard. He is now willing to accept at least three kinds of granite : (1) magma-granite, directly crystallised from a magma ; (2) meta-granite due to simple recrystallisation of rocks of granitic composition such as arkose and certain types of shale ; (3) migmatite-granites, representing the end-product of the metasomatic series : sediments—metamorphite—ultrametamorphite—migmatite-granite.

In his latest work, "Om granit och gnejs och jordens ålder [On Granite and Gneiss, and the Age of the Earth]," H. G. Backlund (*Kungl. Vetenskapsoc. Årsbok*, Upsala, 1947, 67 pp.) discusses a variety of topics including ore-deposits and pegmatites associated with granite, "basic fronts," and the evolution of the series green-schist—amphibolite—metabasite, which is closely connected with the gneiss-granite problem. Backlund believes that all Pre-Cambrian gneiss areas represent relics of old mountain chains produced by several successive orogenies. Intense granitisation takes place in each orogeny; and sediments are replaced by granite and gneiss in all geological eras. Sediments of granitic composition (arkoses, etc.) are most easily granitised. If chemical differences are great, the original rocks will resist granitisation much longer. The "emanation" consists of a migration of ions facilitated by lattice irregularities and deformations, substitutions and inversions. The processes of granitisation are slow even in comparison with rates of denudation and erosion.

A comprehensive investigation of "The Sequence of Geochemical Changes leading to Granitisation" by Dr. D. L. Reynolds (*Quart. Journ. Geol. Soc.*, CII, Pt. 2, 1946, 389-446), using von Wolff diagrams showing the chemical relations of related series of granite, sediments, hornfelses and inclusions, reveals that the initial change in rocks of all kinds is enrichment in mafic constituents and alkalis. Pelitic and semi-pelitic rocks, metasomatically altered in contact with granitic material, undergo the changes in two quite distinct stages. In the first stage they become desilicated relative to the bases present, and give rise to syenitic types or rocks resembling basic and ultrabasic igneous rocks. During the second stage of alteration the desilicated rock is "granitised," i.e. by the addition of silica and an alkali, and by the loss of some alumina and cafermic constituents, its composition approaches that of the associated granite. Somewhat similar series of changes are investigated for psammitic rocks, limestones, and basic igneous rocks. The paper is followed by a valuable discussion in which N. E. Odell and H. G. Backlund took part.

Dr. Reynolds's paper "The Association of Basic 'Fronts' with Granitisation" (*SCIENCE PROGRESS*, XXXV, 1947, 205-19) explores the idea that the advance of fronts of migmatisation or granitisation displaces the cafermic constituents from the affected rocks. Thus a "basic front" is generated in advance of the granitisation front, producing rocks such as amphibolites, skarns, basic hybrids and the like. Basic haloes may form even around inclusions in granites. From this standpoint Dr. Reynolds discusses Sederholm's observa-

tions on the Finnish Archaean, and Backlund's on the Caledonian "granites" of Fennoscandia and Greenland. The rocks of the classical regions in the North-west and Grampian Highlands are also reinterpreted from the same point of view.

Dr. Reynolds has also discussed and reinterpreted the anthophyllite-cummingtonite-cordierite rocks of Cornwall from the point of view of basic fronts ("Hercynian Fe-Mg Metasomatism in Cornwall: a Reinterpretation," *Geol. Mag.*, LXXXIV, 1947, 33-50). Professor Tilley's view is that these rocks were developed from basic igneous types by metasomatic processes involving removal of lime and addition of silica and alkalis, especially potash. Dr. Reynolds would substitute the idea that in Cornwall we have examples of basic fronts generated by and driven in advance of Hercynian granitisation.

Dr. Reynolds's paper "On the Relationship between 'Fronts' of Regional Metamorphism and 'Fronts' of Granitisation" (*Geol. Mag.*, LXXXIV, 1947, 106-9) is a penetrating review of papers by Perrin and Roubault (*Bull. Soc. Géol. France*, 1941) and Lapaduhargues (*ibid.*, 1945). The first deals with a metamorphic front of apparently anomalous characters between Triassic conglomerate and an underlying schist; the second presents a statistical study of the chemical composition of regional metamorphic rocks and related granitic types. Both papers are closely bound up with the granitisation theory; and, on this basis, Dr. Reynolds discusses the parallelism between fronts of granitisation, basic fronts, migmatite fronts and metamorphic fronts.

As its title indicates, Dr. D. L. Reynolds's paper "The Granite Controversy" (*Geol. Mag.*, LXXXIV, 1947, 209-23) is mainly controversial, maintaining the "Transformist" view of the origin of granite against various criticisms by "Magmatists." It also contains a good discussion of Hutton's observations in Glen Tilt, and of the processes of ionic diffusion which are believed to lead to the transformations in question.

T. F. W. Barth describes "The Birkeland Granite, a Case of Petroblastesis" (*C.R. Soc. Géol. de Finlande*, XX, 1947, 173-82) as a granitic mass developed within gneisses, the strike of which conforms to the margin of the mass. In places it is impossible to draw any boundary line between granite and gneiss. The phenomena show that the Birkeland Granite did not originate simply by intrusion of "younger" granitic magma, but was generated by a slow process of metasomatic replacement. It is neither magmatic nor metamorphic. Barth pictures the process as replacement in greater or smaller degree by a cloud of migrating ions of granitic composition,

penetrating the crystal lattices and intergranular spaces. If the ions crystallise directly to granite the process is petroblastic; if they react with a pre-existing rock and then crystallise, it is a metasomatic process.

Describing "The Textural Features of the Granites and Invaded Rocks of the Singo Batholith of Uganda and their Petrogenetic Significance," B. C. King (*Quart. Journ. Geol. Soc.*, CIII, 1947, 37-64) shows that the batholith invades arenaceous and argillaceous rocks of the Karagwe-Ankolean System which were sharply folded before the emplacement of the granite. The structures show that the invaded rocks were not displaced, but replaced, by the granite, which is nevertheless extremely uniform in composition. Some of the marginal granites contain sericite instead of feldspar, and are only distinguishable from sediments by the absence of bedding. Quartz has always crystallised early, and the sericite is believed to represent an intermediate stage in its replacement by feldspar. The coarser granites also show the replacement of quartz by potash feldspar and by oligoclase.

It is therefore believed that the granite never existed wholly as a magma, but that it was formed as the result of a series of replacements by material introduced from below, and emplaced within the invaded rocks.

B. C. King's paper on "The Mode of Emplacement of the Post-Karagwe-Ankolean Granite of South-west Uganda" (*Geol. Mag.*, LXXXIV, 1947, 145-56) shows that the granite was emplaced within two systems of ancient sedimentary rocks, the Toro System (quartzites and pelitic schists) and the Karagwe-Ankolean System (mudstones, phyllites, sandstones, quartzites), named in order of age. The granite is believed to have been generated within the Toro System which, however, has remained structurally almost unaffected; but the granite has a much greater degree of cross-cutting relations with the Karagwe-Ankolean rocks. This difference of behaviour is correlated with differences of structure in the two systems. "The effect of the structural contrast . . . was to telescope into a relatively limited crustal depth zone the transition between the ultrametamorphic (syntectic) and the eruptive (rheomorphic) 'aspects' of the post-Karagwe-Ankolean Granite."

"The Petrology of Two Granite-Slate Contacts at Cape Town, South Africa" is described by F. Walker and M. Mathias (*Quart. Journ. Geol. Soc.*, CII, Pt. 4, 1947, 499-521). These are famous sections once seen and described by Charles Darwin, among others. The granite is intrusive into the tightly folded pre-Devonian Malmesbury Series which consists of psammitic hornfelses with

subordinate pelites. At two localities the contact aureole is well exposed, and there is a spectacular zone of migmatites. Fourteen new chemical analyses are used to show that the contact phenomena probably occurred in four main stages:—" 1. Intrusion and differentiation of a contaminated biotite-granite magma. 2. Permeation and softening of the Malmesbury hornfels by alkaline emanations from the granite. 3. Penetration of the hornfels by granitic material with the formation of migmatites. 4. Feldspathization of the granite and migmatite zone by late potassic solutions which gave rise to potash pegmatites and large crystals of perthitic microcline.

"The evidence for the formation of an Mg-Fe front outside the migmatite zone is inconclusive."

The theme of R. H. Rastall's short paper on "The Significance of Variation in Granites" (*Geol. Mag.*, LXXXIV, 1947, 29-32) is that there are granites—and granites. Granites proper and granodiorites show wide variations in their mineralogy and chemistry, their minor elements, accessory minerals, and in the accompanying pneumatolysis and ore-deposits. Highly alkaline granites associated with alkali-syenites and basic types, for example, are better explained by differentiation than by "granitisation." The Hercynian granites (Cornwall, Saxony, etc.) carry tin and tungsten; but Caledonian granites, not greatly different in their mineralogy and chemistry, contain no Sn or W, and indeed are extremely poor in metalliferous ores. In Dr. Rastall's view the phenomena of ore concentration in and by granites are most easily explained by some theory of magmatic differentiation.

S. R. Nockolds has studied "The Order of Crystallisation of the Minerals in some Caledonian Plutonic and Hypabyssal Rocks" (*Geol. Mag.*, LXXXIII, 1946, 206-17). He shows that the associated dikes and chilled margins of the plutonic masses often provide much less ambiguous evidence of the order of crystallisation than the plutonic rocks themselves. The ascertained general order is, olivine, rhombic pyroxene, augite, hornblende, biotite, plagioclase, quartz and potash feldspar, although there is a good deal of overlapping, especially in the later minerals. The close correlation between the order of intrusion of the main rock types and the order of crystallisation suggests that a process of differentiation involving a crystal-liquid equilibrium has been operative. The final products at the high-silica end of the variation diagram—aplites and related rocks—should fall on the plagioclase-potash feldspar-quartz cotectic curve, as in fact they are shown to do.

Following the main lines of the previous paper Dr. Nockolds

has studied 63 analyses of aplites, aplitic granites and related rocks, other than those of the Caledonian suite, to ascertain whether these, like the Caledonian, fall on or near the plagioclase-potash feldspar-quartz cotectic curve ("The Granite Cotectic Curve," *Geol. Mag.*, LXXXIV, 1947, 19-28). Twenty-five of the analyses do actually fall on or near the ternary cotectic curve, or on the orthoclase-quartz boundary surface. This is what might be expected if the aplites represent residual liquids derived from more basic granitic or granodioritic magmas. The early crystallisation of muscovite, however, causes a departure from the normal line of descent, such that a special type of potash-rich aplite is generated, with a storage of silica in the residual liquid. The muscovite-granites with which these aplites are associated are consequently richer in quartz than other granites.

"The Geology of the Foyers 'Granite' and the Surrounding Country" is dealt with by Miss D. C. Mould (*Geol. Mag.*, LXXXIII, 1946, 249-65). After a little early appinite the order of intrusion was tonalite, granodiorite, granite. The basic member is found only as sparse xenoliths and as small satellitic intrusions. Xenoliths are very abundant in the tonalite and granodiorite but are absent from the biotite-granite. They consist of basic diorites and Moine Schists. Some of the schist inclusions are 12 inches in thickness and extend as thin sheets for upwards of half a mile.

The Moine Series and the Middle Old Red Sandstone are also dealt with, as also a problematic outcrop of Lewisian. The schists are contact metamorphosed with the development of sillimanite, cordierite and andalusite. There is no discussion of petrogenesis, but the fact that the word "granite" is used with inverted commas is eloquent of the sympathies of the author.

"Late Sillimanite in the Migmatites of Kildonan, Sutherland" is described by Janet Wilson (*Geol. Mag.*, LXXXV, 1948, 149-62). The Sutherland Injection Complex near Kildonan consists of high-grade migmatites in which sillimanite is present in many of the pelitic and semi-pelitic rocks. The mineral also occurs in pegmatitic and aplitic veins. The sillimanite seems to have no direct connection with the conditions of regional metamorphism. The author presents evidence to show that it was formed by pegmatitic solutions at a late stage of the migmatisation process and, although occurring in metamorphic rocks, was a metasomatic product of granitisation. Pneumatolytic sillimanite has been described from many other localities. Hence it is undesirable in many occurrences to use the mineral as an index of the grade of regional metamorphism.

A paper by Hsing-Yuan Ma "On the Occurrence of Agmatite

in the Rogart Migmatite Area: a Study in Granitization" (*Geol. Mag.*, LXXXV, 1948, 1-18) discusses two types of agmatite (= migmatites resembling eruptive or intrusion breccias—Sederholm); one developed from a fine-grained biotite-hornblende-schist, the other from a basic schist of igneous origin. The "transformist" conclusion is arrived at that both types "were formed *in situ* from solid rock as a result of recrystallisation depending on the introduction, diffusion, fixation and expulsion of migrating chemical elements, probably in an ionic state." A "basic front" is well developed in the second type. The complete sequence of recognisable changes is probably: (1) basic schist; (2) hornblende-biotite-pyroxene-schist; (3) hornblendite; (4) appinitic types; (5) migmatite; (6) granodiorite. The puzzling "Ach'uaine hybrids" of Sutherland are probably best explained along the above lines.

"The Broadlaw 'Granite,'" described by Miss D. C. P. Mould (*Geol. Mag.*, LXXXIV, 1947, 178-81), is one of the smaller Caledonian granite masses which cut the Older Palaeozoic sediments of the Southern Uplands of Scotland. It is elongated parallel to the strike of the sediments and its maximum length is less than half a mile. The outermost and probably earliest member is a hornblende-biotite-quartz-diorite, followed towards the interior of the mass by granodiorite mainly grey in colour, which is cut by a red granodiorite of somewhat more acid composition. Small xenoliths are found in all three members, and some of them contain corundum and sillimanite.

"The Granites of South-West Jersey," described by F. A. Henson (*Geol. Mag.*, LXXXIV, 1947, 273-80), consist essentially of a large boss intruded into Pre-Cambrian shales. This was followed by a second granite which appears to have occupied the approximate centre of the earlier intrusion. Where a portion of the original shale roof is preserved, a finer-grained and lighter type of granite has been produced by the contamination of the magma with shale. The two granites are very similar, but the later intrusion shows more abundant deformation effects than the earlier. It thus probably coincided with a culmination of orogeny.

R. Merriam describes "Orbicular Structures in Aplite Dikes near Ramona, California" (*Amer. Journ. Sci.*, 246, 1948, 129-37). The spheroids are sparsely and irregularly distributed. They average 7 inches in diameter, and consist of an aplite core surrounded by a tourmaline-bearing zone, followed by an outer leucocratic zone. They may be partly digested xenoliths, and are believed to have existed as spheroids before the crystallisation of the dikes had been completed.

M. K. Wells has made a notable study of pneumatolysis in "A Contribution to the Study of Luxullianite" (*Min. Mag.*, XXVII, 1946, 186-94). The rock is regarded as the product of the arrested pneumatolytic modification, by boric emanations, of a porphyritic alkali-granite of normal West of England type. The three essential minerals are microperthitic feldspar, tourmaline and quartz, with accessory apatite, topaz (?) and zircon. Of the original minerals, the feldspars and the yellow tourmaline of the first generation proved unstable under the attack of the borofluoric flux, and were replaced by green acicular tourmaline.

J. S. Webb describes "A Replacement 'Pegmatite' Vein in the Carn Brea Granite" (*Geol. Mag.*, LXXXIII, 1946, 177-86) as a vein-like body of quartz, but with no evidence of a primary fissure. It was formed by the metasomatic replacement of the granite by a siliceous solution travelling along a zone of minute fissures. Replacement was not complete; residual feldspars are left bridging the vein, and the vein then resembles a pegmatite dike in form and mineral composition. Minor silicification and quite extensive greisenisation has taken place in the adjacent country rock. Later movements at the period of lode formation have given rise to chlorite-tourmaline veinlets.

The same author also discusses "The Genesis of some Micro-Veinlets in Cornish Granite-Porphyry" (*Geol. Mag.*, LXXXIV, 1947, 65-78). Some veinlets intersect quartz, feldspar and biotite phenocrysts, although the evidence shows that the phenocrysts have not been pulled apart along the vein zone, but have been strained and minutely fractured. The solutions which penetrated these zones have had different effects on the various phenocrysts. Feldspar veinlets also occur (potash-feldspar) and these, likewise, originated partly by replacement, and not simply by solutions filling preformed fissures. The feldspar veinlets came first, with only minor separation of the walls of the fissures; the later quartz veinlets occupied zones of submicroscopic fractures without separation of the walls of the veins.

A notable addition to the literature of pegmatites is the memoir on "The Pegmatites of Central Nigeria," by R. Jacobson and J. S. Webb (*Geol. Surv. Nigeria, Bull. No. 17*, 1946, 61 pp.). The pegmatites carry columbite-tantalite and cassiterite deposits, and are genetically related to a phase of the Older Granites of the region. The authors believe the pegmatites originated from a residual "granitic" magma rich in hyperfusible constituents which suffered continuous differentiation by fractional crystallisation. This involved a progressive enrichment of the solutions in soda,

silica, and the volatile constituents with decreasing temperature, the process taking place essentially in moving solutions. Precipitation of solid phases depended on the physico-chemical conditions that prevailed, in particular, a steeply declining temperature gradient, and insufficient energy or an uncongenial environment in which to effect metasomatism.

BOTANY. By Professor W. H. PEARSELL, D.Sc., F.R.S., University College, London.

GROWTH SUBSTANCES.—The numerous investigations into the role of growth regulators indicates that interest in this field shows no sign of abating. Recent work aims particularly at throwing more light upon the mechanism by which these substances may affect plant growth. The types of analysis differ in different instances. They range from those which attempt to analyse the general growth responses to those which attempt to determine the effect of substances on cell structure or cell metabolism. Amongst the former must be placed the work of L. J. Audus and J. H. Quastel (*Ann. Bot.*, 12, 27, 1948), who have used sulphonamide derivatives of aniline, pyridine, thiazole and guanadine, which have been shown to inhibit root growth of Cress at concentrations as low as 10 parts per million. In contrast, they also use *p*-aminobenzoic acid which antagonises the inhibitory action of sulphonamide, neutralisation being complete when the ratio of *p*-aminobenzoic acid/sulphonamide approaches unity. *p*-aminobenzoic acid also shows slight antagonism to the growth inhibitory action of certain other plant growth substances of the phenoxyacetic and indoleacetic acid types, but very high ratios of *p*-aminobenzoic acid to growth substances must be attained, before a marked effect is given. It is concluded from these ratios that the phenomenon of root-growth inhibition by sulphonamide is quite distinct from that of sulphonamide bacteriostasis and also from that of root-growth inhibition by plant growth regulators.

Work of a somewhat different type has been carried out by J. D. Ward and J. W. M. Roodenburg (*Kon. Nederland. Wetensch.*, 51, 2, 1948). They assumed that a plant raised from seed is not able to initiate flower-buds before having completed its juvenile or vegetative state in which a minimum number of leaves must have been developed. According to O. N. Purvis and F. G. Gregory (*Ann. Bot.*, 51, 919, 1937), this minimum for winter rye is seven leaves, for *Kalanchoe* five leaves, while for the tomato it is stated that eight leaves are necessary. J. W. M. Roodenburg (*Meddel. Direct. Tuinbou.*, p. 296, 1947) states that it is only after the

completion of the juvenile stage that external influences, if favourable are able to induce the formation of the first flower buds.

In some experiments carried out by Ward and Roodenburg, just germinated seedlings of tomato were sprayed with a solution of tri-iodobenzoic acid, 200 mg./litre. At this stage only the cotyledons are visible, but there are two or three small leaf primordia. The plants were raised in a hot-house, with a high air humidity, in order to prevent the early formation of flower buds, and such plants normally produced fifteen or nineteen leaf primordia without any sign of flower initiation. On the other hand, the treated seedlings showed a characteristic elongation of the uppermost internode, while the actual growing point divided rapidly to form a large number of flower initials, even though the normal minimum of eight vegetative leaves had not been produced. It was not found possible to obtain real flowers from these buds, so that the effect of the treatment is to induce the process of flower bud initiation, but not to affect the phenomenon of flowering. This work has thus confirmed the earlier observations of P. W. Zimmerman and A. E. Hitchcock (*Contr. Boyce Thompson Inst.*, **12**, 491, 1942), that tri-iodobenzoic acid induces abnormal flower formation and at the present this is the only synthetic substance which is known to have this effect, although hypothetical flower hormones with various attractive names have long figured in the literature. It may be noted also that A. W. Galstone (*Amer. J. Bot.*, **34**, 356, 1947) found that an increase in the number of flowers in short-day plants was caused by treatment with tri-iodobenzoic acid.

In the Polish Review of Agricultural Research (*Przegląd Doswiadozalnictwa Rolniczego*, **3**, 193, 1947) M. Falkowski describes a field experiment in which the effect of synthetic growth substances on rye and potato was studied. The seeds or the tubers were treated immediately before sowing, rye with the potassium salt of α -naphthylacetic acid, potato with the potassium salt of β -indolylbutyric acid. The crops thus treated showed no marked reduction in amount and indeed exhibited no checking effect due to the administration of the synthetic growth substances. There was a tendency, however, for the starch grains to be a little larger as a result of the treatment, and it is, therefore, claimed that the synthetic growth substances named increase the size of the starch grains in these plants, just as heteroauxin or ethylene are stated to do.

The effect of substances of the auxin type upon the growth of isolated cells has also received attention by J. R. Roborg and J. B. Thomas (*Kon. Nederland. Akad. Wetensch.*, **51**, 87, 1948). They have grown the alga *Chlorella* in pure culture in solutions con-

taining glucose, illuminated in a shaking apparatus, and have observed the effect of various low concentrations of heteroauxin on the cell size and cell number. As a result of a detailed analysis of growth in these cultures, they interpreted their data as showing that heteroauxin accelerates cell enlargement in low concentrations, so that individual cells reach their full-grown state more quickly and thus they become capable of dividing more quickly. The increase of cell multiplication observed by some authors may, therefore, be regarded as a secondary effect of heteroauxin. In higher concentrations, above 5 parts per million, a definite and general retardation of growth is observed.

The authors also found that there was evidence that growth substances of this type are produced by the alga and can be extracted from it by appropriate methods, although no such material was observed under good growth conditions to diffuse from the cells into the medium. The substances produced were capable, when asymmetrically applied, of producing curvatures in *Avena* coleoptiles, and so presumably are of the general auxin type. The maximum rate of production occurs during the periods of maximum growth rate, either in light or in darkness, light not being essential to the production of growth substances. This is an interesting paper, because if it is confirmed it will reconcile the very varied views that have been expressed as to the effect of auxins on the growth of a unicellular alga.

A very useful summary of the use of chemical substances and "antibiotics" in the control of weeds is contained in a lecture by G. E. Blackman (*J. Roy. Hort. Soc.*, 73, 134, 1948). This ranges from the early use of copper sulphate and sulphuric acid to the more modern employment of derivatives of phenoxyacetic acid. It is, of course, mainly concerned with the practical aspects of the subject. Papers of greater scientific interest are the two studies by W. G. Templeman and W. A. Sexton on the differential effects of synthetic growth substances and other compounds upon different species of plants (*Proc. Roy. Soc. B.*, 133, 300 and 480, 1946). In these papers it has been demonstrated that certain low concentrations of a number of synthetic growth substances prevented germination or restricted seedling growth of some plant species, while producing no effect on others. The first of these papers is concerned mainly with substances which are known to produce an effect by altering the extension growth of cells, mainly derivatives of phenoxyacetic acid and of naphthoxyacetic acid. These are much more effective on dicotyledons, for example charlock (*Sinapis arvensis*), yarrow (*Achillea millefolia*), plantain (*Plantago major*), corn buttercup

(*Ranunculus arvensis*), fat hen (*Chenopodium album*), corn marigold (*Chrysanthemum segetum*), corn spurrey (*Spergula arvensis*) and field poppy (*Papaver rhoeas*), in contrast to the cereals which are less affected. The cereal which was most often investigated was oats, but wheat, barley and rye were also used.

In the second paper the effects of substances known to interfere with nuclear division in plants have been investigated. Colchicine is perhaps the best-known substance of this type, though others like chloral hydrate and acenaphthene have also been recognised. Templeman and Sexton have followed up the known effects of phenylurethane by investigating the action of a great number of arylcarbamic esters, mainly derivatives of phenyl carbamates. These materials markedly reduce the germination growth of cereals in very low concentrations, while similar concentrations have apparently little or no effect upon the dicotyledonous plants which have been tested. These include yellow charlock, mangold, sugar beet and potato. It seems reasonable to suppose that the effects of these carbamates may be explained by an earlier observation by Lefevre, that phenylurethane (ethyl phenylcarbamate) interferes with mitosis in wheat seedlings, and that this conclusion may be extended to these other substances. It remains to be seen whether all the compounds which give these morphological effects in restricting growth are also active in inducing polyploidy.

Growth regulators may produce their effects by methods other than those which have been already outlined above. One of the most interesting of recent attempts to investigate such problems is contained in two papers by M. Staey and W. Webb (*Proc. Roy. Soc. B*, 134, 523 and 538, 1947). These authors have investigated the antibacterial properties of the bile acids, which in relatively high concentrations are truly bactericidal, while in more dilute solutions they inhibit growth of organisms. The investigation of a wide range of derivatives of cholane, with bacteria susceptible to their action, has suggested that a simple relationship exists between the bacteriostatic activities of the compounds and their ability to depress the surface tension of the metabolic medium. The limiting dilution at which the "active" bile acids were bacteriostatic for *Staphylococcus aureus* corresponds to a surface tension of the medium of approximately 4.5 dynes. Similar investigations of the antibacterial properties of the basic derivatives of cholane and norcholane showed that they were, in contrast to those already considered, more bacteriostatic against the Gram-positive organisms than against the Gram-negative. With these, no relationship was apparent between the lowering of the surface tension induced by the

compounds and their bacteriostatic activity. This line of enquiry is an interesting return to the ideas which were put forward nearly forty years ago, by Czapek, and at that time very critically received.

WATER RELATIONS.—The old question of the structure and properties of sun and shade leaves has been raised again by G. Lémée (*Rev. Gen. Bot.*, 54, 105, 1947), in relation to the leaf structure and water economy of the wood sage, *Teucrium scorodonia*. This is a plant living at the margins of woods and one which occurs in a great range of light conditions. In this paper the comparison is made between the structure and form of the leaves in the sun and in the shade. In addition to the structural features, data are given for the diurnal march of transpiration of the two types of leaves in relation to leaf-area and to leaf-weight, and the degree of opening and closing of stomata and the number of stomata are also given in full. Interesting comparisons are made between the rate of water loss under different conditions. In shade, the sun form transpires more than the shade form under equal conditions of humidity. When the leaves are saturated with water, the sun leaf transpires 2.2 times as much water from an equal fresh weight and 3.3 times as much from an equal surface. There is much less difference when both leaves are exposed to full light, when the sun leaves lose more, 1.3 times by weight, 1.9 times by surface. Analysis of the possible causes of these differences leads the author to suggest that they are due to a slightly increased cuticular transpiration, to a considerably greater density of stomata and finally to the very much wider opening of the individual stomata. The author points out that there must be, in addition to these, a difference in an internal physiological state which may also affect the responses of the sun leaves to water loss.

A new method of approach to the problems of water relations in leaves has been developed by G. E. Fogg (*Proc. Roy. Soc. B*, 134, 503, 1947), who has studied the wetting of leaves by water, using the contact angle as a measure of the ease with which wetting takes place. The contact angle is the angle made by the leaf plane with the inner surface of a drop of water on the surface of the leaf. The magnitude of the contact angle of water on a leaf surface has been found to vary with the position of the leaf on the plant, and it varies very greatly for different species, being as low as 50–70° in charlock (*Brassica sinapis* = *Sinapis arvensis*) and as high as 120–140° in flax (*Linum usitatissimum*). In charlock the contact angle is found to be greater on the lower surface of the leaf.

The angle also showed, however, a characteristic diurnal fluctuation, with a range which may be as much as 30°, and these diurnal

variations are shown to be correlated with changes of leaf water content. In general, the angle increases as evaporation rises during the day, and it varies markedly as wilting proceeds in detached leaves, this range being reversible on recovery of turgor. Evidence is presented to show that these variations in the behaviour of water on leaves are caused by changes in the degree of corrugation of the leaf surface, produced by changes in the water content of the tissue and in the resultant tensions.

PLANT PHYSIOLOGY. By PROFESSOR WALTER STILES, Sc.D., F.R.S.,
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SALT RELATIONS OF PLANT CELLS.—By the end of the first quarter of the present century it had become clear that the absorption of salts by plant cells could not be explained in terms of the simple diffusion of the salts through the cell membranes into the cell interior, for this absorption was characterised by (1) the unequal absorption of the two ions of a salt and (2) the absorption of the ions from dilute solutions far beyond the point necessary to establish equality of concentration between the external solution and the liquid in the cell vacuole. During the next ten years two other main facts emerged, the first being that this excess absorption of ions, or accumulation as it is now generally called, was connected in some way with aerobic respiration; the second, Lundegårdh's discovery that respiration by the roots of wheat seedlings was increased by the presence of a salt and that the excess respiration was related to the amount of anion of the salt absorbed rather than to the absorption of cation. The more recent work considered in this article carries on the story from that point.

Much work on the salt relations of plant cells has been carried out with storage tissues such as potato tuber and the roots of carrot and beet in the form of thin slices. Using discs of carrot root 1 mm. thick, W. Stiles and A. D. Skelding began a systematic investigation of the absorption of salts by this tissue. In 1940 the results of experiments with potassium and manganese salts were published. ("The Salt Relations of Plant Tissues, I. The Absorption of Potassium Salts by Storage Tissue," *Ann. Bot., N.S.*, 4, 329-64, 1940; "The Salt Relations of Plant Tissues, II. The Absorption of Manganese Salts by Storage Tissue," *Ann. Bot., N.S.*, 4, 673-700, 1940.) These experiments, while confirming previous work on the inequality of absorption of the two ions of a salt, showed that this inequality was most marked during the early stages in absorption. In fact, two phases in absorption could be distinguished, the first lasting for a few hours, in which one ion might be absorbed to a

greater extent than the other, the second in which absorption of both ions continued far beyond the point required to bring about equality of concentration inside and outside the tissue. During the first phase either one ion or both ions of a salt, if these were initially present in the tissue, might diffuse out from the cells into the external solution if this were sufficiently dilute; thus exosmosis of both ions was observed with 0.0002 M solutions of potassium chloride, while an initial exosmosis of potassium was commonly observed into solutions of 0.001 M potassium chloride, potassium sulphate and potassium dihydrogen phosphate. In the second phase this exosmosis always gave place to absorption.

Although a two-phase course in the absorption of salts has been called in question, later work by W. Stiles and K. W. Dent leaves no doubt that these two phases do actually occur ("The Salt Relations of Plant Tissues, III. Further Observations on the Absorption of Manganese Chloride by Storage Tissue," *Ann. Bot.*, N.S., 10, 203-22, 1946), and that their existence is related to the preliminary treatment of the tissue before being brought into contact with a salt solution. In this work, in which thin slices of the roots of swede, mangold and garden beet were used as well as carrot, it was found that when the slices of tissue were washed in running tap water for about 24 hours before transference to a 0.001 M solution of manganese chloride there was initially a rapid absorption of the manganese ion but very little absorption, or even exosmosis, of the chlorine ion. This phase lasted only a few hours, the rate of cation absorption falling off strikingly. This phase passed over into the second phase, when both manganese and chloride were continuously absorbed until a large proportion of the salt originally present in the external solution had been absorbed. It was found that if the period of preliminary washing in running tap water was prolonged the second phase of absorption occurred correspondingly sooner after transferring the tissue to the salt solution. Thus in experiments with garden beet, in which the tissue was washed for one day in running tap water, the second phase of absorption only became obvious about two days after the tissue was placed in the solution, but if the preliminary washing period was continued for four days the second phase of absorption had obviously started after one day in the solution, while with preliminary washing for seven days no distinction between two phases in absorption could be recognised, the second phase in absorption beginning immediately. These results with long preliminary washing are very similar to those obtained by F. C. Steward and J. A. Harrison on the absorption of rubidium bromide by thin slices of potato tuber

("The Absorption and Accumulation of Salts by Living Plant Cells, IX. The Absorption of Rubidium Bromide by Potato Discs," *Ann. Bot., N.S.*, 3, 427-53, 1939).

It seems clear that the two phases in salt absorption correspond with what Steward called induced absorption and primary salt absorption. The first phase results from an exchange of ions between tissue and external medium, which will occur as a result of any change in the ionic composition of the external solution and is quite possibly conditioned by Donnan equilibrium. The second phase is related to the metabolic activity of the tissue, of which respiration intensity is a measure. The lag period between immersion of the tissue in the solution and the development of the second phase in absorption is explained by observations of W. Stiles and K. W. Dent on the respiration of thin tissue slices ("Researches on Plant Respiration, VI. The Respiration in Air and in Nitrogen of Thin Slices of Storage Tissue," *Ann. Bot., N.S.*, 11, 1-34, 1947). Immediately after tissue slices are cut from a storage organ, such as a beetroot, their respiratory activity is very low, but after transference to running tap water the activity progressively increases for days. Such behaviour had previously been observed by T. A. Bennet-Clark and D. Bexon ("Water Relations of Plant Cells, III. The Respiration of Plasmolysed Tissues," *New Phyt.*, 42, 65-92, 1943). We are not concerned here with the explanation of this rise in respiratory activity, but with its occurrence, for the period during which the respiratory activity of the tissue slices is increasing from its initially low value corresponds with the development of a higher rate of metabolic activity in the tissue.

The work of S. C. Brooks ("Ion Exchanges in Accumulation and Loss of Certain Ions by the Living Protoplasm of *Nitella*," *J. Cell. & Comp. Physiol.*, 14, 383-401, 1939) on *Nitella coronata* and *N. clavata* has shown that in these large-celled algae also two phases in salt absorption are recognisable, the first being the result of ionic exchange, the second that described as primary accumulation and probably related to metabolic activity, although here the behaviour of the cells appears to be more complex, for during the second phase periods of absorption appeared to alternate with periods of exosmosis.

Much work on salt absorption by wheat seedlings has been carried out in the past by H. Lundegårdh and H. Burström as a result of which they propounded their well-known theory of anion respiration. In a recent publication by the former worker ("Absorption, Transport and Exudation of Inorganic Ions by the Roots," *Arch. f. Bot.*, 32A, No. 12, 139 pp., 1945) further data on this and

allied subjects are presented. It is made clear that with this material also two distinct processes are involved in salt absorption, an interchange of ions determined by the physical laws of mass action and membrane equilibrium and an active absorption of ions which may proceed against the concentration gradient and which is related to metabolic activity. While the cations are chiefly involved in the process of ionic exchange, both ions of a salt are concerned in the process of active absorption. Since it is this latter process which is related to metabolic activity and hence to respiration, and since anions are generally only slightly concerned in the process of ionic interchange, it is understandable why the increase in respiration related to the absorption of salt bears a close relationship to the absorption of anion, but not to that of the cation, since the latter is also largely involved in the process of ionic interchange which is unrelated to metabolic activity.

This relation between salt accumulation and respiration has formed the subject of a number of papers by R. N. Robertson and his collaborators. In the first of these Robertson ("Studies in the Metabolism of Plant Cells, 1. Accumulation of Chloride by Plant Cells and its Relation to Respiration," *Austral. J. Exp. Biol. and Med. Sci.*, 19, 265-78, 1941) determined the accumulation of the chlorides of potassium, sodium, lithium, calcium and magnesium, and at the same time determined the respiratory carbon dioxide evolved. With the monovalent chlorides in concentrations ranging initially from 0.0044 M to 0.0108 M the presence of the salt brought about an increase in respiration rate above that of control tissue kept in distilled water while at the same time salt accumulated in the tissue. However, whereas the increased respiratory activity remained practically constant for 100 hours or more the rate of salt absorption fell off with time. With the divalent chlorides, however, the respiratory activity, although initially higher than that of the control tissue in distilled water, subsequently fell to that of the control, or even lower. The excess respiration resulting from the presence of the salt Robinson calls "salt respiration"; it is thus the same quantity as Lundegårdh's "anion respiration." In relating the accumulation of salt to the salt respiration Robinson decided that with potassium chloride, at any rate, the salt respiration per unit of salt accumulated was approximately the same in all experiments. Robinson also concluded that the rate of accumulation tends to become independent of the external concentration of the salt, at least in higher concentrations of salt, and to explain this he suggested that in an outer phase of the cell the concentration of salt remains constant, as by its adsorption to a constant concentra-

tion, and that from this outer phase salt is removed to an interior phase by an energy-expendng process.

In a second paper ("Studies in the Metabolism of Plant Cells, 2. Effects of Temperature on Accumulation of Potassium Chloride and on Respiration," *Austral. J. Exp. Biol. and Med. Sci.*, **22**, 237-45, 1944), Robinson determined the total respiration, salt respiration and salt accumulation by thin slices of carrot tissue immersed in a 0.01 M solution of potassium chloride at four temperatures, 7°, 13°, 19° and 25° C. After the phase of ionic exchange, which appeared to take place in about ten minutes and which appeared to be practically independent of the temperature, the phase of salt accumulation followed, the rate of accumulation falling off with time. Salt accumulation, salt respiration and ground respiration were all found to have high temperature coefficients, the average values of the coefficients for these three processes being respectively 2.1, 2.9 and 3.0. Robinson considers that the experimental data are consistent with his earlier expressed view that there is a region of the cell in which salt is maintained at a constant concentration and that it is accumulated from this region by a process deriving its energy from respiration.

In the third paper of the series, R. N. Robertson and J. S. Turner ("Studies in the Metabolism of Plant Cells, 3. The Effects of Cyanide on the Accumulation of Potassium Chloride and on Respiration; the Nature of the Salt Respiration," *Austral. J. Exp. Biol. and Med. Sci.*, **23**, 63-73, 1945) show that, although the ground respiration is not affected by cyanide, both the salt respiration and salt accumulation are inhibited by cyanide. As is to be expected, the first phase of ionic interchange is not affected by cyanide. This result confirms Lundegårdh's theory of a cyanide-sensitive respiratory process on which salt accumulation depends.

In a further contribution Robertson in collaboration with M. Thorn has shown that the additional respiration brought about by the presence of salt ceases as a result of the removal of salt ("Studies in the Metabolism of Plant Cells, 4. The Reversibility of the Salt Respiration," *Austral. J. Exp. Biol. and Med. Sci.*, **23**, 305-9, 1945). Thin discs of carrot tissue were again used, and the salt employed was potassium chloride in an initial concentration of 0.015 M. The salt respiration rate remained constant for forty hours, after which the salt solution was replaced by distilled water, with the result that the respiration rate slowly fell to a steady rate slightly above that of a control in distilled water, this taking many hours. On return to a salt solution the respiration rate again rose and salt accumulation was resumed. A similar sequence of events was

observed with tissues kept in a solution of potassium chloride in an initial concentration of 0.01 M. The accumulated salt is not lost from the tissue when this is transferred from salt to water. It would thus appear that salt respiration is reversible, but the fall in respiration rate to the ground respiration level takes about 60 times as long as the rise in respiration rate on application of the salt. In a fifth publication Robertson in conjunction with J. S. Turner and M. J. Wilkins ("Studies in the Metabolism of Plant Cells, 5. Salt Respiration and Accumulation in Red Beet Tissue," *Austral. J. Exp. Biol. and Med. Sci.*, **25**, 1-8, 1947) has extended his observations to red beetroot tissue and has found that beetroot behaves very similarly to carrot root, both as regards the relation between salt respiration and salt accumulation and the inhibition of these processes by cyanide.

In two papers, one in which potato tuber was used, in the other of which leaves of *Sambucus nigra* and *Parthenocissus quinquefolia* were the subjects of experimentation, W. W. Lepeschkir has recorded some observations on the exosmosis and accumulation of salts ("Einfluss des Lichts auf Exosmose und Speicherung von Salzern im Kartoffelknollengewebe," *Protoplasma*, **34**, 55-69, 1940; "Influence of Temperature and Light upon the Exosmosis and Accumulation of Salts in Leaves," *Amer. J. Bot.*, **35**, 254-9, 1948). For example, leaflets were floated on a 0.006-0.0009 M solution of calcium chloride with the upper non-stomatal surface downwards and the change in electrical conductivity determined. A weak exosmosis of electrolytes from the leaflet to the solution was observed, this decreasing with increase in temperature from 3° to 45° C. This result is attributed to temperature increasing accumulation of the salt by the cells more than it increases exosmosis from the tissue. Exosmosis of salt was found to increase in daylight and decrease at night. Covering the upper stomatal surface of the leaves with petroleum jelly, which by blocking the path of gaseous exchange would reduce respiratory activity, brought about an increase in the rate of exosmosis. This was attributed to a reduction in the rate of salt accumulation consequent on the reduction of respiratory activity.

ENTOMOLOGY. By A. D. LEES, M.A., Ph.D., Agricultural Research Council, Unit of Insect Physiology, Cambridge.

THE HISTORICAL ASPECT OF INSECT DISTRIBUTION.—In spite of its significance to the ecologist, the modern taxonomist and to the student of evolution, this field remains surprisingly neglected. No doubt this is due to the poverty of the insect fossil record. Nevertheless, even though speculation cannot altogether be excluded, as

most writers on this subject admit, present methods seem to provide a basis for a more positive attitude in the future. Some examples of recent work are quoted below.

It is well known that the composition of the present European fauna has been profoundly influenced by climatic and geographical changes during the Pleistocene period. B. P. Beirne (*Trans. R. ent. Soc. Lond.*, 1947, 98, 273) has recently attempted to arrive at a logical explanation of the present composition and distribution of the British Macro-Lepidoptera in terms of geological history. The evidence brought forward is of three kinds: ecological, taxonomic and zoogeographical. The first traces the connection between present feeding habits and the probable past distribution of the same host plants. The second is used as a rough indicator of antiquity, the existence of well-defined subspecies pointing to long isolation. Most reliance is placed on the third line of evidence, in which present range and distribution is related to known geographical and climatic changes in the past.

Of the 809 resident species of British Macro-Lepidoptera, a few have been introduced by human agency, while about 80 are classified as known or suspected-migrants. The occurrence of subspecies or the possession of feeble powers of migration suggest, however, that the majority have been in this country for a long period. There is no reliable evidence that any lepidopteron has survived from Pliocene times and survivors from the warm third interglacial period were probably few. Present distribution is regarded as being largely determined by the course of the last or Upper Pleistocene glaciation. This period embraces three glacial phases and two milder interstadial phases corresponding to the retreat of the ice front.

The first and most extensive glacial phase saw the entry from the south of several boreal and alpine species, of which the Mountain Ringlet, *Erebia epiphron*, is a well-known example. One species at least, *Xylophasia exilis*, arrived over land connections from the north-west, for this moth now inhabits the tundra areas of Labrador and Spitzbergen and the mountain bogs of Scotland, but is absent from continental Europe.

The long and temperate first interstadial phase marked the arrival of many southern species. It is argued that those which escaped extinction during the second glacial phase may have done so by developing adaptations which involved the adoption of changed habitats and often changed food preferences. Some species adopted a maritime habitat, and survived in the more oceanic climate of these regions. An example is *Aporophyla australis*, which has a coastal distribution in England, although in southern Europe it is

not exclusively confined to a maritime habitat. Marsh and fenland species, like *Porthetria dispar*, which in England has the status of a rare fen insect, fall into a similar category. These are possibly the remnants of a larger population once inhabiting the low-lying, and now submerged, "Dogger Land." Some heath-dwelling Lepidoptera, such as *Cleoria cinctaria*, which feeds on heather and birch in England but on *Hypericum* and *Artemisia* on the continent, are cited as further examples.

Fewer species are listed as possible arrivals during the second and third glacial, and the short second interstadial phases. But at least half the remaining British species are considered to be post-glacial immigrants which arrived before the continental land connection was severed.

The same lines of argument are followed in a short paper dealing with the origins of some other British insects (B. P. Beirne, *Proc. R. ent. Soc. Lond. A.*, 1948, 23, 1). Regarding the post-glacial arrivals, a point of interest is that further spreading northwards and westwards may have been limited by the prevailing cool and humid conditions rather than by physical barriers. Many of those species which failed to reach Scotland also, therefore, failed to spread to Ireland. Evidence is provided by the fact that sun-loving aculeate Hymenoptera, Lepidoptera and Hemiptera are now very poorly represented in Ireland, whereas aquatic insects, which might be expected to have been less influenced by climatic conditions, are well represented. Extreme examples are the sun-loving Chrysididae (only 12 per cent. of the English species are recorded in Ireland) and the wholly aquatic Corixidae (81 per cent.).

An ambitious and stimulating treatment of faunal movements is provided by R. Jeannel in his book entitled *La Genèse des Faunes Terrestres* (1942, Presses Universitaires de France, Paris). In this work the author discusses the origins of the land fauna in terms of Continental Drift, as conceived in Wegener's maps. Although doubt has been cast on Wegener's views, particularly where the mechanisms of Continental Drift are concerned, the theory has appealed to many biologists as providing the only reasonable explanation of certain facts of distribution. In Jeannel's book, although other groups of animals and plants are mentioned, the theme is illustrated mainly by references to the Coleoptera, and particularly to two groups with a world-wide distribution, namely the Trechitæ, a family of small ground beetles, and the Catopidae, a family of saprophagous Staphylinidae. The arguments here rest upon the affinities of living forms, as shown by comparative morphology, and present geographical distribution.

The material, which is arranged historically, is used to trace post-Permian lines of migration and evolution on the Laurentian and Gondwanian land-masses; to describe the subsequent population of the Eurasian continent from Gondwanaland and the radiation of faunal elements throughout the holarctic region during the Tertiary. Many examples which illustrate the Wegener concept of the fragmentation of Gondwanaland are enumerated. To give but two: closely related Carabids of the family Hiletidæ occur in Brazil and West Africa. This is not unexpected if the separation of South America and Africa by the Atlantic is a comparatively recent event. Similarly, the presence of allied Nemaditæ (Catopidæ) in South America and Australia can be explained in terms of the southern continental land connection which persisted long after the widening of the Indian Ocean during the Jurassic.

The most recent detailed investigation of the radiation of a single group of insects is probably that of F. E. Zeuner (*Trans. Zool. Soc.*, 1943, 25, 107) on the evolution of *Troides* and its allies, a group of Papilionid butterflies distributed throughout the Australasian Archipelago. The primary object in this "experiment in 'palæontology without fossils'" was not to investigate faunal changes but to reconstruct the phylogeny of a rapidly evolving group of related species. The work is particularly notable for the attention given to the design of methods capable of reducing the speculative character of this type of research. The method adopted is to construct provisional phylogenetic trees which are based solely on a study of non-adaptive characters and on the distribution of the geographical subspecies. These are then tested by critical comparison with the known geological history of the area; conformity will then provide independent support for the correctness of the conclusions. In the present instance there is evidence that the eustatic lowering of the sea level during the Pleistocene was sufficient to unite Malaya, Java and Borneo in the west and New Guinea and Australia in the east. On the other hand, the Celebes and Molucca groups were probably never connected with these shelves. The existence of this barrier to eastward and westward migration proves to be in good agreement with phylogenetic trees arrived at after rigidly excluding geological evidence.

THE FUNCTIONS OF THE CORPUS ALLATUM IN INSECTS.—In discussing in SCIENCE PROGRESS (1946, 34, 800) some recent work on the control of moulting and metamorphosis, reference was made to the view that in some insects metamorphosis was not caused by the corpora allata ceasing to secrete "juvenile" or "inhibitory" hormone, thus releasing the potentialities of the latent imaginal

characters, but to the production by these glands of a special "metamorphosis-promoting" hormone. This has appeared particularly plausible in the higher Diptera where, as E. Hadorn (*Rev. Suisse Zool.*, 1941, **48**, 495) and others have shown, larval metamorphosis can be initiated by implanting the mature ring gland. However, some recent work on *Drosophila* by M. Vogt (*Nature, Lond.*, 1946, **157**, 512) has shown that the central part of the ring gland in the adult (which contains the corpus allatum) can cause the local suppression of adult characters when it is implanted into the larva. Under these conditions at least the gland must be secreting the juvenile hormone.

V. B. Wigglesworth (*Nature, Lond.*, 1947, **159**, 872; *J. exp. Biol.*, 1948, **25**, 1) has recently returned to this subject in *Rhodnius*. He confirms the finding that in the 5th and last nymphal stage the corpus allatum ceases to secrete juvenile hormone; but, in addition, the gland plays a more active role in metamorphosis by absorbing from the blood any traces of the juvenile hormone that remain. This can be demonstrated, among other methods, by causing the 5th stage nymph to moult in the absence of the corpus allatum; the adult then develops characters that are partly nymphal. Conversely, if the corpus allatum of the moulting 5th stage nymph is implanted into intact 3rd or 4th stage nymphs, the activity of the juvenile hormone is reduced and some show a partial premature metamorphosis on reaching the 5th stage. These effects are always diffuse and are never localised around the implant, as they often are when juvenile hormone is secreted. It is unlikely for this reason that the gland is producing a "metamorphosis-promoting hormone."

The author suggests that there may be here a basis for a theory of metamorphosis which will prove applicable to all groups of insects. In the Diptera the appearance of a metamorphosis hormone may be simulated by the active elimination of juvenile hormone.

In the newly moulted adult *Rhodnius* the corpus allatum continues for a time to function in a similar manner, then turns over once more to secrete juvenile hormone. The corpus allatum taken from an adult three weeks after moulting will induce an extra nymphal stage when implanted into a 5th stage nymph. Juvenile hormone is, therefore, produced in increasing quantities at a time when the corpus allatum is also elaborating the "gonadotropic hormone" required for the maturation of the ovaries and male accessory glands. As juvenile hormone from 4th stage nymphs will induce egg development, Wigglesworth suggests (as Pfeiffer had previously concluded for *Melanoplus*) that the juvenile and yolk-forming hormones are identical.

A fascinating problem, at present only partially solved, concerns the functional control of the corpus allatum. It appears that it is not the corpus allatum itself which "counts the instars" but some part of the nervous system. As a result of severing its nervous connections the functions of the corpus allatum in secreting or inactivating juvenile hormone may become unco-ordinated; and in some instances there are clear indications that the corpus allatum of the host may dominate or overcome the activity of an implanted organ.

INSECTICIDAL MISTS AND FLYING INSECTS.—The relations that exist between flying insects and the component particles of an insecticidal mist are of considerable practical importance, yet it is only recently that this subject has begun to receive close attention. An analysis of the relationship is contained in a series of papers by W. A. L. David (*Bull. ent. Res.*, 1946, **37**, 1; *Ann. appl. Biol.*, 1946, **33**, 133) and by W. A. L. David and P. Bracey (*Bull. ent. Res.*, 1946, **37**, 177) dealing with the behaviour of mosquitoes (*Aedes ægypti*) and houseflies when exposed to fine kerosene mists with an average particle size of 10 μ .

The quantity of insecticide deposited on the flying insect is, on the one hand, greatly influenced by the physical properties of the mist. One important aspect concerns droplet size. It is shown, for example, that in assessing insecticidal efficiency, the range of particle sizes, as well as the total concentration of insecticide, must be known. Very fine droplets rapidly diminish in size by evaporation, eventually becoming ineffective. If evaporation is suppressed by the addition of a proportion of non-volatile oil, the performance of the mist is improved correspondingly. The killing power of the mist is further enhanced by increasing the droplet size during atomisation, but this leads, in turn, to increased precipitation and wastage of spray material.

On the other hand, the results of exposing the insects to a mist with standard properties could only be predicted by taking into account certain biological factors which included the behaviour and physiological state of the insects. For example, any increase in flight activity among the insects in the test chamber was reflected in the higher pick-up of spray particles and in the higher subsequent mortality. (This is, of course, one sound reason for incorporating a small quantity of pyrethrins, which exert an irritant action, in fly sprays containing DDT.) The age and nutritional state of the insects were other important factors.

The quantity and distribution of the mist particles on the body of the insect were determined by spraying the insects with kerosene

containing a red sudan stain, then extracting and estimating the deposits colorimetrically. The mosquito in flight accumulates droplets mainly on the wings where these coalesce, forming flattened patches held by the microtrichia. The ultimate fate of the impacted droplets is greatly affected by the behaviour of the insects, however. Both *Aedes* and *Musca* clean themselves after alighting and the deposits may be smeared on the surroundings or combed off by the legs. In *Musca*, DDT and benzene hexachloride are readily absorbed, not only from the legs, but also through the cuticle of the wing blade; these substances are then translocated along the wing veins to the body cavity.

Similar problems, yet involving a different order of size, are discussed by J. S. Kennedy, M. Ainsworth and B. A. Toms (*Anti-Locust Bull.*, No. 2, 1948) in a paper dealing with the exposure of a large insect (*Schistocerca gregaria*) to relatively coarse oily sprays (droplet size 300-500 μ) containing benzene hexachloride or dinitro-ortho-cresol. This laboratory study was inspired by the need for devising an efficient procedure for killing locusts when swarms are sprayed from aircraft.

Droplets of this size have a relatively high terminal velocity and, therefore, impact well when falling directly towards the body of the insect. The difficulty is rather to increase the number of drops approaching the insect. In this context the authors distinguish between "attraction" and "impaction." Attracted particles are those which are drawn in towards the body by the air-flow created by the beating wings. Experiment showed that the number of droplets hitting the flying locust was in fact increased when the droplet size (within the range employed) was *reduced*. In other words, the larger droplets were not diverted by the flight movements but tended to fall past the insect. This contrasts with the situation encountered with the impalpable "stationary" mists used in fly sprays; in such mists, according to this conception, droplet accumulation is due mainly to "attraction," the impaction factor being constant and of comparatively minor significance.

In the flying locust wing-beat frequency is relatively low and the wings are, therefore, rather poor drop collectors; more spray droplets are received on the head and body, most of all on the legs. It is shown that the legs, and particularly their soft articulations, are also most vulnerable to the penetration of insecticides. Very little absorption appears to take place through the wings.

PHYSICAL ANTHROPOLOGY. By N. A. BARNICOT, B.Sc., Department of Anthropology, University College, London.

A CONSIDERABLE literature has accumulated since physical anthropology was last reviewed in this journal in 1939. Perhaps no field of work is at present more important for our understanding of the biological relationships of human ethnic groups than is that of serological genetics, and the present and succeeding article will attempt a summary of some of the more important publications on this subject during and since the war years. Boyd's valuable compilation of the bulk of the data up to 1939 (*Tabulæ Biologicæ*, 17, 113, 1939) testifies to the large amount of anthropological work on blood-groups which has been done all over the world since the discovery of the ABO-system in 1900, and its first application to racial studies in 1919. Boyd himself (*Am. J. Phys. Anthr.*, 27, 333, 1940) and Haldane (*Human Biol.*, 12, 457, 1940) have summarised some of the virtues of blood-group data for elucidating the origins and relationships of living peoples. They point out that the various anthropometric and pigmentary features which anthropologists commonly use are often difficult to record objectively (skin- hair- and eye-colour), are to a varying and inexactly known extent affected by environment (stature, cephalic index), are subject to change with the age of the subject (stature, eye-colour), and are more or less complex in their genetic determination, so that the results of race-crossing cannot easily be interpreted. The ABO (now A_1A_2BO) blood-groups are not open to these objections. Their comparatively simple determination by four allelomorphic genes (alleles) permits the calculation of the actual frequencies of these genes in a given population, thus providing a more fundamental characterisation than is afforded simply by the frequency of the phenotypes (blood-groups $A_1-A_2-B-A_1B-A_2B-O$) to which their combinations give rise. Since, they point out, the possession of a particular blood-group of this system is not known to confer any marked advantage or disadvantage on the bearer from the point of view of survival of the genes concerned, in a random-mating population of sufficient size, the gene-frequencies will reach a stable equilibrium, which may persist for long periods in the absence of crossing with populations of different composition. This means that a population may preserve an archaic blood-group pattern long after selection has caused divergence in adaptive characters such, perhaps, as pigmentation. One must be cautious, however, in concluding that selection has played no significant role in the evolution of blood-group patterns, since Waterhouse and Hogben (*Brit. J. Soc. Med.*, 1, 1, 1947), by a statistical examina-

tion of European and American ABO data, adduced evidence for the elimination by early foetal death of some 8 per cent. of A- children. The suggested mechanism is the lethal action of the anti-A (or alpha) antibody of the group O mother on a foetus bearing the A-antigen, the situation being analogous to that in the Rh-system presently to be discussed. Boyd and Haldane stress the part which fluctuations in gene-frequency due to chance (the drift effect of Sewall-Wright) may have played in small isolated prehistoric communities. The subsequent growth and crossing of these populations which had thus become divergent accounts, they suggest, for the main outlines of the present ABO distributions. Naturally, in any particular case historical and other evidence must be considered in conjunction with blood-group data, in order to arrive at the most plausible picture of the origin and relationships of the population concerned. Quantitative considerations, it seems, render it unlikely that mutation has played much part in the spread of blood-group genes in living races (at least if no selective pressure is granted) since the required time-scale would be too long. Further discussion of these matters may be found in Gates (*Human Genetics*, Vol. I, 1946) and Cappell (*Glasgow Med. J.*, 28, 397, 1947). The data which have accumulated on the racial distribution of the subgroup A₂, the MN-system, the P-antigen, and the secretor and taste genes will be considered in the following article. The discovery of the Rh-system of antigens and their application to racial studies lies wholly within the period under review, and must now be dealt with. Vallois' (*L'Anthropologie*, 51, 1947) and Cappell's (*loc. cit.*) reviews contain some discussion of these findings. It is necessary to give some account of the history and present position of our knowledge about this rather complex system of genes and antigens, but more detailed treatment may be found in Castle, Wintrobe and Snyder (*Science*, 107, 7, 1948), Race (*Brit. Med. Bull.*, 4, No. 3, 1946), Race, Mourant, McFarlane (*Rev. d'Hématologie*, 1, No. 1, 1946) and Fulton Roberts (*The Rh Factor*, Heinemann, 1947).

Landsteiner and Wiener in 1940 showed that, if rabbits or guinea-pigs were injected with red blood-cells from the Rhesus monkey, they formed an antibody which agglutinated these cells. The antisera so produced could also agglutinate 85 per cent. of human bloods, so that this proportion of the white American population with which they dealt also carried the Rhesus or Rh-antigen on their red cells. The antibodies, however, unlike those involved in the ABO system, are only formed in human serum under the abnormal conditions of incompatible transfusion and mother-foetus incompatibility. A single pair of alleles was held to be involved ;

the genotypes RhRh and Rhrh, carrying the dominant gene and hence the antigen, gave rise to the Rh-positive reaction-group, while the double-recessive rhrh gave rise to the Rhesus-negative condition. It was soon observed that different racial groups differed widely in the proportion of Rh-positive and Rh-negative individuals (Levine, *Science*, 96, 452, 1942). The following figures, selected from various authors, show that in a wide range of races Rh-negative individuals are much rarer than they are among Europeans, and in some they have not been found at all.

TABLE I

	% Rh-negative.	Authors.
Basque	30.5	Chalmers, Ikin, Mourant, in press.
French	14.36	Bessis, 1947.
English	14.78	Race, Fisher, 1946.
Norwegian (Oslo)	13.72	Hartmann, Mourant, Race, 1947.
Whites, U.S.A.	13.3	Wiener, Unger, Sonn, 1945.
Negroes, U.S.A.	7.3	Wiener, Belkin, Sonn, 1944.
Indians, U.S.A.	7.1	Wiener, Sonn, Belkin, 1945.
Chinese, U.S.A.	1.5	Wiener, Sonn, Belkin, 1945.
	0.7	Levine, Wong, 1943.
Japanese, U.S.A.	1.34	Waller, Levine, 1944.
	0.6	Miller, Taguchi, 1945.
Amerinds	0.8 *	Landsteiner, Wiener, Matson, 1942.
Ute Amerinds	0.0	Matson, Piper, 1947.
Mexican Amerinds	0.0	Wiener, Zepeda, Sonn, Polivka, 1945.
E. Eskimo	0.27 *	Jordan, 1946.
Indonesian (Javanese)	0.8 *	Simmons <i>et al.</i> , 1944.
Filipino	0.3 *	Simmons and Graydon, 1945.
Fijian	0.0 *	Simmons <i>et al.</i> , 1944.
Papuan	0.0 *	" " " "
Australian aborigines	0.0 *	" " " "

* It may be noted that all the figures given are not entirely comparable; according to the modern view, which is described later, those values marked * are D-negative, while the others are the percentages of cde.

The values given by different authors vary slightly, and may have to be modified as larger samples become available, but the general picture is clear. Many of the groups were drawn from residents in U.S.A. and white admixture cannot be excluded. As Vallois suggests (*loc. cit.*) and Mourant (personal communication) confirms, on data from Nigeria, Rhodesia, the Sudan and Cameroons, the proportion of Rh-negatives in negroes sampled in Africa is approximately 3 per cent., which is lower than the least-mixed group examined by Wiener in New York (7.3 per cent.). Khanokar and Sangvhi (*Nature*, 155, 427, 1945) also report the absence of Rh-negatives in 70 Hindus in Bombay. The high value for Rh-negatives among Basques, which Dr. Mourant kindly allows me to

quote from work in press, confirms Etcheverry's finding (cited by Mourant, *Nature*, 160, 505, 1947) and is of great interest. It has long been known that in a proportion of cases in which an Rh-negative mother bears an Rh-positive foetus, miscarriage or hæmolytic disease of the new-born results from the stimulation of Rh-antibody formation in the mother. This situation, therefore, involves the selective elimination of certain genes responsible for the Rh-antigens (D-d, *vide infra*), as was recognised by Haldane (*Ann. Eug.*, 11, 333, 1942) and Wiener (*Science*, 96, 407, 1942). The present gene-frequencies in Europe may, therefore, be in unstable equilibrium, and Wiener postulated that the existing condition had arisen from the mixing of peoples of pure Rh-positive and Rh-negative constitutions within the last few thousand years. We have seen that substantially pure Rh-positive and Rh-negative races are available; it may well be, therefore, that the Basques, with their peculiar linguistic isolation, are the relatively unmixed descendants of the postulated Rh-negative population. As would be anticipated, the incidence of hæmolytic disease of the new-born in predominantly Rh-positive races such as Negroes, Chinese and Japanese, is reported to be much lower than in Europe (Levine, *Science*, 96, 452, 1942). The original simple scheme for the Rh-factor and its inheritance was soon outmoded. Further work with anti-Rh sera from human cases showed that more than one antigen was involved. Wiener considered that there were six alleles, that is to say six possible gene substitutions at one locus on the chromosome; these genes he symbolised in his later work as R_1 — R_2 — R_0 — r' — r'' — r , to which the rare ry and Rz were subsequently added. Each gene was held responsible for the production of an antigen, except R_1 and R_2 , which produced two each (R_0 - r' and R_0 - r'' respectively). The discovery of sera reacting with Rh-negative blood by Levine, which was confirmed by Race and Taylor in England, led to further modifications, and Race (*Nature*, 153, 542, 1944) published a new scheme, based on suggestions by Fisher, which subsequent discoveries and the greater clarity and simplicity of the nomenclature have brought into wide use. Fisher envisaged six genes, instead of Wiener's final eight, each of which produced an antigen. These he called C—D—E—c—d—e to indicate that they constitute three allelomorphous pairs. Furthermore, one member of each pair is closely linked to the other two on the chromosome, constituting a complex of three genes which behave genetically as one, and it is the eight possible combinations which correspond to Wiener's eight alleles. The relationship is shown on page 706. The English workers now use the capital R for all except ode.

Wiener.	R ₁	R ₂	R ₀	r'	r''	ry	Rz	r
Fisher-Race	CDe	cDE	cDe	Cde	cdE	CdE	CDE	cde

The gene D is the most potent in stimulating antibody production; C and D being weaker, rh', rh'' and rhy behave, for most clinical purposes, as Rh-negative, as does cde, the full Rh-negative combination. It must be realised that since each person carries two chromosomes numerous genotypes can occur. Thus in Europe Rh₁ is the commonest serological type; the commonest genotype being R₁r or CDe/cde (37 per cent.), followed by R₁R₁ or CDe/CDe and R₁R₂ or CDe/cDE, approximately 15 per cent. each. Methods are available for the calculation of the gene-complex and gene-frequencies, but to apply these a full range of potent sera to distinguish the genotypes is needed, so that few of the results of Rh typing on non-European populations are available in these terms. The large number of gene-complexes involved in the Rh-system makes for a very wide range of possible patterns in their frequencies, and in this respect it has great value for the study of racial origins and relationships. Some of the results already obtained are given below in Table II. In this table the older serological-type symbols have been retained, since the literature on racial differences is almost entirely in these terms. This is a phenotype nomenclature, and the frequency of the various possible genotypes in each serological type is not indicated.

TABLE II
THE PERCENTAGES OF DIFFERENT Rh-SEROLOGICAL TYPES IN A NUMBER OF RACIAL GROUPS

	Serological Type.							
	R ₁	R ₂	Rh ₁ Rh ₂	Rh ₁	Rh'	Rh''	rh	
English	54.91	12.19	13.59	2.48	0.65	1.20	14.78	Fisher, Race, 1946.
American Whites	54.4	13.7	15.0	2.4	1.0	0.2	18.3	Wiener, Unger, Sonn, 1945.
Negroes, U.S.A.	21.2	21.9	7.8	41.6	0.7	0.0	7.8	Wiener, Belkin, Sonn, 1944.
Chinese, U.S.A.	60.6	8.0	24.1	0.8	0.0	0.0	1.5	Wiener, Sonn, Belkin, 1945.
Japanese, U.S.A.	37.40	18.85	47.25	0.0	0.0	0.0	1.34	Waller, Levine, 1944.
Mexican Americans	48.0	9.2	41.8	1.0	0.0	0.0	0.0	Wiener <i>et al.</i> , 1945.
Ute Americans	23.86	28.84	37.8	0.0	0.0	0.0	0.0	Matson, Piper, 1947.
Indonesian (mainly Javanese)	75.0	2.0	22.0	0.0	0.0	0.0	0.0	Simmons, Graydon, 1947.
Fijian	89.1	1.8	9.1	0.0	0.0	0.0	0.0	" " "
Papuan	98.0	0.0	4.0	0.0	0.0	0.0	0.0	" " "

A very striking fact is the high proportion of cDe (41.6 per cent.) in Negroes, which distinguishes them from any other group, including the Melanesians to whom they have sometimes been considered related, and the proportion may well be higher where white

admixture can be excluded. The Chinese and Japanese, compared with Europeans, show a high percentage of the combination CDe/cDE, are low in cDe and cde, and lack the other gene-complexes; the Mexican Amerinds resemble them fairly closely, the Ute of Utah rather less so. The Melanesians of Papua and the Admiralty Islands have almost exclusively CDe, while the Fijians and Indonesian group appear to be somewhere between them and the Chinese. Table III reproduces figures from a few full investigations in which the calculation of gene-complex or Rh chromosome frequencies permits a more basic comparison.

TABLE III
THE PERCENTAGE OF DIFFERENT Rh GENE-COMPLEXES IN A NUMBER OF
DIFFERENT RACIAL GROUPS

	Chromosome or Gene-complex.								
	(R ₁) CDe	(R ₂) cDE	(R ₃) cDe	(r') Cde	(r'') cdE	(ry) CdE	(Rs) ODE	(r) cde	
English *	43.6	12.8	3.0	0.8	1.7	(0.005)	0.1	39.9	Fisher, Race, 1946.
Basque *	33.8	6.9	0.6	1.6	0.0	0.0	0.0	57.2	Chalmers, Ikin, Mourant, in press.
Negro, U.S.A. . . .	12.9	14.2	42.9	1.3	0.0	0.0	0.0	27.0	Wiener, Belkin, Sonn, 1944.
Admiralty Islands.	84.0	3.0	3.0	0.0	0.0	0.0	0.0	0.0	Simmons, Graydon, 1947.

* The English and Basque results are quoted here only to the first decimal place.

From the few examples given, it can be appreciated how much our knowledge may be advanced by more extensive and detailed work, particularly when Rh results are collated with A₁A₂BO and MN findings, since the inheritance of each of these systems is independent of the other. A new allele, C^w at the C-c locus, was recently discovered in this country (Race, Mourant, and Callender, *Nature*, 157, 410, 1946), thus further increasing the applications of Rh-typing to anthropological work.

Finally, one may hope for further data on the Rh-system in lower Primates, particularly anthropoid apes. Mourant and Race (*Science*, 104, 227, 1946) confirmed Wiener and Wade in finding an Rh-negative reaction in a chimpanzee; moreover, the blood of this animal failed to absorb anti-E and anti-e sera, showing absence of these antigens, and perhaps of this locus on the chromosome.

NOTES

The Synthesis of Adenosine Triphosphate

Adenosine triphosphate was first isolated by Lohmann (*Naturwiss.*, 1929, **17**, 624) and by Fiske and Subbarow (*Science*, 1929, **70**, 381) from muscle, and, during the past fifteen years, its key position in the biochemical mechanisms of the cell has been increasingly realised. Not only has Lohmann's view been confirmed that it is in this compound, with its high energy pyrophosphate bond, that the energy derived from the aerobic and anaerobic processes of the muscle is accumulated before being liberated as mechanical work during muscular contraction; it has become quite clear that this compound acts as a carrier of phosphate groups as well as of energy, and that it functions as a phosphorylating agent in living tissue generally. We may regard the chief purpose of the catabolic, energy-liberating reactions of the cell to be the synthesis of A.T.P.; once this has been built up, the energy becomes available, either for chemical work, in the sense of being utilised for synthesising various important compounds, or for mechanical work, as in muscular contraction. Evidently the complete synthesis of this remarkable compound is an event of great interest to all biochemists.

A.T.P., as its name implies, is formed out of adenosine and three molecules of phosphoric acid, adenosine itself being a compound of the purine derivative adenine and the pentose sugar, ribose. When one phosphoric acid group is removed from A.T.P. we obtain adenosine diphosphate, A.D.P., and when two molecules are removed we are left with muscle adenylic acid. This last compound is adenosine-5'-phosphate.

For some years Professor A. R. Todd and his colleagues have mainly devoted themselves to the elaboration of methods for the synthesis of nucleosides, and one of their important achievements was the synthesis of adenosine by a method which left no doubt as to the precise structure of this compound (see Todd, *J. Chem. Soc.*, 1946, 647). This synthesis gave the final proof of a structure which had previously been arrived at by the efforts of many workers, using analytical and largely physical methods. Todd and his co-workers

also worked out an elegant method of introducing phosphoric acid groups into organic compounds which depended on the use of dibenzyl chlorophosphonate and which could be much more completely controlled than processes using reagents such as phosphorus oxychloride. By the application of this new technique of phosphorylation to a suitable derivative of adenosine, Baddiley and Todd (*J. Chem. Soc.*, 1947, 648) prepared muscle adenylic acid (and also adenosine diphosphate) identical in all respects with the compound derived from muscle.

The introduction of the third phosphoric acid group into the adenosine nucleus was obviously a somewhat tricky business, and might have been expected to present formidable difficulties. However, Baddiley, Michelson and Todd now report (*Nature*, 1948, 161, 761) that all these difficulties have been overcome and that they have succeeded in introducing a third phosphoric acid group, and, what is also important, have introduced it in such a way that its location is not in doubt. The synthetic product proves to be completely identical with the natural compound, not only in its physical and chemical behaviour, but also in its biochemical properties. This synthesis is another signal contribution to the advancement of biochemistry by the synthetic organic chemist.

Mathematical Tables for Science and Industry

It is considered that important investigations in the physical, chemical and engineering sciences, in mathematics and in industry may be held up by the absence of relevant mathematical tables. Several important tables of a fundamental nature have in the past been produced by the British Association through its Mathematical Tables Committee. The work in this field has now been transferred to the Royal Society on the invitation of the British Association and a special Royal Society Mathematical Tables Committee has been established to continue and extend the earlier work. This Committee would be glad to receive suggestions from industrial research groups, from individual investigators and from Service departments relating to existing needs in their special fields. Requests for assistance may be met either by advice as to the most economical way of producing the desired tables, or by an undertaking to produce the tables under the auspices of the Royal Society, or, in exceptional cases, by a grant towards the cost of tabulation or to make possible the publication of important tables which may exist only in manuscript. In the first instance enquiries should be addressed to the Assistant Secretary, The Royal Society, Burlington House, London, W.1.

Miscellanea

The honours list published on the occasion of H.M. the King's birthday included the following : *Baron* : Sir Alfred Webb-Johnson, president of the Royal-College of Surgeons. *K.B.E.* : Sir Hector Hetherington, principal and vice-chancellor of the University of Glasgow ; Acting Air-Commodore Frank Whittle. *Knights* : A. L. B. Ashton, director of the Victoria and Albert Museum ; Dr. A. H. Gardiner, the well-known Egyptologist ; Dr. C. R. Harington, director of the National Institute for Medical Research ; Prof. C. N. Hinshelwood, Dr. Lee's professor of chemistry, University of Oxford ; J. D. G. Medley, vice-chancellor of the University of Melbourne. *C.B.* : H. M. Garner, principal director, scientific research (air), Ministry of Supply ; N. B. Kinnear, director of the British Museum (Natural History) ; O. Thornycroft, director of aeronautical and engineering research, Admiralty. *C.M.G.* : W. N. Allan, recently director of irrigation, Sudan Government ; Dr. P. Stocks, chief statistician (med.), General Register Office ; Dr. H. H. Storey, secretary of the Colonial Agricultural Research Committee. *C.B.E.* : Prof. W. E. Agar, lately professor of zoology, University of Melbourne ; H. H. B. Allan, director of the Division of Botany, Plant Research Bureau, New Zealand ; Prof. J. H. Biggart, professor of pathology and dean of the Faculty of Medicine, Queen's University, Belfast ; H. W. Cremer, president of the Institution of Chemical Engineers ; Mrs. Maud E. Cunningham, for services to archæology ; Prof. A. Findlay, emeritus professor of chemistry, University of Aberdeen ; B. E. Frayling, lately chief inspector of mines, Nigeria ; Lieut.-Colonel W. French, superintendent of the Technological Department, City and Guilds of London Institute ; Dr. A. A. Griffiths, research engineer, Rolls-Royce, Ltd. ; W. F. Higgins, superintendent of the Physics Division, National Physical Laboratory ; Prof. W. R. Jones, emeritus professor of mining geology, Imperial College of Science and Technology ; C. Kershaw, director of British Optical and Precision Engineers, Ltd. ; Dr. A. King, chief scientific liaison officer, Office of the Lord President of the Council ; Prof. H. A. D. Neville, lately professor of agricultural chemistry, University of Reading ; Miss Margery Perham, reader in Colonial administration, University of Oxford ; Prof. J. Ritchie, professor of natural history, University of Edinburgh ; Dr. H. V. Taylor, lately senior education and advisory officer, Ministry of Agriculture ; S. G. Taylor, director of irrigation, Ceylon.

The Right Hon. Sir Stafford Cripps has been elected a fellow of the Royal Society under the Statute which provides for the election

of persons who either have rendered conspicuous service to the cause of science or are such that their election would be of signal benefit to the Society.

The following have been elected foreign members of the Royal Society: Prof. D. W. Bronk, director of the Eldridge Reeves Johnson Research Foundation, University of Pennsylvania; Prof. L. E. J. Brouwer, professor of mathematics in the University of Amsterdam; Prof. Maurice Caullery, formerly professor of zoology at the Sorbonne; and Prof. L. C. Pauling, professor of chemistry in the California Institute of Technology.

The United States Medal for Merit, the highest award made by the President of the United States to a civilian, has been presented to Sir Alexander Fleming and to Sir Howard Florey for their work on penicillin.

The American Medal of Freedom with Silver Palm has been awarded to Prof. S. Zuckerman, professor of anatomy in the University of Birmingham, and the Medal of Freedom to Prof. W. N. Thomas, professor of engineering, University College, Cardiff.

Sir Robert Robinson, P.R.S., is a member of the council set up, under the chairmanship of General Lord Ismay, G.C.B., C.M.G., D.S.O., to supervise the Festival of Britain 1951. The function of the Council will be to supervise the Festival of Britain as a whole, whilst responsibility for the detailed planning and arrangements will rest on a number of constituent organisations with specialised knowledge and competence in the various fields to be covered. A special council is to be constituted to supervise the detailed exhibition arrangements for science and technology.

Dr. F. J. Wilkins has been appointed principal director of scientific research (defence) in succession to Dr. E. T. Paris.

The following have been appointed to the Advisory Council on Scientific Research and Technical Development, Ministry of Supply, which advises on the important scientific and technical problems with which the Ministry is concerned: Sir Howard Florey, professor of pathology, University of Oxford; Prof. J. L. M. Morrison, professor of mechanical engineering, University of Bristol; Prof. L. Rosenhead, professor of applied mathematics, University of Liverpool.

The following have been appointed to be members of the Chemistry Research Board of the Department of Scientific and Industrial Research for a period of four years: Mr. A. L. Bacharach, head of Nutrition Unit, Glaxo Laboratories, Ltd.; Prof. H. J.

Emeléus, professor of inorganic chemistry, Cambridge University; Prof. D. M. Newitt, professor of chemical engineering, Imperial College of Science and Technology.

The Lord President of the Council, on the advice of the Advisory Council on Scientific Policy, has approved the setting-up of an Inter-departmental Committee on Overseas Scientific Relations to consider and advise on questions of United Kingdom Government policy on matters of overseas scientific relations. The chairman is Sir Edward Appleton, G.B.E., K.C.B., F.R.S., and, in addition to representatives of government departments, the membership will include the foreign secretary of the Royal Society, representatives of the British Council and of the Conference of Research Associations, and two University scientists. The secretary of the Committee is Mr. H. L. Verry, head of the Overseas Liaison Division, Department of Scientific and Industrial Research.

The Minister of Fuel and Power has appointed the following to be members of the Scientific Advisory Council which will advise him on the scientific aspect of his statutory duties: Sir Alfred Egerton (Chairman), Eng. Vice-Admiral Sir Harold Brown, Dr. H. Roxbee Cox, Sir Charles Ellis, Prof. F. H. Garner, Sir Charles Goodeve, Dr. E. S. Grumell, Mr. J. Hacking, Dr. H. Hollings, Prof. D. M. Newitt, Dr. H. R. Ricardo, Prof. S. Zuckerman.

The Government of India has created a Department of Scientific Research with effect from 1st June, 1948. The Department will be in the charge of the Prime Minister of India, and Sir Shanti Bhatnagar, director of scientific and industrial research, has been appointed secretary and principal executive officer. The Department will take over the work of the Board of Research on Atomic Energy and the Council of Scientific and Industrial Research. The latter body, although attached to the new Department, will retain its unofficial character and will continue to function as before. The new Department will deal also with scientific advice to government departments, *ad hoc* scientific research in universities and research institutions, research scholarships in applied scientific subjects, international scientific unions, scientific liaison offices, the Scientific Consultative Committee and such other subjects as may be transferred to it. The Department will co-ordinate the scientific activities of the other Ministries and in this work it will be assisted by a Co-ordination Committee consisting of eminent scientists.

We have noted with great regret the announcements of the death of the following scientific workers during the past quarter: Prof.

S. C. Brooks, professor of zoology in the University of California ; Dr. A. S. Corbet, of the Department of Entomology of the British Museum (Natural History) ; Dr. Vaughan Cornish, geographer ; Prof. W. T. David, professor of engineering in the University of Leeds ; Dr. E. P. Harrison, formerly professor of physics at Presidency College, Calcutta, and later chief scientist to the Mine Design Department in H.M.S. *Vernon* ; Dr. A. E. Jones, lecturer in mathematics at the Imperial College of Science and Technology ; Prof. Alfred C. Lane, emeritus professor of geology at Tufts College, Massachusetts ; Prof. Jacques de Lapparent, lithologist ; Mr. C. A. Malcolm, C.I.E., formerly chief conservator of forests, Central Provinces, India ; Sir George Newman, G.B.E., K.C.B., formerly chief medical officer of the Ministry of Health and of the Board of Education ; Mr. G. L. Overton, formerly keeper of the Department of Air and Water Transport at the Science Museum ; Dr. J. G. Parker, formerly principal of the Leathersellers' Technical College, London ; Dr. G. H. Pethybridge, O.B.E., formerly director of the Plant Pathology Laboratory, Harpenden ; Sir Daniel Richmond, C.I.E., formerly chief conservator of forests, Madras ; Dr. H. W. Richmond, F.R.S., geometer ; Mr. J. J. Shaw, C.B.E., seismologist ; Prof. Väinö Tanner, geologist ; Sir D'Arcy Wentworth Thompson, C.B., F.R.S., professor of zoology in the University of St. Andrews.

In 1946 the Royal Astronomical Society marked with a special celebration the centenary of the discovery of the planet Neptune. In honour of the occasion, Prof. W. M. Smart of Glasgow delivered an address upon the contribution of John Couch Adams to that great triumph of gravitational astronomy. The address was an abridgment of a memoir, the material for which Prof. Smart derived from the Adams papers, committed to his keeping. This memoir has now been published (*John Couch Adams and the Discovery of Neptune*. Pp. 56, with 3 plates. London : Royal Astronomical Society, 1947). Copies are on sale to the public at 5s. each, and may be obtained from the Assistant Secretary of the Society, Burlington House, W.1. Of especial interest to the reader are Prof. Smart's account of Adams's early years, the fullest that has yet appeared, and his general sketch of the problem, tackled independently by Adams and Le Verrier, of deducing, from the known disturbances of the motion of Uranus, the existence and position of an unknown disturbing planet. Prof. Smart deals at some length with the painful controversy that arose concerning the respective shares of the two analysts in the discovery of Neptune. When all is said, it remains a mystery why Airy, the Astronomer Royal, did not give Adams's prediction

of the unknown planet's longitude equal publicity with Le Verrier's estimate (with which it agreed to within one degree) during the interval of close upon four months between the publication of the Frenchman's memoir of June 1 and the discovery of the planet on September 23, 1846. The memoir contains three full-page portraits of Adams, one of them an attractive photograph taken in 1856 and hitherto unpublished.

We have received from the Laboratoires des Huiles Elektrion, Ghent, a booklet entitled *La Viscosité des Mélanges de "Fluides Normaux"*, by Michel Roegiers and Lucien Roegiers [pp. 47, with 8 figures]. Beginning with the Boltzmann theory, the authors derive an equation of the form $\eta = Ae^{-\frac{RT}{E}}$ for the viscosity of a Newtonian liquid free from directive forces, in opposition to the more usual form such as that of Andrade: $\eta = Ae^{E/RT}$. For a liquid mixture, the logarithms of the viscosities of the component liquids are shown to be additive when concentrations are expressed in fractions of intermolecular attraction energy, although the final equation is incorrect dimensionally. An abacus is described which enables the viscosity of a binary mixture to be read off to an accuracy of 1 per cent. The equations have been found to hold within 1 per cent. for mixtures of refined mineral lubricating oils with very different viscosities. A revision to Raoult's Tonometric Law is suggested in the light of their theory in that the *logarithms* of vapour pressures of component liquids are additive when concentrations are expressed in mole-fractions. Here again it would appear that the two sides of the equation have different dimensions.

A group of very soft glasses have recently been developed at the B.T.H. Research Laboratories by means of which two pieces of glass can be "soldered" together without distortion. These "solder" glasses, as they are popularly termed, are lead borate glasses whose resistance to weathering has been improved by the addition of zinc oxide. By comparison with other types of glass, the temperature at which they become soft is very low, usually being well below 460° C. One production process in which use has been made of this new type of glass is in the manufacture of miniature radio valves. Here a solder glass with a suitable coefficient of expansion has been successfully employed to join the soda-lime-silicate bulb to the lead glass base without distorting either.

The solder glasses can also be used to seal mica to metal or glass, as well as metal to glass. An interesting example of what is possible with this new technique is shown in *B.T.H. Activities*. A piece of

soda glass tubing has had a mica window sealed on to one end, a glass plate on to the other, and a sheet of nickel-chrome-iron alloy sealed across the middle to form a diaphragm, all the seals being made with solder glass.

The *Bell Laboratories Record* for April 1948 contains an article entitled "Artificial Dielectric Lenses for Microwaves" by W. E. Kock. This article gives a short but lucid account of the development of an antenna capable of passing a very wide band of frequencies. The development will be of great value in the future when radio relay circuits make full use of the broad band-widths available.

Early experiments showed that lenses were in many ways superior to other types of antenna for concentrating the beam in the direction of the receiving station. The lenses in use at the present time consist of rows of conducting plates and operate on wave guide principles, the waves being speeded up as they pass between the plates. By suitable design of the plates, the beam can be focussed, but the focussing power of the lens depends on the wavelength, as in an ordinary optical lens. For a band of wavelengths, some wavelengths will be more effectively focussed than others for the particular link considered, and consequently a band width limitation is imposed on the whole system.

It has been known for quite a long time that lenses made of dielectrics such as polystyrene had good focussing properties over very broad bands. Unfortunately their great weight for 10-foot lenses precluded their adoption in place of the metallic plate type of lens. This recent development has been to construct what might be termed a *metallic dielectric* which possessed the same characteristics as a true dielectric but which could be made much lighter.

In a true dielectric the molecules, when placed in an electric field, exhibit induced polarisation due to the distortion of the molecules. A similar effect takes place when an insulated metal sphere is placed in an electric field, the electrons moving so that positive and negative charges appear on opposite sides of the sphere. Thus, by building up a lattice of conducting spheres similar to the molecular lattice of a dielectric, a *metallic dielectric* can be made. For micro-waves whose wavelengths are longer than the spacing between the spheres, the artificial dielectric behaves in a similar way to a true dielectric, and in fact an appreciable sealing up of molecular lattice spacings has been achieved. Provided the lattice spacing requirement is met, the refracting properties of these artificial dielectrics do not vary with wavelength, and a lens built up with

small conducting elements has very broad frequency band characteristics.

The conducting elements can be in the form of spheres or discs, made of copper foil to reduce weight and possibly mounted in lightweight polystyrene foam. For use in radio repeater circuits the lenses are made 10 feet square, and, as only vertically polarised waves are used, the conducting elements are in the form of thin metal strips mounted horizontally. The lengths of the strips are varied to give the final assembly a shape similar to that of a convex optical lens, and are mounted between slabs of polystyrene foam. This new type of lens will first be used in the recently announced New York to Chicago link.

A well-written and finely-illustrated brochure on the operations of the Illinois State Geological Survey, by its Director, M. M. Leighton (*Illinois State Geol. Surv. Circular*, No. 126, 49 pp.), would lead one to think that the model Geological Survey had found the ideal state. But, banter apart, the Illinois Geological Survey is a very live organisation which plays an important part in the economy of the state. Along with the Natural History Survey it occupies the fine new Natural Resources Buildings, specially designed and constructed in 1939, and opened in 1940 just in time to allow the Survey to take its full part in the fundamental mineral research necessitated by the War. The scope of the Survey's work may be indicated by listing the subjects of applied research: coal, oil and gas, industrial minerals, clay, fluorspar, zinc and lead ores, ground-water geology, geophysical exploration, engineering geology, soil mechanics and clay mineralogy. In the last-named subject especially have the geologists of the Illinois Geological Survey made great progress and enhanced the reputation of the organisation. The Survey fully discharges its primary function of furnishing information on mineral resources to the public and, in addition, is well aware of a correlated and often neglected function, namely, that of an educational programme designed to extend knowledge of state resources and their significance in public welfare.

Agricultural Meteorology is the name given to the study of biological problems connected with plant growth in relation to soil and atmospheric conditions. A section of Agricultural Meteorology was started at the Meteorological Office, Poona, fifteen years ago, and an account of the progress made is given in an article by L. A. Ramdas published in the January issue of the *Indian Journal of Scientific and Industrial Research*.

The subject is rich in opportunities for the experimental physicist, and he is able to apply many of his own techniques to this new field. For instance, a sensitive portable galvanometer and thermocouple set has been developed and is very useful for measuring the temperature of growing plants. A bimetallic spiral of brass and invar has been used to close a warning circuit when the temperature drops below a pre-set limit. This instrument is installed in the open, and gives the farmer early warning of approaching frosty conditions so that he can light suitable heaters to warm up his orchard or vine garden. Other instruments have been designed to measure heat losses by convection, the rate of percolation of water through different soils and of evaporation from water and soil surfaces.

A particularly interesting set of experiments have been made on the problem of irrigation. In arid tracts it is preferable to use "sub-soil" irrigation in order to reduce the loss of moisture from the soil surface and thus to make best use of the little water available. Information was required on the effect that the depth of the free water table had on a plant's growth. Special containers were designed, each consisting of two concentric cylinders, the inner one to contain soil and the outer one water. The level of the water could be adjusted, and tests were made on nine pairs of jowar seedlings which were transplanted into similar containers whose free water table varied from 1 inch to 19 inches below the soil surface. The results showed that up to a depth of 17 inches the length of the roots increased rapidly in their search for water, but for depths greater than this the plants could not survive. The colour of the foliage depended on the depth of the water, but the length of the shoots was only slightly affected.

Electronic devices have been incorporated in many pieces of apparatus, including a spectrograph which presents, on the screen of a cathode ray tube, a graph of light intensity against wavelength. This instrument is used to determine the water content of the atmosphere by measuring the absorption in the water bands, and is of great use in weather forecasting. Another instrument employs a photocell to charge a condenser which then discharges through a neon lamp and operates a mechanical counter—the number of counts depending on the amount of light falling on the photocell. This instrument is used to obtain an integrated value of the solar radiation and is a marked improvement on the older methods.

Many other instruments have been developed, for measuring the moisture content in the soil, for filtering dusty air or water, and so forth. Much of the work has been done by voluntary post-graduate research scholars working with the small regular staff. The progress

made is truly remarkable, as the long list of papers, published by members of the department, bears witness.

The ways in which tobacco seeds, the chief by-product of the tobacco industry, could possibly be used, forms the subject of an article in the May issue of the *Indian Journal of Scientific and Industrial Research*. Although 8000 tons of these small seeds are available in Madras alone, their utilisation has not attracted the attention it deserves.

By pressing and other means, a greenish yellow oil can be extracted from the seeds. This oil is odourless, does not contain nicotine or other toxic materials, dries on exposure to air and solidifies at 25° C. It has been used in special types of dull paints, but is inferior to linseed oil. Recent experiments suggest that it can be used for edible purposes, and some reports state that it has already been put to this use in Bulgaria. Hydrogenation of the oil should make it into a useful raw material for the soap industry, while, as an illuminant, it compares favourably with other oils.

A seed cake can also be made from the tobacco seeds, which, after the addition of lysine, has been shown by experiment to promote growth in animals. The possibility of using the cake as a fertiliser, or as a source of raw material for the plastics industry, deserves further attention.

The recent increase in occurrence of woodworm, particularly in Utility furniture, has been causing some concern and it is welcome news that the Department of Scientific and Industrial Research are now prescribing suitable treatment, details of which are given in a Press Release dated June 8th.

The woodworm, or *Lyctus* beetle, infests only the sapwood of hardwoods and does not attack softwoods such as pines, deals, etc. The beetles emerge during the summer months, spreading infestation. The treatment, which has been developed by the Forest Products Research Laboratory of the D.S.I.R., is to spray timber with a 2 per cent. D.D.T. emulsion made up from commercially available oil concentrates by dilution with water. The spray only acts as a preventative for protecting timber and is not recommended for use on timber already known to be attacked. Full information on how long a single application will remain effective is not known, but tests indicate that it should give protection for at least one season.

A new edition of the book *The Inventor and His World* by H. Stafford Hatfield has recently been included in the Pelican Books

series. Numerous additions and alterations have been made to the first edition which was published in 1933.

The author's aim is not merely to describe the past achievements of a few inventors but rather to consider in more general terms the many aspects of the subject of invention. How are inventions made, and what type of mind is most likely to make them? It is interesting to read in the early chapters that it is not necessarily the prerogative of those with high academic qualifications to break new ground. Sometimes the step ahead is made by a person of fairly low general intelligence but who possesses a creative mind. What is the mechanism by which a new idea, or the neat solution to a certain problem, suddenly flashes into the mind? Can it be explained by saying that the subconscious mind takes the known facts, works out the solution, and forwards it to the conscious mind? This and many other questions are discussed by Dr. Hatfield in the first part of his book.

Many examples of the work of men with creative minds are given. We read of Goodyear's long struggle which led, after many years of disappointment, to the final perfection of a method of vulcanising rubber, of Lord Kelvin whose genius was soon recognised, of Edison, of Leonardo and many others. In the later chapters, which are devoted in turn to different branches of technical invention, it is refreshing to read of a few wild ideas which were not successful as well as of those ideas which were. The author's outlook is essentially practical and he tends to be a little sceptical of those careful analytical researches, which, while not having any obvious application at the time, have so often proved of the utmost value at a later date.

The future prosperity of our country is a problem which is very much to the fore at the present time, and the suggestion that we must again lead the world in standard of production, that we must turn cheap raw materials into highly valued articles for export, is certainly very sound. The place of science in society, a question which has been aggravated by the development of the Atomic Bomb, is also discussed.

Throughout the book the author points to subjects for invention that the lone worker might well tackle, and considerable space is devoted to such questions as the financing and development of an invention. It is questionable whether nowadays the individualist can profitably compete with the very large teams of workers who, contrary to the author's view, do often produce creative work despite the fact that they have to work from 9 to 6. The last chapter in the book is devoted to a description of one of the great

mysteries of modern times—Patent Law. A Bibliography is included which, as the author points out, is not complete but is still quite comprehensive.

The sixth number of *Science News* (Penguin Books, 1s. 6d.) fully maintains the standard of previous numbers. From the editorial we cull the following: "In our view scientific news is of three kinds. Partly it is a summary of what scientists are thinking and talking about in the Labs., what subjects they are investigating, *not* the detailed results of that investigation. Here it is the current atmosphere, the scientific weather, that we are out to report. . . ." In fact, the excellent articles in the present number, entitled Farming Front, Medical Front and Physics Front, are reminiscent of "Recent Advances" in SCIENCE PROGRESS, but treated in a less technical and more journalistic manner which would appeal to the intelligent man in the street as well as to workers in the respective sciences. Other articles are: Making Penicillin, Problems of Nuclear Physics, Diatoms, Report on Antarctica (by G. Rabel), and Glaciers (by M. F. Perutz). The last-named is notable for good discussions of the mechanism of flow of ice and its crystallographic structure, the latter being illustrated by some remarkable photographs of thin sections of ice. It would be interesting to microscopists and petrographers to learn how these thin sections were made.

All users of scientific instruments will welcome the first post-war edition of the Scientific Instrument Manufacturers' Association Handbook, obtainable from 26 Russell Square, London, W.C.1, at 10s. 6d. post free. Its main feature is an alphabetical index (containing about 2000 entries) of the products made by the member-firms of the Association, supplemented by a list of the firms with their products. By using these lists, it is easy to discover the names and addresses of the makers of almost any type of scientific instrument, as well as the products of any particular firm. The Handbook contains also full information about the Association, illustrated reviews of the British Scientific Instrument Industry and its work, and an 8-page list of research organisations.

ESSAY REVIEWS

THE TRAINING OF A DOCTOR. By HAROLD C. EDWARDS, C.B.E., M.S., F.R.C.S. Being a Review of **The Training of a Doctor.** Report of the Medical Curriculum Committee of the British Medical Association. [Pp. 151.] (London: Butterworth & Co. (Publishers) Ltd., 1948. 7s. 6d. net.)

THE time for a critical total review of the medical curriculum is over-ripe. During the past thirty years there have been spasmodic attempts both by individuals and by committees to re-organise medical education to match the changed needs of medicine. None has succeeded in producing more than minor modifications. Well-intentioned individual efforts to bring about reforms have come to naught when brought against the solid wall of tradition, the varied claims of the different institutions, and of the "specialities." Most activity of this kind petered out—or at least went no further than pious expressions of opinion. Perhaps most efforts at reform have failed because they are too sectarian in outlook; or because those in charge—if indeed there be anybody "in charge"—are too reactionary. The latter may fear that change will "make confusion worse confounded." Or they may have reached an age at which the exertion required to effect any radical change is too great for their corporate body to sustain.

Why, may the observer ask, is there any need for reform? Broadly this can be answered in three ways. First, general education has, for various reasons, yielded place to utilitarian instruction. The boy at matriculation stage has to decide his future—science or the arts—before his basic education is near completion, so that the medical student of to-day is usually indifferently educated. Secondly, with the growth of specialisation, the general practitioner of medicine, to create whom the curriculum was devised, has a changed role to play. The family doctor as we used to know him is in danger of becoming extinct.

Thirdly, the advance of knowledge, and again the claims of the specialist, have so enlarged the field, that the undergraduate student is overburdened—he is indeed in danger of being overwhelmed.

Added to these, the qualifying examinations themselves are suspect of demanding too much. Their scope has increased, and their standard perceptibly heightened.

Against this background, the B.M.A. in 1945 appointed a committee of 21 under the able chairmanship of Professor Henry Cohen to "review the Association's report on Medical Education (1934) in the light of later developments and the requirements of modern practice." This term of reference has been interpreted by the committee as: "to consider what modifications should be made in the aim, conduct and structure of the medical curriculum in the light of increasing medical and educational knowledge, of the changing needs of medical practice."

The result of the committee's work is a 150-page booklet entitled "The Training of a Doctor," into which is poured the views, not only of the committee, but of some 90 experts in medical education, together with the wisdom contained in a not inconsiderable volume of literature previously published.

The report compels admiration. Not only is it expressive of an immense amount of hard work; not only is it illumined by wholly non-partisan opinions; but it is written in a style, and with a clarity, that entitles it to an established place in medical literature.

In the committee's view, the aim of the medical curriculum is to train students in both the science and art of medicine. The aim of medical education is not to impart to the student a mass of factual information in each branch of medicine, but to equip him with sound basic principles, including a scientific outlook and method, a knowledge of the fundamentals of the medical sciences, competence in, and understanding of, certain indispensable techniques, and an intellectual resourcefulness and initiative in handling of unusual and unexpected situations. The undergraduate medical course should be primarily concerned with the training in those basic principles of medicine which are a necessary foundation for all forms of medical practice.

The method of preliminary choice of medical students, and the qualities necessary for teaching, are both discussed. The selection of students should be in the hands of a small committee, and natural ability for medicine, intelligence, and personal character, are the important factors to be considered in selection. On the reverse side of the picture it is pointed out that ability to teach is often not regarded as a primary qualification in a teacher of medicine. The best teachers are those who infect students with the best habits of thought, the best technique, and the best outlook on life. More of such teachers are needed, for there is at present an inclination for

the teacher to take the easy line of spoon feeding, whereas he should be concerned to encourage the student in logical thought. It is important for the teacher to place the greatest emphasis on diagnosis and all that it implies, and to view all the prevalent mechanical methods of diagnosis, for example by X-rays, in their correct perspective. Systematic lectures are a survival from the time when they had to do duty for the textbook, and are of doubtful value in some subjects. The lecture should be used by the teacher to approach the subject from an individual and new angle, and should be made attractive by the use of visual aids such as films. At the end of it time should be afforded for questions and discussion.

There should be no vocational bias in the pre-university studies of the prospective doctor, who should continue to receive a general education until he reaches the university. There he should spend the first year on the further studies of the sciences on which medicine is based, and endeavour to develop a scientific attitude of mind. Scholarships should be awarded on the basis of general cultural studies instead of on a premature specialised knowledge of a few subjects.

The pre-clinical period, that intervening between the first year and the entrance to the hospital proper, should be reorganised to allow of integration of the teaching of anatomy and physiology. Physiology and biochemistry should form a single unified course. Less emphasis in physiology should be laid on animal experiment. The organic chemistry included in the course should be only such as will help the student to understand the biochemical processes of health and disease. A study of psychology should be included in the pre-clinical time-table, which should occupy 4 terms of 12 weeks, and a month of the summer vacation.

In the clinical period it is stressed that the approach should be based on the conception of disease as a disturbance in the structure and function of the organism due to pathogenic agents of various types. The patient should at all times be considered as a whole, and the general purpose of clinical training should be to teach the student the general principles of medicine, to train him to diagnose and treat common ailments in minor medicine and surgery, and to recognise conditions for which he should summon more expert help. The object of undergraduate teaching is not to train general practitioners, but to provide the student with a foundation for his later career in any branch of medical practice. The curriculum should be planned and organised by a committee composed of all the senior members of the teaching staff. The proposal that general practitioners should participate in general undergraduate teaching ?

is rejected. There should be more whole-time teaching appointments, and the conditions of part-time teaching should be sufficiently attractive to prevent its being subordinated in importance to the calls of private practice.

The syllabus should allow the student leisure for both relaxation and reflection, and the teacher should not only take a personal interest in the progress of his students, but should himself be provided with facilities for training in the art of pedagogy. It is recommended that on entry to the clinical period the student should undergo a six months' introductory course—called the transition period—the object of which should be to lead the student from his study of the normal man to the study of deviations from the normal, and the general principle underlying the practice of medicine. Pathology and bacteriology should not be taught in lumps, but should be spread out over the whole of the clinical years. Medical clerkships and surgical dresserships should each be divided into two periods of three months, out-patient teaching being concentrated in the second period in each. There should be a compromise between the waste of unnecessary time in the operating theatre and no attendance at all, the student only attending operations on his own patients. Orthopædics and other larger branches of surgery should not be divorced from general surgery, and the importance of working in the Casualty Department is stressed. In the highly specialised branches of surgery, it is sufficient for the student to see something of the scope, achievements and methods. The student should be assigned to an anæsthetist for instruction during his surgical dressership. It is stressed that obstetrics is of relatively greater importance to the medical student than gynaecology, and a clerkship in obstetrics should be of two months, and gynaecology one. The student should deliver at least twenty normal cases, and efforts should be made to secure sufficient domiciliary experience for him. There should be a close liaison between the teaching of obstetrics and pediatrics, which latter should be studied after obstetrics and gynaecology.

The study of psychiatry should pervade the whole of the clinical period, and should include systematic lectures, and attendance at extra-mural psychiatric centres should be included in the course. Instruction should also be given throughout the clinical period in social and industrial medicine, and public health. Suggestions as to the scope of teaching required in forensic medicine, ophthalmology, oto-rhino-laryngology, dermatology, venereal diseases, physical medicine and radiology are included.

With regard to the time factors in the clinical period, a minimum

of 8 weeks' holiday should be provided during each clinical year, which should consist of three terms of 13 weeks each; one clear term before the final examination should be left free for revision. Suggestions as to the total number of lectures or lecture demonstrations in each subject are given in detail, as are the appropriate amounts of time to be spent in general medicine and surgery, and with the special departments.

The report deals in some detail with professional examinations, and draws attention to the criticism that examinations tend to dominate the work of both teacher and student, and that the kind of test set encourages cramming and reliance merely on memory work. In the committee's view the examination system adopted in medical schools is wrong both in conception and arrangement, and is not the most efficient test of a student's knowledge, or of his capacity for understanding his subject. Well-conducted and well-designed examinations are, however, considered to be an essential part of the machinery of education. The question is discussed of who should examine the student, and whether the examination should be by examiners who are wholly internal or wholly external, or both internal and external. The last is thought to be the best. There should be no professional examinations between an examination in general pathology at the end of the transition course and the completion of the full curriculum—a period of $2\frac{1}{2}$ years. The student should then take the whole of the final qualifying examination at one sitting. The main purpose of the clinical examination should be to test the candidate's powers of observation, and his ability to correlate and assess his finding in arriving at a diagnosis. Six precepts are given for examiners. Perhaps the most important is number three. "Try to find out what the candidate knows. If you find a serious lacuna in his knowledge there is no need to plumb it to its depth; change the subject."

The committee endorse the proposal for compulsory pre-registration resident appointments. The aim of the intern year would be to consolidate the basic training given, and should be devoted largely to an integrated study of patients and not be divided into medical and surgical appointments. The intern should be paid a salary, and there should be one to every 20–25 hospital beds. Admission to the medical register should follow the recommendation from the hospital at which the student has spent his internship, and there should be no further examination.

In conclusion, the committee recognises that, with present staffs and hospital facilities, many of the recommendations cannot be

implemented, and that some of its suggestions call for experiment, the results of which may determine future patterns of medical education. The results of any experiments should be made available to all concerned with undergraduate teaching. An Association of Teachers in Medical Schools should be formed so that various experiments in medical education could be discussed, and a planned assault on many unsolved problems could be designed and made.

The report, which is adorned with many apt quotations, closes with Carlyle's words ". . . Hadst thou not Greek enough to understand thus much : The end of Man is an Action and not a Thought, though it were the noblest ? "

A STUDY OF FUNGI. By C. T. INGOLD, D.Sc., F.L.S., Professor of Botany in the University of London, Birkbeck College. Being a Review of *The Fungi*, Vols. I and II, by FREDERICK A. WOLF and FREDERICK T. WOLF. [Vol. I: pp. x + 438, with frontispiece and 153 figures; Vol. II: pp. xii + 538, with frontispiece and 82 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. Vol. I, 36s. net; Vol. II, 39s. net.)

Good advanced text-books have a profound effect on the development of a science, and Mycology, such a vigorous and important branch of Botany, is urgently in need of a sound work dealing with fungi at an advanced level. Many students of fungi have looked with envious eyes at Fritsch's magnificent two volumes on *The Structure and Reproduction of the Algæ* and have hoped that fungi might receive similar treatment. However, there probably is no one with the necessary knowledge and perseverance to attempt a similar herculean labour for Mycology, and a comparable work would greatly exceed two volumes.

The production of a new two-volume book on "The Fungi" naturally arouses considerable interest amongst mycologists, especially those concerned with university teaching.

The first great text-book of Mycology was de Bary's *Vergleichende Morphologie und Biologie der Pilze, Mycetozoon, und Bakterien*, published in 1884 and made generally available to British and American students by an excellent English translation produced in 1887. So much has become known about fungi during the past sixty years that the book is now largely out-of-date, but it stands as a noble model of what a text-book should be—a model which should be studied by all who contemplate the production of a new text-book on fungi.

The writer of an advanced text-book owes a duty to his readers, especially as so many of them are likely to be students at an impressionable stage of their scientific career. The book must be well-

written and well-illustrated, balanced, stimulating, up-to-date and, above all, accurate. This duty was performed in full measure by de Bary; the present work fails to fulfil these conditions.

The two volumes under review are profusely illustrated, mainly with black and white drawings and diagrams. "References to illustrations are omitted from the text," so says the preface. This is an extraordinary policy, but perhaps it is as well that the unfortunate student should not study some of the figures too closely! "Most of the illustrations," we are told, "are adapted from those of others"—"distorted" would be a better word. Thus Fig. 20, Vol. I, of *Synchytrium endobioticum* is distorted from Curtis so that the hypertrophied cells of the host epidermis pass as sporangia, and in Fig. 22, distorted from Blackwell, biflagellate spores are shown escaping from a sporangium of *Blastocladia*. Again the figure of the sporangiophores of *Rhizopus* (Fig. 35, Vol. I) would certainly not get many marks if it appeared in a student's answer to a question on Mucoraceæ.

The two volumes are so different that each must be considered separately. The first deals with developmental morphology and taxonomy. It does not claim to be exhaustive, certain families being deliberately omitted, mainly, as the authors say, "because they are so little known." This would hardly seem to warrant the omission of any mention of Drechsler's work on Zoopagaceæ. A still more extraordinary omission is that of Winge's work on yeast, certainly one of the greatest mycological achievements of this century. In the author indexes of the two volumes Winge's name figures only in connection with his observations on marine fungi, and the account of yeast in Vol. I omits all mention of haplophase and diplophase in *Saccharomyces*.

The systematic treatment of Fungi in Vol. I will not commend itself to many students. Perhaps the general quality of this volume may be illustrated by taking an example: the section on Chytridiales. The whole presentation of the order is completely muddled. We read "the feature of primary importance in the order is the production within sporangia of anteriorly uniflagellate zoospores." Even if we allow "anteriorly" as a mere slip, there is still no account of the detailed structure of the characteristic chytrid zoospore with its single oil drop and posterior flagellum of the whip-lash type. Having limited chytrids to uniflagellate types, it seems absurd to include *Olpidiopsis* which clearly belongs in the Lagenidiales. The authors rightly deplore the general lack of cytological investigations of chytrids and then fail to cite Wager's paper on *Polyphagus*, probably the greatest contribution ever made

to the cytology of these fungi. The Cladochytriaceæ are said to be "characterised by the possession of a rhizomycelium which is typically limited to a single cell." If the student wishes to discover what is a "rhizomycelium," a term introduced by Karling, he may turn back seven pages where he will find it wrongly defined. Further the rhizomycelium of the Cladochytriaceæ is rarely limited to a single cell. In connection with *Synchytrium endobioticum* we are told: "The outer host cells are occupied by the pathogen in the vegetative phase. It is an unwallled protoplast (plasmodium) that is able to penetrate host-cell walls, meanwhile increasing in volume. This protoplast then forms a wall about itself and cleaves into a cluster (sorus) of sporangia. These sporangia are of two kinds: thin-walled ones that germinate at once, and thick-walled ones, called resting sporangia, that can only germinate after a period of dormancy." To anyone familiar with the life-story of this fungus the gross inaccuracy of this needs no further comment. In the last sentence of the section on chytrids is the statement "*Macrochytrium* . . . may well be a link between Chytridiales and Leptomitales." Presumably the authors mean Blastocladales and "Leptomitales" is just another slip. Finally the figures that decorate the section are as unfortunate as the text. Fig. 16 bears the legend "variety in form of chytrids." Of the four species figured only one, *Rhizophidium globosum*, would probably be assigned by most modern authorities to Chytridiales.

This detailed consideration of a small section of the systematic volume is enough to illustrate the quality of the whole.

Vol. II consists of a series of essays on special aspects of Mycology, and the table of contents is distinctly attractive. There are chapters on various aspects of the nutrition and biochemistry of fungi, on spore germination and dissemination, on problems of plant pathology such as penetration and physiologic specialisation. Fungal genetics are considered, so also are medical mycology, soil fungi, fungus-insect relationships, marine types and even fossil fungi. But chapter after chapter proves a disappointment: too much is attempted, too little is achieved.

The authors aver that their efforts have been "concentrated on helping the student to understand fundamentals." What, then, is the value of the chapter on the "Effects of Temperature," which is largely a disjointed catalogue of the empirical results of many investigators, or of the chapter on Fossil Fungi, chiefly a list taken from Hirmer's *Handbuch der Paläobotanik*, and much the same sort of criticism can be levelled against the chapter on Marine Fungi.

From early times the larger fungi have attracted attention as

things to eat—or to avoid. Even to-day the first reaction of the intelligent layman on seeing an agaric is: "Can you eat it?" It is not, therefore, surprising that Wolf and Wolf should have a chapter on "Poisonous and Edible Fungi." The mycologist, unlike the general public, is well aware that most toadstools are edible, and that their bad repute is due largely to the deadly poisonous nature of *Amanita phalloides*, the death-cap, which is responsible for the great majority of deaths from fungal poisoning. One would have thought that Wolf and Wolf would have taken care to present a good figure of this species so that it might be recognised by students. Instead, their Fig. 58 bearing the legend "Some common poisonous fungi" shows five species: A, supposed to be *Amanita phalloides*, is certainly not that species but probably *A. muscaria*, and E is *Morchella esculenta* which the present writer had previously supposed to be edible.

In the past decade the genetics of fungi has become a flourishing branch of Mycology, springing from the fundamental work of B. O. Dodge in America, using *Neurospora* and Winge's researches in Denmark on yeast. In their chapter on Genetics Wolf and Wolf ignore Winge's work, but that is not the worst feature. Fig. 55 is a "schematic representation of sex segregation at the third nuclear division in *Neurospora sitophilla*." In accordance with the general policy, no reference is made to the figure, and the reader can only suppose that third-division segregation is an established fact. An outstanding result of work on fungal genetics is that such third division segregation has never been demonstrated.

As a final example of the quality of Vol. II the chapter on Spore Dissemination may be briefly considered. The important question of dispersal of spores around a centre of origin, of so much interest to plant pathologists in recent years, is largely ignored and no reference is made to Gregory's valuable contribution to this subject. In connection with animal dispersal we are informed that "a number of fungi, notably species of *Pilobolus*, *Sordaria*, *Panæolus*, *Anellaria* and *Coprinus* normally occur on dung and are regarded as coprophilous." No doubt whales, because they occur in the sea, should be considered as marine. In connection with the violent discharge of basidiospores we read: "Concerning the force employed to discharge each spore Buller believes that it is caused by surface tension. From ingenious experiments and from calculations he found that the surface energy on a drop of exudate on the spore of *Psalliota campestris* is 0.000012 erg." Neither by ingenious experiment nor by calculations did Buller ever reach any such conclusion. The reviewer cannot leave this chapter without protesting against

Fig. 29, distorted after Buller, and Fig. 31, distorted after Ingold.

Perhaps the best that can be said for Vol. II is that the list of references at the end of each chapter may prove of value to advanced students.

The work is beautifully produced and is, in that respect, a credit to the publishers. But its price in this country (£3 15s.) is very high even for an American publication.

REVIEWS

MATHEMATICS

Methods of Algebraic Geometry, Vol. I (Book I: Algebraic Preliminaries; Book II, Projective Space). By W. V. D. HODGE, M.A., F.R.S., and D. PEDOE, B.A., Ph.D. [Pp. viii + 440, with 46 figures.] (Cambridge: at the University Press, 1947. 30s. net.)

ALGEBRAIC geometry has been undergoing critical examination from the standpoint of modern algebra, and is emerging with a new standard of rigour and with emphasis on fresh aspects. Briefly, one pays more attention now to the minimum assumptions necessary for the truth of a theorem; it is notable that, in this process, the field of complex numbers loses its traditional place as the ground field from which numbers are taken.

The present volume is the first of an important new text on algebraic geometry which will probably have considerable influence on the teaching of the subject. It is divided into two books, the first of which lays a foundation of modern algebra suitable for the geometry which follows. English readers, whose gift of tongues is less than pedantry requires, will be glad that the authors have made available this section on algebra. The chapters are headed: I, Rings and Fields; II, Linear Algebra, Matrices, Determinants; III, Algebraic Dependence; IV, Algebraic Equations.

Book II may be described as dealing predominantly with the geometry associated with linear equations or linear dependence. Chapter V is devoted to an algebraic theory of projective space. This is followed, in Chapter VI, by an independent synthetic approach, which is then linked with the algebraic theory by means of the concepts in Chapters I, II. Partly for this purpose, the theory of matrices was developed over a non-commutative field. Chapter VII on grassmannian co-ordinates deals with the spaces of dimension k lying in a space of dimension n and investigates a set of quadratic relations connecting the co-ordinates of such spaces. The theory and classification of collineations and of correlations occupies Chapters IX, X.

Everything is done for n dimensions; this tends to make the reading difficult for a beginner, but the effort required is much reduced by familiarity with the ideas involved when $n < 3$; lectures will help here, and, no doubt, the reader will often interpret the text with a specific value for n in his mind.

The requirements of rigour are often severe in their demands on students, for example, in the matters of multitudinous definitions and notations; the authors of the present volume have been fairly kind in this respect.

The reviewer regrets that there are so few historical notes. Students reading at this level will often wish to consult original papers related to the text.

Volume II is to deal with the theory of algebraic varieties and with the

study of certain loci which arise in many geometrical problems. It will be looked for with great interest.

J. W. A.

Theory of Functions. - By J. F. RITT. Revised edition. [Pp. 181.] (New York : Kings Crown Press ; London : Oxford University Press, 1947. 16s. net.)

THE aim of this book is to present the basic material of an undergraduate course in the theory of real and complex functions. The real numbers are developed as infinite decimals by a method which is equivalent to a Dedekind section of the rationals whose denominators are powers of ten, but which may be less frightening to beginners than the usual theory. Continuity and derivatives are developed along standard lines ; the constructive definition of the integral is Riemann's, not Darboux's. Uniform convergence and functions of several variables are also touched on. The general impression is one of precision without pedantry, achieved partly by not pursuing any one topic at great length (the real variable section covers less than sixty pages). After adding that the author has a pleasant style, a high standard of rigour and a consistency in the use of terms which students could be recommended to emulate, it should also be stated that the book is more nearly a collection of lectures than a systematic text. In so far as theorems make up a subject, the work is well done, but the applications are another story. This is most serious in the theory of functions of a complex variable, which takes up two-thirds of the book. No previous knowledge of complex numbers is assumed, and the theory is carried just beyond Weierstrass' factorisation theorem. Cauchy's theorem is proved in turn for a triangle, a polygon, and a rectifiable curve in a simply connected domain. The topological difficulties are not laboured, nor are they ignored. But not a single contour integral is worked out in the text. It is quite possible to follow the whole work and not know how to evaluate a real integral by complex methods, or calculate a residue, or factorise a function like $\sin z$ or deal with a bilinear transformation. These are serious matters which will lessen the value of the work. So will the absence of an index.

The format of the book is a novelty. It is lithographed from a varityped original, and its pages are bound in a loose-leaf style that must have contributed to the low price of the book, but which may cause librarians to hesitate before adding it to their shelves.

H. K.

A Text Book of Mathematical Analysis : The Uniform Calculus and its Applications. By R. L. GOODSTEIN. [Pp. xii + 475.] (Oxford : at the Clarendon Press, 1948. 30s. net.)

THIS book claims attention as a calculus and as an introduction to analysis, but [it will probably be judged on its attitude to the fundamentals of analysis. In an essay which appeared in 1939, A. Heyting, defending the Intuitionists, wrote : "Souvent les mathématiciens nous demandent de formuler exactement nos axiomes de logique et de mathématiques ; ensuite on pourrait étudier ce système d'axiomes au même titre que d'autres systèmes. C'est là une position que nous ne saurions accepter. Pour nous, les mathématiques consistent en une activité intellectuelle spontanée ; l'expression par la parole ou par l'écriture, quoiqu'indispensable pour la communication, n'est jamais

adequate." One must be grateful to the author of the Uniform Calculus for having revealed, if not the axioms, at least some of the main features of this system.

As a calculus, the book is an accurate account of a substantial part of a two-years honours course. Its real numbers are infinite decimals, it defines exponential and trigonometric functions by series, it has an interesting chapter on differential equations (the uniqueness being well handled by the Wronskian), and it has a large collection of examples, many worked.

E. T. Bell divides mathematicians into two classes: the "we can find" and the "there exists" men. The author of the Uniform Calculus is a radical "we can find" man. It is unlikely that the student who takes up the book will realise that its phraseology and selective logic are deliberate. This is a pity, for, although the author disclaims any intention to solve philosophic problems in the foundations of mathematics, it is only by knowing what these problems are that the reader can judge the work. Why are proofs by *reductio ad absurdum* rejected, why is it better to define differentiability in terms of intervals rather than points, why is it necessary to give a serious proof showing that if two functions are equal so are their derivatives? The author is anxious to put mathematical rigour within the reach of the non-specialist; in his judgment the obstacle is the "depth and wealth of abstraction on the threshold of classical analysis," and his remedy is to stick to the certainties, the calculables. But this is a dubious course. The physicist struggling with thermodynamics is not worried by real numbers: it is a rational account of differentials (not mentioned in this book) that he needs. When he defines a quantity as the limit of a sum, he has no intention of sacrificing his intuition that area is $\sum y dx$: he wants a rational account of why the "second order" quantities are neglected. In other words, it is rigour in relation to a set of postulates more or less completely understood that he wants; and this after all is the reasoned attitude of many mathematicians from Hilbert downwards. There may be good reasons why the author takes $\int (xy - yx) dt$ as his basic formula for area and then elaborately deduces $\int y dx$, but the scientist will probably miss them.

In regard to the specialist student of mathematics, the use of decimals rather than sections is a matter of detail; the real novelty is the outlawing of all definitions which are not "constructive." A convergent sequence in this book is what is usually called a Cauchy sequence ("we can" replacing "there exists") and in a seven-page appendix it is proved that such a sequence has a limit. This admits numbers defined by the classical method of repeated subdivision, but not bounds. If $0 < x_1 < x_2 < \dots < x_n < \dots < 1$, one cannot assert that the sequence converges, presumably because in the logic admitted by the author one cannot say of a given number whether it exceeds all the x_n or not. Continuity of a function is taken to mean uniform continuity in an interval; this implies boundedness, but not the classical proposition that a continuous function in a closed interval has a maximum. The theorem that a continuous function which assumes positive and negative values also has a zero manages to survive, thanks to a certain deviation from the straight path of finitism. Differentiability is also tied to uniformity, so that for the author every derivative is continuous. This of course facilitates the proof of the mean value theorem; the irrelevance of continuity, in the classical formulation, is ignored. The constructive definition of the integral is naturally nearer to Riemann than Darboux, but again continuity dominates the discussion.

Enough will have been said to show that for a mathematical specialist this book presents a truncated version of mathematics; as a preparation for post-graduate studies, theory of sets, integration, topology, or calculus of variations it is not without its dangers; a specialist nursed with so much Uniformity may find weaning a very troublesome process.

H. K.

Five Figure Tables of Natural Trigonometrical Functions.

Prepared by H.M. Nautical Almanac Office. [Pp. iv + 123.] (London: H.M. Stationery Office, 1947. 15s. net.)

THESE tables were prepared by the Nautical Almanac Office in 1941 at the request of the War Office for use in survey calculations, but were not made generally available to the public until 1947.

The "main table" contains values of the sine, tangent, cotangent and cosine arranged semi-quadrantly for every ten seconds of arc ($10''$). Five decimals are retained throughout in the sine, tangent and cosine, but the values of the cotangent have been given to five significant figures only in most of the range; from 27° to 45° five decimals have been retained.

The "auxiliary table" gives values of the cotangent for every second of arc up to $7^\circ 30'$ to five significant figures.

The characteristic feature of the table is the small interval of $10''$, novel for five-figure tables of naturals. It has been constructed particularly for work in surveying and associated problems and, in providing naturals, is intended mainly for use in conjunction with a calculating machine. With the small interval no interpolation between the tabular entries is necessary to the accuracy normally required in field survey work, but, if desired, interpolates to tabular accuracy can be obtained by sight for sine, cosine and tangent.

Layout and typography are pleasing, but the format is rather large, the page size being 9.5 in. by 12 in.

H. O. HARTLEY.

Theoretical Aerodynamics. By L. M. MILNE-THOMSON, M.A., F.R.S.E.

[Pp. xx + 364, with frontispiece and numerous figures.] (London: Macmillan & Co., Ltd., 1948. 40s. net.)

It is now over twenty years since Glauert published his *Elements of Aerofoil and Airscrew Theory*, which has been, for many mathematicians, their introduction to the important subject of Aerodynamics. Since then there have been published a number of other books on the subject, most of which have had an appeal rather to the engineer, and of course we have had the Durand Collection. But there has been of recent years a very great need for a single-volume treatise which could be given to the theoretical student. Prof. Milne-Thomson has satisfied this need in an admirable manner.

It is always difficult to know how, in a first book, to introduce the many new ideas and concepts that are necessary to appreciate a growing subject; when, for example, to speak of boundary layer and viscosity. The author has in this book introduced them at the start, and this seems to be the right place, particularly when they are propounded with such clarity. After a chapter on Bernoulli's Theorem, the reader finds a comprehensive account of two-dimensional flow and, in particular, a whole chapter devoted to the Joukowski Transformation. This perhaps is the only point to criticise, but maybe it is justified on historical grounds. The chapter on thin aerofoils

is particularly of interest. Three-dimensional theory and applications follow and special chapters devoted to Propeller and Wind Tunnel corrections. It is pleasing to find a well-written chapter on supersonics: a topic that is of great importance today. The student should have no difficulty in appreciating the fundamentals here outlined.

Prof. Milne-Thomson concludes, unusually, with a chapter on vectors, which have been used where required throughout the book. The complex variable has been employed where possible and the necessary fundamental theory incorporated in the text. Only a few references to original papers are given and an increase here would be desirable. Numerous examples, many of them practical, are given at the end of each chapter.

The production and layout of the work reflects much credit on the publishers, and readers familiar with the author's book on Hydrodynamics will recognise the same value in this companion volume.

R. C. K.

Mechanics. By J. C. SLATER, Ph.D., and N. H. FRANK, Sc.D. International Series in Pure and Applied Physics. [Pp. xiv + 297, with 44 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 20s. net.)

THE publication in 1933 of Slater and Frank's *Introduction to Theoretical Physics* set a new standard for text-books in that field. The basic theory of classical and quantum physics was brought together in one volume, thus bringing out, in a masterly fashion, the unity of the whole. Classical mechanics was no longer treated as a subject developed for the express purpose of solving the problems appearing on mid-nineteenth-century Cambridge Tripos papers, but as a subject important as the fundamental basis of electromagnetic theory, quantum mechanics, and of all theoretical physics.

A new work is now being written to replace the 1933 volume. In this there will be three volumes, one on mechanics, another on electromagnetism, and a third on quantum theory. The work at present under review is the first of this trilogy. The authors explain in their preface that they believe, with the new arrangement, they have been able to give a somewhat more rounded treatment of each of these fields, while still preserving the general unity of the whole subject.

The present work covers in thirteen chapters the sections on dynamics, vibrations and waves, elasticity, and hydrodynamics, of the original work, but these have been considerably enlarged, revised, and re-written. The lucidity and conciseness which made reading the original work so enjoyable are also a feature of the present volume. No occasion is missed of pointing out where methods developed for the solution of mechanical problems are also applicable to widely different branches of physics.

There are a considerable number of problems at the end of each chapter, but these are designed to illustrate points of principle or to bring out new concepts rather than to give the student extensive practice in manipulation.

There are eight useful appendices on various mathematical topics, including numerical solution of differential equations, complex numbers, vectors, tensors, Fourier Series, Bessel's Functions, etc. These appendices are extensions of the early chapters of the original work, and it appears an improvement to have collected them at the end rather than interrupt the development of the main theme.

There is no doubt that this new version of the *Introduction to Theoretical Physics* is destined to play just as important a role in the training of the theoretical physicist of the future as the original work has done during the past fifteen years.

E. H. S. B.

Theory of Servomechanisms. Edited by H. M. JAMES, N. B. NICHOLS and R. S. PHILLIPS. [Pp. xiv + 375, with 159 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. \$5.00.)

THIS book is a collective work by several authors associated with the Radiation Laboratory at the Massachusetts Institute of Technology. The chapters cover the theory of servomechanisms in two main divisions, the first devoted to the sinusoidal steady-state theory, and the second to the statistical methods of analysis which are particularly helpful in the consideration of systems in which the input data is contaminated by unwanted interferences. To each part there is an ample and clear mathematical introduction. The treatment is of advanced standard.

In the first part of the book servo components, the theory of design, and the theory of pulsed data systems are treated. Reference is made to practical electrical designs for purposes of illustration. The second part contains a consideration of the errors which arise from the nature of the input signal, and from random disturbances in the error-measuring and amplifying systems. Methods of design based on statistical considerations, whereby the root-mean-square error in the output is minimised, are proposed and are illustrated by examples. The designer will often find an adequate solution to his problem with the aid of the more conventional methods treated in the first part of this book, but the theory in the second part is none the less important in providing an elegant though tentative approach to the difficulties presented by random interferences.

The authors' interests have centred in the applications of servomechanisms in radar devices. They have written broadly, however, and there is much in the volume which will be useful to every designer or student of servomechanisms, though some may feel that, admirable as is the treatment against the radar and electrical background, there are omissions of matters which are of interest in other fields of application. Nevertheless, the work can be described as comprehensive, and the treatment is generally clear and complete.

In places the style and arrangement shows evidence of a haste in preparation to which the authors confess in their preface. However, only slight irritation is thus caused to the reader. The volume can be recommended to the advanced student and designer.

A. A. HALL.

PHYSICS

Intermediate Physics. By C. J. SMITH, Ph.D., M.Sc., A.R.C.S. Third edition. [Pp. xii + 1033, with 709 figures.] (London: Edward Arnold & Co., 1947. 21s. net.)

THIS text-book is too well known to require an extensive note, and, in consequence, the remarks which follow are mainly with reference to the differences between the second and third editions.

There has been some rearrangement of material, an addition of some 133 pages, and, so far as a relatively cursory examination has shown, very few paragraphs in the second edition have been omitted from the present volume. About one-third of the space is, as before, devoted to electricity (including magnetism) and there is now an extended treatment of thermal effects. The sections on osmosis and elasticity have been expanded, and additional paragraphs have been inserted throughout the chapters on heat; but the most noteworthy increases are to the sections on optics and acoustics which now incorporate some of the material which Dr. Smith has recently published in the *Philosophical Magazine*. While all these additions are useful and of interest to teachers, it is to be hoped that the book will not in course of time outgrow its title. The treatment is singularly free from blemishes, and the clear diagrams aid the comprehension of the text. (It may, however, be noticed in passing that the diagram of the field of view of a sextant is misleading, and that hot-wire ammeters do not normally possess uniform scales.)

The paper and binding of the review copy are of "post-war" quality; the former does not appear to be of uniform texture, and the latter tempts one to predict that "jackets off" will soon be a phrase not limited in application to the large number of earnest examination candidates who will welcome the new edition.

E. J. I.

Paramagnetic Relaxation. By C. J. GORTER, Ph.D. [Pp. viii + 127, with 25 figures.] (Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1947. 12s. net.)

PARAMAGNETIC relaxation is closely connected with the problems involved in adiabatic demagnetisation and with the theoretical approach to paramagnetism, the crystalline state and dielectric relaxation. One of the important features of this authoritative monograph is that it presents a coherent account of the hitherto scattered results of both theoretical and experimental studies of the subject. Prof. Gorter has also included accounts of work which has recently been carried out in Leydén, and they contain a great deal of new and interesting information.

The monograph may be conveniently divided into three sections, the first of which (Chapter I) is devoted to a survey of the theoretical basis of paramagnetic relaxation and absorption. Probably owing to limitations of space, this survey is presented as a brief summary only. In the second section, comprising Chapters II and III, is given a critical account of three available experimental techniques, viz. those methods using calorimeters, bridges and heterodyne oscillators. A full description of the results of measurements on a large number of substances, employing these methods, is also included in this section. The theory of normal paramagnetism and the correlation of the theoretical and experimental aspects are discussed in the last section (Chapters IV and V). In his concluding remarks the author clearly illustrates some deficiencies in the theoretical treatment and indicates further extensions of the work, particularly in the realm of nuclear magnetism.

The book is well produced, with the possible exception of two functional circuit diagrams which are rather difficult to follow, and may be warmly recommended to those who are interested in paramagnetic relaxation and associated subjects.

R. STREET.

New Developments in Ferromagnetic Materials. By J. L. SNOEK. [Pp. viii + 136, with 52 figures.] (Amsterdam : Elsevier Publishing Co., Inc. ; London : Cleaver-Hume Press, Ltd., 1947. 13s. 6d. net.)

It is always a keen pleasure to read a booklet which is packed with new ideas and new knowledge, and for these reasons this little monograph is of the greatest importance to all interested in the theory and applications of ferromagnetism. Its publication is a proud witness of the activity of the minds of research workers in Holland during the five years of German occupation.

The work falls into three main parts, with the headings: Statics of Ferromagnetism, Dynamics of Ferromagnetism, and Development of Magnetic Materials. In addition, there are some important appendices, one of which will interest all university teachers of magnetism, for it deals with the ancient and intriguing problem of the overlap between the virgin magnetisation curve and the closed cycle curve for the same maximum field. How many teachers, without a scientific reason for it, have regarded a student's record of such an overlap as evidence of honest work? This is precisely what it is in the case of bar or rod specimens with appreciable demagnetisation factor, for Dr. Snoek gives a convincing argument that eddy currents prevent this factor from having its proper effect whenever there is a trace of time-lag in magnetisation and field changes are made too quickly.

In the first section of the booklet there is a most interesting discussion of magnetostriction phenomena and their theoretical aspects. The second deals extensively with magnetic after-effects (time-lags). The third contains very much new material on the properties of the ferrites, with special reference to the behaviour of zinc as a constituent, and also includes some stimulating remarks about permanent magnet materials. The work is most welcome and can be wholeheartedly recommended to the attention of all interested in modern aspects of ferromagnetism.

L. F. BATES.

Electrons in Gases. By SIR JOHN TOWNSEND, F.R.S. [Pp. viii + 166, with 31 figures.] (London : Hutchinson's Scientific and Technical Publications, 1948. 25s. net.)

It is more than thirty years since the appearance of Townsend's *Electricity in Gases* and, apart from one or two short résumés, one has had to wait till now for a continuation of the account of the work by the Townsend school. Let it be said immediately that this book is not a comprehensive survey of the field, but follows the pattern of the earlier publication; in fact, considering the information now available, its scope is more restricted—almost of necessity because of the wealth of material that has accumulated. The latter half of the book is concerned chiefly with that ever-present problem of the mechanism of the gaseous discharge. The various alternative theories that have been put forward to explain the discharge are discussed fairly fully in the light of recent experiments, but in the end Sir John is not convinced and, on the whole, stands by his explanation first suggested forty-five years ago, although he does admit the relevance of other factors such as the effect on the cathode of ultra-violet and α us ions.

The book is copiously illustrated with tables and curves, but the discussion of controversial issues would be more convincing if the counter-proposals were illustrated with the same attention to detail. Such a presentation does

not permit the reader who is not completely versed in this subject to make a considered judgment.

However, as a reference text the book contains a great deal of valuable experimental data set out in clear form, the value of which would have been increased still further if an index had been included. True, there is a detailed list of contents, but this does not compensate the lack of an alphabetical index.

W. E. DUNCANSON.

Exclusion Principle and Quantum Mechanics. Lecture given in Stockholm after the award of the Nobel prize of Physics 1945. By WOLFGANG PAULI. [Pp. 52.] (Neuchâtel: Éditions du Griffon, 1947; London Agents: H. K. Lewis & Co., Ltd. 4s. 9d. net.)

W. PAULI was awarded the Nobel prize for Physics in 1945 for the discovery of the "exclusion principle" or, as it is mostly called, the "Pauli principle." In its original form the principle states that of all the electrons forming the outer part of an atom no two can occupy one and the same quantum state. This has very important consequences for the theory of spectra and for the understanding of the periodic table of the elements. In the language of quantum mechanics the exclusion principle can be formulated in a much more general way: The wave functions describing the behaviour of a system of electrons must always be "antisymmetric" in their co-ordinates (including the spin co-ordinates). The problem as to whether the principle also holds for other fundamental particles was attacked with ingenious methods, and it was found that it also applied to protons and neutrons, but not to photons. These results are of fundamental importance for nuclear physics on the one hand and for the quantum theory of fields on the other hand. Although the most refined developments in this domain have made it abundantly clear that the exclusion principle is an integral part of quantum mechanics, we are still unable to derive it from quantum mechanics. The present book, in which the discovery and development of the principle is described by the highest authority on it, makes fascinating reading, which is clear and exact everywhere, although it avoids the use of mathematical formalism altogether.

R. FURTH.

Cosmic Rays. By L. JÁNOSSY. The International Series of Monographs on Physics. [Pp. xiv + 424, with 6 plates and 129 figures.] (Oxford: at the Clarendon Press, 1948. 35s. net.)

PROF. JÁNOSSY's book has been eagerly awaited by workers in Cosmic Ray Physics, and its publication now (although delayed by the familiar difficulties of present-day publishing) is most opportune because of the notable advances in the subject since the war. The book is intended primarily for the general reader and the experimentalist; the author's active experience in both experimental and theoretical research gives a particularly illuminating quality to his treatment.

A main feature of the work, which will be invaluable to active workers in the subject, is the provision of detailed treatments of the basic features of Cosmic Rays: of collision theory, particularly in the relativistic region, of the electron-photon cascade in a uniform material and the extension to cascades in the earth's atmosphere, and of the theory of orbits in a magnetic field and the geomagnetic effects. This is material which has not previously

been available in at all comparable form. It is interesting to notice the stress which Prof. Jánosy places on the interpretation problems for data from counter arrays, and the experimental worker will be grateful for the graphical representation of the elastic collision problem in Figs. 27, 28; it is to be regretted that a similar summary is not given of the simple cascade results. The sections dealing with the application of Liouville's Theorem and the Störmer cone are most stimulating, giving in a straightforward form material which has up to now been particularly inaccessible.

These sections, together with an appendix on the statistical treatment of observations (which will also be welcomed by workers in the field), occupy rather more than half of the text. In the remainder, after a balanced historical summary and a descriptive section on the specialised experimental techniques of the subject, most other features of Cosmic Rays are discussed, in particular the meson component and the origin of mesons. The most serious omission is of any general consideration of the time variations of Cosmic Rays. It is inevitable that the time lag in publication should be more in evidence in these sections, but the general reader will probably find the rather conservative standpoint a satisfactory background to further reading.

Points of detail are open to criticism: the adoption of a notation in which no symbol is used with more than one meaning (even in unrelated topics) has cut across many familiar usages and has involved the extensive use of Gothic type, both features making for harder reading. The subject index also is not entirely satisfactory; overcondensed entries, by which the topic concerned is unlikely to be identified, have been allowed to appear. (Will anyone investigate the entry "parent" except from vain curiosity?) Otherwise the production reaches the high standard which has distinguished the series.

J. G. WILSON.

Introduction to Atomic Physics. By HENRY SEMAT, Ph.D. [Pp. xii + 412, with frontispiece and 169 figures.] (New York: Rinehart & Co., Inc., 1946; London: Chapman & Hall, Ltd., 1947. 24s. net.)

In a book of some 400 pages one cannot expect a full treatment of all subjects which might come under the title, but on the whole the treatment is adequate, with certain serious exceptions. For example, the determination of e/m for electrons is brief, only the bare outline of the electric and magnetic deflection method being given, and no reference is made to any modern deflection method. This lack is particularly noticed coming almost immediately after a rather detailed description of the Hopper and Laby determination of " e ," as well as the classical work of Millikan. However, such uneven treatment is not a very serious criticism and is more than compensated for by the general clearness with which the matter is presented and the way in which modern developments in modern physics are described—the treatment of the action of the betatron, to cite but one such example. Clear and instructive diagrams form a notable feature of the book, for example the energy level diagram of atomic hydrogen on p. 203 and the diagram illustrating the molecular beam resonance experiment on nuclear magnetic moments on p. 239.

The book is divided into three sections, *viz.* Foundations of atomic physics, Extranuclear structure of the atom, and the Nucleus. On examination of the contents of the first part it is a little difficult to see the reason for this title. True, it deals with mass and energy, waves and particles, and a

summary of some of the relevant results of electricity and magnetism, but a fairly full treatment of X-rays and an introduction to α -, β - and γ -rays seem to have little place under such a heading. Moreover, the properties of these radioactive radiations are again dealt with about 200 pages later in the third part.

One feature which appeals very much to the reviewer is the relegation to appendices of some of the more involved mathematical derivations, such as the scattering of α -particles, elliptic atomic orbits, and the Compton effect. Such a method decreases the possibility of a student losing sight of the physics by having to follow through a lengthy piece of mathematics; yet, at the same time, the treatment is conveniently available and avoids the suspicion that is too often induced by the phrase "it can be shown."

On the whole the book can be recommended as a clear and up-to-date treatment of the subject.

W. E. DUNCANSON.

Also. *The Failure in German Science.* By SAMUEL A. GOUDSMIT. [Pp. xiv + 259, with frontispiece and 13 figures.] (London: Sigma Books, Ltd., 1947. 15s. net.)

THIS is an unusual and interesting book. Its main title is the code name of a Mission appointed by the United States War Department at the request of Major-General Groves, Army Chief of the Atom Bomb Project (also is the Greek for grove), and sent to follow immediately behind the armies invading Europe to find out precisely how far the Germans had progressed with the development of the atomic bomb and with some other scientific activities. Dr. Goudsmit, Professor of Physics in North-western University, who was the scientific head of the Mission, tells the story of what was found, how it was found, how the relative failure of the German scientific effort gradually and unexpectedly came to light, and discusses some of the lessons which may be drawn.

Before the departure of the Mission, the American and British scientists, having already progressed far and shown the atomic bomb to be a definite possibility, were sure that the Germans must know as much and even more. After all, Otto Hahn had discovered nuclear fission, and other celebrated physicists were still in Germany. Then, for so long, German science had had the highest reputation. At the same time, the German scientists were equally positive that their science was much superior, and that if they had made little progress, the British and Americans must have made far less. It took some time for the Mission to become convinced, and much longer for them to convince those left at home, that the Germans were in fact far behind and hardly within sight of a real atomic bomb. The German scientists believed that the first problem to tackle was the development of a pile which might later be made explosive.

Prof. Goudsmit has a lively style and a lively mind. All scientists will want to read (for example) his account of why a large German organisation collected large quantities of thorium compounds. Everyone will be interested in his analysis of why German science was, with the exception of certain research and development projects for the air force, relatively so ineffective in helping the German war effort. This book is, in many respects, complementary to the recent book by Leslie F. Simon on *German Research in World War II*. Sigma Books Ltd. are to be congratulated on securing the book.

F. A. V.

The Cathode Ray Oscillograph in Industry. By W. WILSON, D.Sc., B.E., M.I.E.E. Third edition. [Pp. xii + 252, with frontispiece and 197 figures.] (London: Chapman & Hall, Ltd., 1948. 18s. net.)

READERS familiar with the previous edition will find that this, the third, differs from it only in comparatively minor respects. Most important of these are the sections on fluorescent screens and on the merits of magnetic deflection, both influenced by the recently published details of radar research. Other recent developments are mentioned in a 4-page appendix, and there are minor textual modifications. The need for three editions within five years is a fair indication of the usefulness of the book in surveying the large field of oscillography.

The subject is widely conceived, including details of the pumped metal tube oscillograph, the "magic eye" tube, the electron microscope, and the diffraction camera, as well as the ubiquitous glass cathode ray tube. Some of the simplifications and generalisations necessarily involved in such a wide survey tend to be rather misleading, but this detracts little from the value of the book in describing the almost infinite capabilities of the oscillograph.

Numerous photographic illustrations and a classification of oscillography by function help to make the treatment clear, and the emphasis throughout is on applications rather than on the theory of operation. The book forms an admirable introduction to the more specialised texts.

H. MARRIOTT.

The Technique of Microwave Measurements. Edited by C. G. MONTGOMERY. Massachusetts Institute of Technology Radiation Laboratory Series. [Pp. xx + 939, with 628 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. \$10.00.)

THIS book has been prepared by a team of authors all of whom were intimately connected with microwave measurement technique as developed during the war years, both in the U.S. and in this country.

For the reader with a good background of knowledge in the microwave field and familiar with fundamental principles the book is excellent. The ground covered is wide, dealing with frequencies upwards from 1000 Mc/s, but the subject matter is presented in a most illuminating and satisfying way, so that few queries are left without an answer embodying the full extent of knowledge as it is today.

Special emphasis is given to practical experimental methods applicable in the laboratory, and the theoretical arguments on which these methods are based find a useful place in the treatment. Although the work is a composite one, the Editor has secured a high degree of coherent and systematic development of the subject matter, with little to indicate the changes of authorship between one chapter and another. Constructional details of the many different types of apparatus employed are of particular importance in ensuring successful measurements, and a feature of the book is the excellent illustrations which accompany the descriptive sections. There are four main parts into which the work is divided: (i) power generation and measurement; (ii) wavelength and frequency measurements; (iii) the measurement of impedance and standing waves; (iv) attenuators and radiation measurements. The first part includes a study of Klystron oscillators with frequency stabilisation arrangements, followed by a discussion of power measuring devices, and the application of microwave signal generators. In the second

part wavemeters, resonant cavities and spectrum analysers with ancillary equipment are considered. Part three is a most valuable record of the technique of impedance measurement by standing wave indicators and in particular by bridge systems. Arising from this discussion special attention is given to the determination of dielectric permittivities. The fourth and last part necessarily deals with power measurement again, but the point of view is very different from that of the opening chapters and fully justifies separate treatment. Attenuators of all kinds are considered in detail, and a final section is devoted to methods of examining the field distribution associated with an aerial array.

The book can be recommended without hesitation to all practical workers in this field and especially to those with a good knowledge of basic principles. It is not designed as an introduction to the subject, and is therefore scarcely the kind of treatment that a teacher would require for elementary instructional courses, but there is a strong objective approach which one cannot but admire.

Some of the data collected together are of a design character and to the experimentalist engaged on measurements requiring individual consideration that is often a great asset. There can be no doubt that the authors, the editor and the publisher have jointly done a thoroughly praiseworthy job.

H. M. BARLOW.

Radar Beacons. Edited by ARTHUR ROBERTS. [Pp. xx + 489, with 246 figures and 12 tables.] (New York and London : McGraw-Hill Book Co., Inc., 1947. \$6.00.)

THIS is Volume 3 of the Radiation Laboratory Series incorporating the results of the intensive war-time research and development conducted by the gifted team assembled at the Massachusetts Institute of Technology. Its subject matter is the beacon, narrowly defined as a device that emits automatically a pulsed radio signal when "interrogated" by a pulse signal. It does not include, therefore, the subject of radar navigational aids employing beacons that transmit independently of any active interrogation process.

With twenty-four contributing authors, the volume has little uniformity of style or aim and very much repetition. The problems peculiar to beacon design and operation occupy less than a quarter of the book, the remainder being a rather superficial treatment of matters extracted from the detailed treatment in other volumes of the series. These extracts are unfortunately not always those most relevant to the main topic.

Within its limitations the book gives on the whole an accurate account (perhaps the only comprehensive account yet available) of the principles and operating characteristics of beacon systems, and the design of beacon and interrogator equipment. It is a book for the specialist, who will select from it what is of importance, rather than for the general reader.

H. MARRIOTT.

Electronic Circuits and Tubes. By the Electronics Training Staff of the Cruft Laboratory, Harvard University. [Pp. xxiv + 948, with 754 figures.] (New York and London : McGraw-Hill Book Co., Inc., 1947. 45s. net.)

VERY few individuals have the ability, inclination, and time to write a comprehensive volume describing the circuits and electron valves used in telecommunications. The alternative solution of co-operative authorship is well

represented by this volume, written by eleven members of the lecturing staff of the Harvard Graduate School of Engineering.

The most succinct survey of the subject matter is provided by a list of the chapter titles, which are, after some grouping:—A.C. Circuits and Response, Networks and Impedance Matching, Coupled Circuits, Circuit Elements and their Measurement, Filters, Transients, Fourier Analysis of Signals, Emission and the Diode, Multi-element Valves, Cathode Ray Tubes, Voltage and Power Amplifiers, Oscillators, Gas-filled Valves, Rectifiers, Modulation and Detection, Test Instruments, Radio Receivers, Timing Circuits, and four Appendices. Applications of principles and illustrations of theory are always drawn from the field of radio communication rather than from line communication or industrial electronics, but this bias has not greatly affected the choice of fundamental material.

In the diverse authorship of the volume lies both its strength and its weakness. Each author is to some extent a specialist, and the chapters he writes, though chiefly concerned with unspecialised basic theory, derive considerable value from this specialist background. In most parts, for example, the treatment is rigorous and careful, and where approximations are found necessary they are introduced as such and their magnitude estimated. A real attempt has also been made to ensure uniformity of nomenclature throughout the book, and there is a commendable adherence to carefully defined conventions of sign and direction in the A.C. circuit analysis. There is unfortunately no such uniformity of style, and two or three sections have a repellent, patronising air. All the diagrams of the "flip-flop" circuit and its derivatives, for example, are presented in an unfamiliar rhombic pattern merely to support a tenuous analogy with the Wheatstone Bridge, the only real point of similarity being that both circuits are prototypes of numerous developed circuits: perversity could hardly go further. About four-fifths of the volume are written as a theoretical text, but the remainder strays into the fields of the design manual and the factual reference handbook.

There are also some half-dozen errors in page and diagram references, and a similar number of misleading or obviously incorrect technical statements. One could wish that there were less, but can be thankful that there are not more in a first edition of this scope.

In spite of its defects the book deserves, and will no doubt enjoy, wide use as a general textbook in electronics, particularly in view of its emphasis on fundamental principles. In its treatment, its detail, and its scope the book is very suitable for the undergraduate electrical engineer specialising in electronics, and with its companion (but rather less-satisfactory) volume, *Transmission Lines, Antennas, and Waveguides*, from the same source, constitutes a complete course in radio communications engineering to degree standard.

H. MARRIOTT.

Radio Aids to Navigation. By R. A. SMITH, M.A., Ph.D., A.M.I.E.E. Modern Radio Technique Series. [Pp. xii + 114, with 27 figures, including 7 plates.] (Cambridge: at the University Press, 1947. 9s. net.)

THIS monograph is the second to be published in a new series dealing with some of the spectacular advances made in radio technique during the war years. For the most part the author describes the remarkable developments

which took place at the Telecommunications Research Establishment of the Ministry of Supply following the successful application of pulse technique to aircraft detection and location. Although the title might be considered to embrace a rather wide field, the older methods of position finding with the aid of medium- and high-frequency direction finders, and their associated beacon developments, are dismissed in a short chapter of six pages. Furthermore, while the two chapters on "Long-Range Navigational Aids," dealing with Loran and C.W. Systems respectively, do refer to some marine applications, the book on the whole is concerned with the navigation of aircraft, such navigation including the needs of precision bombing, aerodrome approach and landing by the aid of radio instrumental technique alone. Even within this field no more than a passing reference is made to systems developed and used by the Germans, except where these have later been found to have some application to post-war civil needs.

Within the limitations described above, Dr. Smith has provided an interesting account of the radio navigational aids developed primarily in this country. The style throughout is descriptive, and in parts almost of an historical narrative nature; and to the reader with some general knowledge of the subject the monograph forms a most useful and concise survey of this particular field of development. The author has quite rightly omitted all circuit diagrams and technical details; but sufficient values are given of the performance of the various systems to enable an appreciation to be made of the relative merits and fields of application of these navigational aids, some of which are now being adapted to the needs of civil aviation and merchant shipping. It is very satisfactory to see material which has hitherto been "buried" in confidential and secret documents collected together and presented in this way for the general scientific reader. For the benefit of those who wish to delve deeper into the subject some references are given to recent detailed publications, many of which are the result of the Radiolocation and Radiocommunication Conventions held by the Institution of Electrical Engineers in 1946 and 1947 respectively.

R. L. SMITH-ROSE.

Radio Engineering. Vol. I. By E. K. SANDEMAN, Ph.D., B.Sc., M.I.E.E. [Pp. xxiv + 775, with 280 figures, including 5 folding plates.] (London: Chapman & Hall, Ltd., 1947. 45s. net.)

THE publication of a book of this length can hardly fail to be an interesting event in the radio world. The title is a little unfortunate, since it is already associated with one of the best-known textbooks and also because the present book has such a bias towards the transmitting side of broadcasting that a title which better indicated its scope would have been preferable.

The book has the merits and faults of a book by a practising engineer. The valve characteristics, etc., belong to actual types; there are useful charts; the examples are for the most part practical, and reference is made to the specifications which the apparatus has to meet.

On the other hand, the elementary part is put in a strange order and the real beginner would be very confused. For example, we are led a long way through elementary electrical theory, the concepts of force, energy, etc., being freely used, and then these are defined on page 80 and onwards. The references to practice would not in some cases be understood by the beginner (see page 57).

One gets the impression that the author has a certain type of reader in mind—one who has been taught "by numbers" to carry out a routine job and now wishes to understand more fully what he is doing. Some details of a kind which are best confined to working instructions are given at length; for example, the list of B.B.C. transmitters and most of Chapter 15.

The distinguishing feature of the book is the very thorough treatment of power amplifiers and modulators. The production of the book is excellent, with a wealth of well-drawn figures and charts. It will undoubtedly be of considerable use to those working on the broadcasting side of radio engineering.

C. R. S.

Introduction to Wireless. By W. E. PEARCE, B.Sc. [Pp. viii + 247, with frontispiece and 190 figures.] (London: G. Bell & Sons, Ltd., 1948. 7s. 6d. net.)

Introduction to Wireless presents a coherent account of the physical principles of wireless. The first half of the book is devoted to elementary physics, and deals with wave motion, magnetism and electricity, alternating currents and resonant circuits. The remaining chapters give an account of thermionic valves and their applications to transmitting and receiving equipment, followed by a brief description of the cathode ray tube, television and radar.

The chief merit of the book lies in its experimental approach. Many simple and illustrative experiments are described which might well be repeated by the reader. Unfortunately, details which would make it possible to construct the simple radio receivers described are not given. Elementary mathematics is used, but most of the work can be appreciated by the reader with no knowledge of that subject.

G. R. H.

Heat Pumps. By PHILIP SPORN, E. R. AMBROSE and THEODORE BAUMEISTER. [Pp. viii + 188, with 119 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 22s. 6d. net.)

ALTHOUGH the Heat Pump was invented by Lord Kelvin in 1852, its development in Great Britain has been relatively slow, owing no doubt to our temperate climate. The most notable example here is employed for heating the Electricity Offices at Norwich.

Clearly more progress will be made with the new heating system in countries where air-conditioning is desirable all the year round, as in cases of extreme climates. The chief development has taken place in the United States and the present text by three experts of the American Gas and Electric Service Corporation forms a welcome addition to the literature on the subject.

The first chapter discusses the thermodynamic principles and the properties of the various working substances, set out concisely and with ample diagrams. The chief basic heat-pump designs and the corresponding circuit diagrams are described in Chapter 2, while design factors, particularly the effect of climatic conditions, are the subject of the next chapter. The climatic data given are naturally drawn from the United States, but the principles are applicable to all countries.

Chapter 4 is devoted to a discussion of equipment, including fans, compressors and the heating and cooling of air, while detailed calculations for the air-conditioning of a house are given in Chapter 5.

When air is used as the source of heat there is a possibility of the formation of ice on the outdoor coil. The necessary defrosting system is explained in the next chapter. This is followed by a description of a number of applications of the heat-pump to industry.

The economics of the system are also discussed, while the book ends with several illustrations of actual plants.

The treatment is thorough and the book can be recommended as an excellent digest of the subject and well worthy of study.

B. LLOYD-EVANS.

Scientific Instruments. Edited by H. J. COOPER, B.Sc., A.R.C.S.I., A.M.I.E.E. [Pp. 293, with 269 figures.] (London: Hutchinson's Scientific and Technical Publications, 1946. 25s. net.)

THIS is a courageous attempt to fill a gap in scientific literature; to give the people who have to use instruments an idea of how they work, and—to a certain extent—of how they are constructed. The editor has decided that the scope of the book shall be delimited by five sections, i.e. optical instruments, measuring instruments, navigational and surveying instruments, liquid testing, miscellaneous. (The last section covers acoustics, calculating machines, hardness indicators and vacuum tubes.)

With the aid of 14 collaborators, most of them University graduates and all "specialists," 29 chapters within this framework are presented. As the editor says, the division of the chapters between the sections is somewhat arbitrary and there is some overlap, but one wishes that the editorial functions had been exercised more in determining what the collaborators should write on within their respective provinces, for the treatment is distressingly uneven. Some contributions are good and up-to-date; for instance, the electron microscope, the mass spectrograph and the quartz-controlled clock find place, and the section on acoustics is modern and well done. At the other extreme, the author of the chapter on lenses appears not to have heard of perspex nor the writer of the chapter on polarimeters of polaroid, while the fact that we wait to the final chapter for some instruction on how the thermionic valve works leads us to expect that electronic instruments will not be a feature of the work, and we shall be correct in our assumption. Indeed, even in that bold introduction of vacuum tubes in the finale the ubiquitous photoelectric cell finds no place, and we have not come across an apparatus in the book which contains one, though, in the absence of an index, we cannot be certain that such is indeed the case.

Nevertheless—we must reiterate—this is a courageous effort in a practically untrodden field, and we must thank Mr. Cooper and his associates for blazing the trail, feeling sure that, in a second edition, some of the inequalities will be levelled out. But do, please, let us have an index!

E. G. R.

CHEMISTRY

Fundamental Chemistry. By HORACE G. DEMING. Second edition. [Pp. xvi + 745, with 188 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. 24s. net.)

To anyone not familiar with the first edition of this book its title might seem at first sight to be a little pretentious. The title, however, is readily justified, for the book can claim to deal with the fundamentals of chemistry. Within

its covers are marshalled, in a manner which cannot fail to stimulate and excite the intelligent student, a great wealth of chemical fact and theory.

In the preface to the first edition published in 1940, the author remarks that the book "is the result of an experiment with a class of 240 students, who from the beginning were encouraged to ask questions." Only the author can know fully the extent to which the enquiring minds of these students (his "guinea-pigs") helped him "to discover and eliminate ingredients that might be unassimilable or definitely toxic," but, even if the author had not disclosed his method of approach to the subject matter, it would soon have become clear to the reader that the presentation is here less formal and more thought-provoking than in most books which cover the same ground. Because of this, it should be of value not only to those who decide to take up chemistry late in their school days, but also to students who have already taken the Higher School Certificate or Intermediate examination. Many topics which are not often found in a general chemistry text are introduced and discussed at an appropriate level. Among these are viscosity; surface tension, with some interesting practical illustrations; ionic and non-ionic crystals; crystalline liquids (liquid crystals); the hydrogen bond; and nuclear chemistry, including the reactions underlying the preparation of the atomic bomb.

The general appearance of the book is attractive, and most of the illustrations are good, though a few are perhaps too small to bring out fully the details (Figs. 61, 113 and 114). For a book of its size and range it has surprisingly few ambiguities or errors. Some uncertainty may be left perhaps in the reader's mind as to the nature of aluminium chloride as a result of somewhat contradictory statements in various parts of the text, and there is some danger that acetic acid may be thought to form only insoluble salts.

But these small defects in no way detract from the excellence of the book. Unfortunately, the adverse rate of exchange makes it a rather expensive elementary textbook, but all who can afford it will find it well worth the money.

B. J.

Cyanogen Compounds : Their Chemistry, Detection and Estimation. By HERBERT E. WILLIAMS. Second edition. (Pp. xvi + 443.] (London : Edward Arnold & Co., 1948. 40s. net.)

In 1915 the author produced what at that time was probably the first book, in the English language, devoted exclusively to the study of cyanogen compounds, and now, after a period of thirty-three years, he has completely rewritten the work. Such a long interval between the publication of the first and second edition of a scientific book is unusual, and the author must have had some particular urge to undertake the heavy task of preparing the present volume. Maybe it was due to the repeated solicitations of his colleagues and others engaged in this particular field of chemistry, to whom the first edition has been an invaluable guide to the subject. This new edition is therefore assured of a cordial welcome from those chemists, both academic and industrial, who require an authoritative and up-to-date account of cyanogen compounds.

Quite apart from the addition of a large amount of new matter, the present work differs from the first edition in several respects, notably in the exclusion of that part which was devoted to the manufacture and application of

cyanogen compounds; this may be something of a disappointment to those workers in industrial laboratories who are engaged in studying the production of these bodies, especially as no one is better qualified than H. E. Williams to deal with this aspect of the subject. Although this section has been eliminated, the special needs of the industrial chemist have not been overlooked, as the book is well sprinkled with technical matter which will be of much value to those who are concerned with the manufacture of the compounds. Another difference between this and the first edition is the extended use of constitutional formulae, a feature that cannot fail to be helpful and one that might have been further extended with considerable profit to the student. The analytical section, which comprises about one-quarter of the whole text, is particularly valuable and will be greatly appreciated, especially by those who are called upon to analyse troublesome mixtures of cyanogen compounds.

Within the limits of its title, this book is little less than an encyclopædia, a statement which is supported by the magnitude of the name and subject indexes; upwards of 2000 references to authors' publications are cited. Unfortunately, some of the references are incorrect and some other errors have crept in, but in view of the immense task carried out by the author these slips are excusable. Altogether, it is an excellent book, a book that will be a standard work for years to come, and both the author and the publishers are to be congratulated on its production.

W. H. BRINDLEY.

Physical Chemistry. By (the late) E. D. EASTMAN, Ph.D., and G. K. ROLLERSON, Ph.D. [Pp. viii + 504, with 89 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. \$4.50.)

THIS book is based on the course in Physical Chemistry recently introduced at the University of California. By British standards it corresponds to the general requirements for an honours degree in Chemistry (without specialisation in Physical Chemistry). The twenty-six chapters cover the field very well, with the exception that the subject of Colloids is not mentioned. Every other topic that could reasonably be expected seems to be introduced; and the treatment is sound and up-to-date.

To cover so much ground in a moderately sized book has demanded some compactness of style. There is, for example, hardly any reference to the experimental aspect. (Symptomatically the faraday is defined as the product of the electronic charge and the Avogadro number.) To some students the book may, therefore, convey that sense of abstractness that comes from the avoidance of practical illustration. Again, mathematical results are often given without derivation; and, where a derivation is provided, it is apt to be sketchy. (On this ground it is possible to question the way in which the Second Law of Thermodynamics has been related to the Carnot Cycle.) Or again, too great conciseness does not always make things easy. (For instance, the treatment of crystal symmetry in one page, and without diagrams, may not be very helpful to the student.)

But the general impression created by this book is good, and the mention of such criticisms in a review necessarily over-emphasises them. Indeed, the authors face the first two in their opening chapter. They say, "detailed description of instruments, apparatus and experimental techniques has been largely suppressed . . . partly because the genuine values to be obtained

from [them] . . . come best from experience rather than from description"; and "we shall be content . . . merely to outline final [mathematical] results if it appears that to do more would retard our principal interest, which is physical chemistry itself." It will be generally agreed that the result justifies these anticipations. Indeed, this is probably the best textbook of Physical Chemistry of its size available at present.

Each chapter ends with a set of a dozen or more questions, mostly numerical exercises (without answers). Certain paragraphs on advanced topics are asterisked, with the suggestion that they may be omitted at a first reading. The many tables embody a useful amount of our current experimental data. There appear to be few typographical errors. The price in Britain is 27s. at the moment.

J. O. SPEAKMAN.

The Kinetics of Reactions in Solution. By E. A. MOELWYN-HUGHES, D.Sc., D.Phil., Ph.D. Second edition. [Pp. viii + 424, with 52 figures.] (Oxford: at the Clarendon Press, 1947. 25s. net.)

THE questions of how and why natural phenomena occur have been predominant philosophical problems ever since man has been capable of logical thought, but only during the past half-century has there been any valuable advance in such reasoning when applied to chemical problems, even those which appear to be of a simple type. Reactions in solution offer even more possibilities of complexity than do those in the gaseous phase, but owing to the relative simplicity of the manipulation involved they were amongst the earliest reactions in which the change was studied as a function of time, temperature, and concentration. Later, when the theory with regard to gaseous reactions had been further developed, the results obtained in solution appeared to follow approximately, but only approximately, similar laws. The adaptation of the general theory to include the additional complications which can arise when reactions occur in solution has been developed only during the past two decades, and the author of this volume has contributed in no small measure to this advance.

As a result of its first edition, Moelwyn-Hughes' book is already established as the standard work on its subject, and its status will be further enhanced as a result of this second edition, in which the text has been completely rewritten in the light of the experimental and theoretical advances which have been made during the past fifteen years. The result is an excellent compilation of our present knowledge of the subject, presented in an attractive and critical manner. As is inevitable with a specialist work the criticism may be levelled that some aspects are over-emphasised, whilst others are given mention which is inadequate in relation to their importance. Organic chemists might have desired a fuller exposition of the kinetics of hydrolysis reactions and of molecular rearrangements, whilst some physical chemists would have preferred a fuller discussion of the primary and secondary salt effects. This, however, is not to the detriment of the book, and it is rather to its credit that it elaborates especially those aspects in which the author is most interested.

The quality of the paper, the clarity of the printing, and the binding suggest a welcome relief from the drab-looking productions which have all too frequently been necessitated by recent conditions.

J. W. S.

Tables of Properties of Gases. By E. W. GEYER, B.Sc., Ph.D., and E. A. BRUGES, B.Sc. [Pp. 102, with 19 figures.] (London: Longmans, Green & Co., 1948. 12s. 6d. net.)

It is well known that the products of combustion, found in the exhaust pipe of an internal combustion engine, or in the flues of a steam boiler, consist of carbon dioxide, water vapour, oxygen and nitrogen, although a change in the physical condition of the mixture, such as an increase in temperature, or a decrease in pressure, tends to cause dissociation of the carbon dioxide into carbon monoxide and oxygen. Part of the water vapour is dissociated into hydrogen and oxygen. These effects of dissociation are very important in technical combustion processes, and the thermodynamic properties of gases are of supreme importance in the design of all classes of internal combustion engines. The publication of the present set of tables, which are based on the results of modern statistical thermodynamic analyses, is therefore welcomed.

The first part of the book is devoted to an explanation of the principles upon which the tables are based, and, in order that the fundamental ideas of dissociation and of equilibrium constant may be understood, a full description of the van't Hoff equilibrium box is given. The tables, which follow, include such properties as entropy, internal energy and total heat, as well as the equilibrium constants and heats of reaction for certain of the more commonly used hydrocarbons. The values are listed in tabular form at intervals of 100° F., and, to facilitate interpolation, differences are also tabulated. For comparative purposes, modern values of specific heats, along with those previously suggested for the technically important gases, are tabulated. The differences between the modern quantum values and the older ones are considerable. In order to assist the reader, some worked examples, together with problems in dissociation, are included, and the application of the tables is helped by means of diagrams.

This book, which contains a useful bibliography, can be recommended to engineers who require concise and up-to-date information on the thermodynamic properties of gases.

F. H. N.

Reactions of Organic Compounds. By W. J. HICKINBOTTOM, D.Sc., Ph.D., F.R.I.C. Second edition. [Pp. x + 481, with 5 figures.] (London, New York, Toronto: Longmans, Green & Co., 1948. 25s. net.)

Most research workers, whose field of study comprises Organic Chemistry, will already be familiar with the first edition of Dr. Hickinbottom's book. For them it will be a sufficient recommendation of the new edition to say that it amply maintains the high tradition of accuracy and general usefulness established by its predecessor. For potential readers who have not hitherto "consulted Hickinbottom" when seeking directions for practical organic manipulations, it may be said that the book presents the facts of Organic Chemistry in terms of the reactions—with extensive illustrations—of functional groups. This method of approach has considerable advantages both for the research worker, who desires a reliable source of references to the literature, and for the undergraduate student who needs to learn general reactions rather than to remember too many specific examples. Nevertheless, the text is fully supplied with cases of exceptional behaviour where the importance

of the anomalies justifies presentation. The book is provided with an extensive subject index so that it can also be consulted from the point of view of the reagent. This is a feature of some importance as all chemists "skilled in the art" will realise.

Although it would not be easy to improve upon Dr. Hickinbottom's book, there are, perhaps, a few general points where the reader might hope for some alteration in future editions. Firstly, the text concentrates too much upon the reactions of aromatic compounds and too little upon the behaviour of natural products. For example, there are very few illustrations from steroid or polyterpenoid chemistry, although so much research has been devoted to these compounds in recent years. Secondly, there is scarcely a mention of the mechanisms of the reactions discussed, although such a treatment would make the text more readable and of greater interest. As it is, it will be an exceptional student who will read the book from cover to cover as a textbook. In the main, it will continue to be used as a valuable work of reference.

D. H. R. BARTON.

A Text-Book of Practical Organic Chemistry. By ARTHUR I. VOGEL, D.Sc., D.I.C., F.R.I.C. [Pp. xxiv + 1012, with numerous figures and tables.] (London: Longmans, Green & Co., 1947. 42s. net.)

SEVERAL years have elapsed since the appearance of a new text-book of practical organic chemistry in Britain, and the publication of the present comprehensive volume has aroused considerable interest. It is claimed that the book should meet the requirements of a student of organic chemistry throughout the whole of his training, and it can be safely said that this ambitious claim is fulfilled to a very large degree; certainly the book can be said to begin at the beginning, for conical flasks, beakers, funnels and condensers are all illustrated in the section on apparatus. The opening chapter deals with the theory underlying the most important operations of organic chemistry and discusses such topics as vapour pressure, variation of boiling-point and pressure, fractional and steam distillations, etc. The second chapter is devoted to a discussion of experimental technique and contains descriptions of apparatus, including that with interchangeable ground-glass joints. This account, comprising 155 pages, also contains a great deal of information on technique, including such useful details as the drying of the various organic solvents and the preparation of inorganic reagents. Numerous references are given to firms supplying specialised apparatus, but it is likely that the value of this information will be limited, owing to the rapidly changing supply position and the frequent improvements in apparatus design. A section on glassblowing might well have been included in this chapter, as well as more details on several of the more refined methods of chromatography which play such an important rôle in organic chemistry today. No mention is made of the technique of reactions in liquid ammonia solution, or the convenient method of preparation of hydrogen bromide from bromine and naphthalene rather than the more arduous methods from bromine and hydrogen or bromine and tetralin. More guidance might have been given on the many commercial preparations of active carbon and the frequent alkalinity of commercial sodium sulphate is not stressed. These are small points, however, and there is no doubt as to the usefulness of this chapter, which well repays careful study.

The following eight chapters deal with preparative organic chemistry and comprise the main portion of the book. Several hundred preparations are given and the various chapters cover aliphatic, aromatic, heterocyclic and alicyclic compounds, miscellaneous reactions, dyestuffs, indicators and related compounds, physiologically active compounds and synthetic polymers respectively. Many of the preparations duplicate the methods already available in *Organic Syntheses*, although no indications are given by the author as to the existence and merits of alternative methods, and no literature references are included. Likewise no information is given as to the commercial availability of the compounds described. Details of the preparations are very full and the student should have no difficulty in following them.

The final chapter is devoted to qualitative organic analysis—no quantitative methods are given in the book—and it is a rather unfortunate consequence of the arrangement of the subject matter that the tables of derivatives of the various classes of compounds are scattered throughout the whole volume. Although the page numbers of these tables are given on the back cover, it would have been more convenient to have grouped them together in this chapter. Guides to the use of the literature are included in the Appendix, where a useful account is given of the arrangement of Beilstein and the various Abstracts as well as the recent Dyson system of nomenclature. Also included in the Appendix are sections on First Aid, the physical constants of certain common solutions and the preparation of hydrogenation catalysts.

Apart from grouping all classes of compounds, e.g. alcohols, amines, esters, ethers, etc., under "Aliphatic" and "Aromatic," which is convenient enough when once realised, the index appears to be fairly adequate. A "type of reaction" index would have been useful. The book is well printed and produced, although the binding could have been made more robust in view of the probable frequent excursion of the volume into laboratories.

The thorough treatment of the subject and inclusion of detailed descriptions of so many modern methods and refinements ensure that this is a volume which should be available in every laboratory and which should recommend itself to all organic chemists.

A. W. JOHNSON.

The Water-Soluble Gums. By C. L. MANTELL, Ph.D. [Pp. xvi + 279, with 48 figures.] (New York: Reinhold Publishing Corporation, London: Chapman & Hall, Ltd., 1947. 36s. net.)

Gums have been used by man from the earliest times in many different parts of the world. There is evidence to show that the North African gum arabic trade was in existence even before the Christian era. With the development of industry the uses of vegetable gums increased and they now find uses in many widely different manufacturing processes. Large quantities of gums now find their way to the world's markets.

It is fitting, therefore, that a book devoted entirely to gums should now appear. The writer deals with the subject mainly from the chemical angle and appears to have made a close survey of the advances that have been made in the chemistry of gums in recent years. There is also a great deal of useful information of a general nature throughout the book. Separate chapters are devoted to the more important gums such as gum arabic, Indian ghatti gum, gum tragacanth and tragacanth substitute gums. The present-day uses of gums are dealt with comprehensively under sections

entitled "Gums in Cosmetics and Perfumery," "Gums in Paints and Coating Compositions," "Gums in Textile Operations" and "Miscellaneous Applications." There is also a chapter on "Specifications, Identification and Testing."

In addition to commercially important gums, the better-known vegetable mucilages are also dealt with, such as those from seaweeds (agar, Irish moss or carrageen), Iceland moss (a lichen) and various seeds (e.g. psyllium, quince, flax). Some may feel that a more suitable title for the book might have been "The Water-Soluble Gums and Vegetable Mucilages."

It is unfortunate that some errors in the spelling of botanical names have escaped the author's notice and that a few obsolete names have been used (e.g. *Acacia verec* for *Acacia senegal* and *Acacia horrida* for *A. karroo*). The statement (page 21) that some botanists consider *Acacia verec* (*A. senegal*) a variety of *Acacia arabica* will also be challenged, for the two species are quite distinct—with pods and inflorescences totally dissimilar—and have never been confused by botanists. However, these small points should not seriously detract from the value of the book to those interested in one way or another in this little-known but important group of economic plant products.

F. N. H.

Synthetic Resin Chemistry for Students. By S. R. W. MARTIN, Ph.D., F.R.I.C., A.R.C.S., D.I.C. [Pp. viii + 160, with 18 figures.] (London: Chapman & Hall, Ltd., 1947. 15s. net.)

DR. MARTIN has written an original type of book for the benefit, as he explains, of those entering the resin industry from the universities. He has been successful, in that his volume contains much that will help such students. It should be pointed out, however, in case of misunderstanding, that the title correctly describes the contents; this is not a book on plastics, but rather on resins for paint and coatings.

The style is pleasing and the appearance and layout are attractive. Modern chemical ideas, e.g. on phenolics, are not always dealt with quite adequately, but they generally provide sufficient background for future study. It is perhaps an omission that references to original work are not included, particularly because a note on the dust cover states: "It is an excellent book for the qualified chemist to hand to his young assistant and will save him many explanations." The materials covered include rosin and derivatives, straight and modified phenolic resins, amino-resins, alkyds, maleic anhydride resins, polyamides and ethenoid resins; space is also given to the general principles of resin formation, to efficient note writing (an excellent feature) and to analytical methods. The inclusion of numerous practical exercises is noteworthy and useful.

A few minor misprints, e.g. on pp. 18, 65, 100, 126, 138 ("acronitrite"), 154 and 160 were noted. There are also certain minor errors, e.g. the prefixes mentioned on p. 2 are surely Greek not Latin, ethyl alcohol (comparison with allyl alcohol) should be propyl alcohol (p. 13), formalin generally contains much more than a trace of methanol (p. 45), the cross-reference to p. 157 should be to p. 151 (p. 102), the preparation of polyisobutylene (p. 137) bears no relation in fact to that of polyethylene, being carried out at very low temperature in solution, while on p. 141 the functional group for dimethylene urea needs correcting. These are, however, minor blemishes

in a useful book, and can be easily rectified in a subsequent edition. This volume is of value to others than students, and will give food for thought to plastic technologists as well as those concerned with paint problems.

N. J. L. MEGSON.

Boiler House and Power Station Chemistry. By W. FRANCIS, M.Sc.Tech., Ph.D., F.R.I.C., F.Inst.F., F.C.S. Second edition. [Pp. xiv + 274, with 83 figures, including 8 plates.] (London: Edward Arnold & Co., 1947. 21s. net.)

THE duties of the fuel technologist in the boiler house and power station are extensive and important; he is concerned, not only with the selection of the most appropriate classes of coal, its analysis and efficient combustion, but also with questions of flue-gas treatment, water conditioning, prevention of scale formation, the testing of turbine, switch and transformer oils, and with a variety of other problems, upon which depend the smooth and efficient running of his plant.

To provide comprehensive instruction and guidance over such a wide field would be beyond the scope of any one book, but, by a judicious selection of subject matter, Dr. Francis has succeeded in bringing together a considerable body of information upon the theory and practice of steam raising and power production and presenting it in an eminently readable form.

The book, which comprises sixteen chapters, is divided into two parts, the first dealing briefly with the constitution and classification of coal and with the technology of water and oil, and the second with details of all the analyses and other tests undertaken in normal boiler-house and power-station routine.

The reader is given a brief outline of modern views on the theory of formation and constitution of coal, and there is an excellent chapter on the combustion of coal in boilers, in which the heat balance is discussed in some detail and practical examples are worked out. Other sections deal with flue-gas treatment, feed-water conditioning, standard methods for the sampling and analysis of coal, ash, flue-gas, water, and turbine and insulating oils.

The book is well illustrated by photographs and line drawings and tables of useful data are given in the text and in appendices.

D. M. N.

Chemical Engineering Fundamentals. By C. G. KIRKBRIDE. [Pp. xii + 419, with 78 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 30s. net.)

THE existence of the wide gulf that is fixed between American and British university teaching is clearly shown by a brief study of Prof. Kirkbride's book. We have had from the same publisher and on the same theme, elements, principles, and now fundamentals. This repetition seems to be inevitable in an academic system in which such great stress is laid on publication as a means of progress. The author has indicated in his preface that chemical engineering teachers needed a text which would enable them to give instruction to students in applied chemistry, physics, and mathematics, in order to introduce what he terms somewhat luxuriantly the "primary fundamentals" of this branch of engineering; this want he has set out to supply.

It is open to debate whether the author of a work on chemical engineering should attempt to condense into a single chapter of some fifty pages the principles of both physical and organic chemistry, and it seems unnecessary to devote a further fifty to the reproduction of numerical tables, which data the student could with advantage discover for himself in their original form. This collection of facts seems only to be pandering to the laziness and indolence of the world at large, of which the author complains in his chapter on human relations. He seeks, however, to correct this by submitting his students, after their perusal of Chapters 1, 2, and 10, to a catechism, from which examples may be selected at random and the publication of which seems to indicate that a paper shortage is not yet universal. Queries such as "What are the nine essential qualities of leadership?" or "What are 5 out of the 10 'do's' and 'don't's' in dealing with one's fellow-men?" lead almost naturally to the pertinent question "Why does an employer hire chemical engineers?"

The most useful sections of this work are devoted to demonstrations of the industrial applications of the laws of conservation of mass and energy. Numerous numerical examples are worked out in full, but the underlying principles are given but scant attention. This is, of course, the avowed intention of the author, who claims that by so doing the student subsequently appreciates the more formal approach to the underlying theory. With this idea many teachers will differ, but it is undoubtedly a reaction to the isolation in watertight compartments of various branches of engineering and their infliction on the student by specialists unsympathetic to the aims of others.

Chapters devoted to a consideration of economic principles and to the presentation of technical reports may be of value to the chemical engineering student. Prof. Kirkbride's book has, however, been written with such a special object in view that it is doubtful if his method of approach to the teaching of chemical engineering, which necessitates taking a double bite at the same cherry, will wholly commend itself to the technical teaching institutions in this country.

W. P.

Encyclopedia of Chemical Technology. Vol. I: A to Anthrimides.

Edited by R. E. KIRK and D. F. OTTMER. [Pp. xxiv + 982, illustrated.] (New York: The Interscience Encyclopedia Inc., 1947. \$20.00.)

THE editors say that they have long been familiar with the great need for an encyclopædia of chemical technology, written in English and representing modern American practice and chemical engineering. This need is also impressed on an examiner's mind when trying to mark such a question as "How would you make acetic anhydride?" Some students start with a most improbable substance, go through the most expensive compounds in the textbook, involve themselves in some five times as many reactions as they need and leave a large quantity of reacting chemicals to be disposed of on a long-suffering countryside. The difficulty which besets the examiner, with his pencil poised for marking, is that theoretically the student may be perfectly correct. In many cases the steps are perfectly logical and the only thing that is wrong is that the student knows the theory but not the practice of his craft. Why is it that A is the best starting-point for making B? Why is it in making B from A oxidation by nitric and chromic acid is suitable but air oxidation gives no yield? The full answer is often beyond our

present knowledge, but it should be within the scope of an encyclopædia to indicate why the process selected is a good one and to indicate the reasons for failure of other likely processes. One difficulty is that, if a likely reaction is unsuccessful, the reasons for its lack of success are often buried with the trial. Few papers are written about failures because the experimenters are too dispirited to continue the matter and record their experience for others. Sometimes the reasons are not technical, but due to lack of finance, etc. On these counts the ideal technical encyclopædia is perhaps more than we have a right to expect at the present development of science.

There are 93 contributors to this volume and about the same number of essays on selected subjects, of which the following might be mentioned: Alkali and chlorine industries, Aluminium, Ammonia, Alkaloids, Alcohol, Anthraquinone dyes, Amination by reduction, Amino resins and plastics, Alkyd resins, Adsorption, Acetic acid, Acetylene, and Absorption. Topics which are technologically related to one another are grouped together. There are articles on "uses," such as Abrasives and Adhesives. DDT comes under Insecticides, procaine under Anesthetics.

The size of the page is about 7 inches $\times$ 10 inches and to our paper-starved eyes the margins are more than generous. The printing and layout are well arranged and easy to read, and the line diagrams and graphs clear and informative. Each main article is accompanied with an average of some 20 to 40 references at the end of the text. The information given in this volume is ample and authoritative. If the rest of the volumes maintain this standard it will be a work which no library can afford to be without.

M. B. DONALD.

Safety Rules for Use in Chemical Works. Part I: Model Rules.

Third edition. [Pp. 69.] (London: The Association of British Chemical Manufacturers, 1947.)

THE Works Safety Committee appointed by the Council of the A.B.C.M. and the Association of Chemical and Allied Employers is to be congratulated on the publication of this little volume in a revised and up-to-date third edition setting out in simple terms the principles on which the safe working of chemical plant can be assured. Their advice, tabulated in nine main sections covering the major types of risk experienced in chemical and allied works, is given in the form of simple rules, not only for the operation, but also for the design and maintenance of equipment used in the chemical industry. In addition to the risk element of operation, information is also given concerning necessary health and welfare services, and the rules governing the employment of women and young persons. The rules themselves are numbered and given marginal titles and, where necessary, reference is made to appropriate legislation.

The great influx of inexperienced supervisory staff to the chemical industry during the war demonstrated the need for guidance on matters of safety in an easily accessible form such as is provided in this book. The distribution of copies, even of the earlier edition, to all those responsible for the safety of their fellows would have been of even greater value than the appointment of so-called safety officers. It is an elementary military maxim that every unit is responsible for its own safety; so must every industrial chemist and chemical engineer make himself responsible for the safety and well-being of his subordinates, and a study of the principles enumerated in this little volume will greatly simplify his task.

Although it would clearly be impossible, without unduly increasing the size of this work, to publish as appendices all those sections of the various Acts enforcing safety principles, it would have been useful to include the Chemical Works Regulations, for there exists today many a chemist born to blush, but not unseen, for failing to give that account of his duty towards his neighbour which is enforced by these instructions.

W. P.

GEOLOGY

The Formation of the Continents by Convection. By G. F. S. HILLS, B.A. [Pp. vi + 102, with 5 figures.] (London: Edward Arnold & Co., 1947. 7s. 6d. net.)

In this book the author has tackled one of the most fundamental and least understood problems of Geology. Events in the early history of the cooling earth have led to the formation of the continents, rafts of light material "floating" on a denser substratum. Why discrete masses should have formed rather than a continuous shell (which would inevitably have been buried deep below a world wide ocean) is an unexplained problem which has caused much speculation. The author's theory, that convection currents carried early formed crystals of the lighter minerals like floating scum towards the poles, is attractive and is in this volume buttressed by many facts drawn from recent seismological and isostatic work. The effect of the earth's radioactivity is also considered. The book is not free, however, from the fault of trying to explain too much, and certain parts of the argument, as for instance the discussion of the depth of the Cretaceous sea on p. 55, are open to serious criticism.

A. W.

AGRICULTURE, HORTICULTURE, AND FORESTRY

Nature and Prevention of Plant Diseases. By K. STARR CHESTER, Ph.D. Second edition. [Pp. xii + 525, with frontispiece and 224 figures.] (London: H. K. Lewis & Co., Ltd., 1947. 25s. net.)

As stated in the Preface, the object of this medium-sized textbook is to meet the needs of the general agricultural student who is not primarily interested in plant pathology as a field of research and whose undergraduate studies in this subject are of comparatively short duration. Nevertheless, the author aims at covering the whole range, both in respect of detailed treatment in some important disease problems and of general principles of study.

The greater part of the work—approximately four-fifths—is devoted to a survey of the more important plant diseases, and, as one would expect, emphasis is laid upon those which figure most prominently in the agriculture of the North American continent. The grouping is mainly based upon the systematic position of the causal agent, so that successive chapters deal with Rusts, Smuts, Higher Basidiomycetes, Ascomycetes, Imperfect Fungi, etc., with in some cases a further subdivision according to the type of plants (cereals, vegetables, etc.) affected. Then follow chapters on diseases caused by Bacteria, Viruses, Parasitic Seed Plants and Algae, and on non-parasitic or physiological diseases. A useful innovation is the inclusion of a short account of some of the more important plant diseases caused by parasitic nematodes. The final six chapters are of a more general nature and relate

to methods of study, effect of environment on disease, and the broad lines of practical control, whether by legislative action, by the breeding of resistant types of plants, by cultural methods or by the use of plant protective agents. References to selected books and articles (in the English language only) are appended to most of the chapters, and there is finally a short glossary of technical terms and a 23-page subject index.

This book, though it follows more or less conventional lines and though it offers no new viewpoint to the student of plant diseases, can well be recommended to a wide circle of readers who are interested in the problems of crop production. The presentation is everywhere clear, if now and then perhaps a trifle over-dramatic, and it definitely excels some of its competitors in its realistic outlook, especially where control measures of practical worth are concerned. As the majority of the diseases illustrated are not confined to the North American area, but many of them or similar ones are equally important elsewhere, agricultural students all over the world will find this book an excellent guide to the problems which are set by plant disease and to the methods by which they can be combated.

W. BROWN.

Chemical Insect Attractants and Repellents. By VINCENT G. DETHIER, A.M., Ph.D. [Pp. xvi + 289, with 69 figures.] (U.S.A.: The Blakiston Co.; London: H. K. Lewis & Co., Ltd., 1947. 25s. net.)

THE sense of smell is so important to insects and is so highly developed in some orders—witness the “assembling” of male Gipsy moths—that it has long been a popular study of the field naturalist. The more fundamental studies have been in ill-defined territories where entomology meets chemistry, physics, botany and physiology; Dr. Dethier is the first to evaluate this scattered work and to fuse it into a book of great usefulness to the ecologist and the applied entomologist. He has aimed at the assembly and review of every pertinent scientific contribution to his subject and has included data culled from war-time research and previously available only in official reports. He has handled his material from the chemical standpoint and, after introductory chapters, deals with attraction by essential oils, by fermentation and decomposition products. Then come helpful chapters on the quantitative aspects of the olfactometer and on practical applications in baits and traps. The next chapter, on repellents, is not up to standard—a curious lapse, for the economic importance of repellents is obvious to the sufferer from malaria and to the angler. The final chapters, on the chemical basis of taste and olfaction and on the evolution of feeding preferences, are provocative and stimulating discussions of these hypothesis-ridden subjects. It may be noted that, although the author decides in favour of the Hopkins Host Principle—which, in general terms, states that the insect prefers to oviposit on the food upon which it fed as a larva—he does not appear to realise that this hypothesis is tacitly assumed in Verschaffelt's demonstration of the preference of cabbage caterpillars for plants containing mustard oils or in his own more comprehensive investigation of the role of terpenes in the nutrition of larvae of the Swallowtail butterflies. In neither case had the larvae any choice in the plant on which they hatched; that choice rested with the ovipositing butterfly.

The book will become well-thumbed by every applied entomologist and

no one will be seriously misled by several errors in chemical nomenclature; but the author would do well to obtain a chemist's revision of the text for the second edition.

HUBERT MARTIN.

Hormones and Horticulture. By G. S. AVERY and E. B. JOHNSON. [Pp. xii + 326, with 51 figures.] (New York and London: McGraw-Hill Co., Inc., 1947. 27s. net.)

THIS account of the use of special chemicals in the control of plant growth is a useful summary of modern work in this field, fully documented by means of extensive bibliographical references in relation to each chapter.

"Hormones and the rooting of cuttings," which is the subject of the second chapter, contains a table of species which have been experimented upon, cites the concentration employed and the percentage rooting of treated and untreated material. Over 600 different kinds are recorded in which some measure of success was achieved, whilst with over 100 no response was obtained.

The use of hormones to control pre-harvest fruit drop and as an aid to the setting of fruits are the subjects of further chapters. For apples this method of diminishing fruit drop is in commercial practice, but for other fruits it is still regarded as in the experimental stage; as a method of ensuring fruit setting of seedless fruits, the tomato is the only subject that can as yet be successfully treated on a commercial scale.

The treatment of seeds with hormones holds out little promise of success, but their employment as selective weed killers, although still in its infancy, has already achieved great success, particularly since, as Tukey and others in America have shown, the underground parts of such pestilential perennials as Bindweed and Perennial Sow Thistle can be killed by the use of weed killers of the phenoxyacetic acid type when the foliage is treated with a dilution of one part per thousand. Even Water Hyacinth can be controlled by a mixture of 2-4 dichlorophenoxyacetic acid and indolebutyric acid.

Other topics dealt with are the chemical thinning of fruit blossom, the production of polyploids by means of Colchicine, which we owe to Dr. Blakeslee, and the chemical methods of treating dormancy or of prolonging or inducing it. For example, *Syringa vulgaris* can, by treatment with ethylene chlorohydrin, be induced to flower four to five weeks earlier. On the other hand, by means of methyl ester of naphthalene acetic acid, the sprouting of potato tubers can be inhibited in storage.

The subject matter is full of interest, and the book serves as a summary of present knowledge that should be helpful alike to the scientist and the practical horticulturist.

E. J. S.

Forest Valuation: With Special Emphasis on Basic Economic Principles. By HERMAN H. CHAPMAN, M.F., So.D., and WALTER K. MEYER, M.F., Ph.D. American Forestry Series. [Pp. xii + 521, with 21 figures.] (New York and London: McGraw-Hill Book Co., Inc., 1947. 36s. net.)

THE earlier textbooks of Prof. Chapman of Yale on forest valuation and finance are already familiar to scientific foresters. His new book represents a broad approach to a highly specialised subject. His experience in teach-

ing forest valuation proved the need for a background of economic theory. Accordingly the early chapters are concerned mainly with basic principles controlling forest enterprises. The authors hope that their approach represents a definite breakaway from the original European sources of evaluation and statics. Thus the economic discussions are largely pertinent to North America and to a "competitive economy founded on private initiative and free markets, though the importance and economic role of public ownership and operation is given proper consideration and bulks large in forestry in all countries regardless of their forms of government." The economic discussions cover many topics: present worth of forests producing annual and periodic income; the role of compound interest; profit margins; rotations; taxation; damages; fire insurance; assessment of stumpage values; assessment of forestry purchases for future operations. Of particular interest is the authors' emphasis that, while compound interest is necessarily the basis for computing total cost of investment until annual income is normally earned, it then ceases to apply as a valuation basis. The valuation of the forest as a whole would then be based on the annual instead of a deferred income. But, even for such going concerns, the value or present worth of the business can be assessed only by discounting the expected future net annual income. The book thus performs a useful service in making clear that past costs and indeed market prices are surprisingly limited as criteria of present worth. Convincing arguments are produced why experts in forest finance should cease apologising for "complicated" formulæ and techniques, which are common to the entire field of business finance and valuation, and instead should encourage foresters to acquire and use the necessary tools of their profession.

J. J. MACGREGOR.

BIOLOGY AND BIOCHEMISTRY

Harmony of Nature: A Study in Co-operation for Existence.

By L. RICHMOND WHEELER, Ph.D., M.Sc., F.L.S. [Pp. viii + 200.]
(London: Edward Arnold & Co., 1948. 10s. 6d. net.)

DR. WHEELER is a naturalist of wide experience and independent mind who is keenly interested in the general problems of life and evolution and their bearing on politics and religion. He has compressed into this little volume the fruits of a lifetime of observation and reading, and his views are well worthy of attention. He protests against the over-emphasis of struggle and competition in Nature, which was a feature of nineteenth-century thought, with disastrous results in human affairs. He points out, with a wealth of illustration, not only that mutual aid and co-operation play a part in organic life, but, more especially, that all living things are linked together in mutual interdependence ecologically; there is, therefore, in this broad sense a harmony in Nature, as a basic condition of life at all.

Dr. Wheeler gives a good deal of attention to the theory of evolution, criticising the Darwinian and Neo-Darwinian points of view, and favouring the views of Berg and Willis. He is vigorously anti-materialistic in his philosophy.

The book is almost too full of meat; the range of subjects covered is remarkable, and the extent and variety of Dr. Wheeler's enquiries is evidenced by the extensive and up-to-date bibliography which forms a useful appendix to his extremely interesting volume.

E. S. RUSSELL.

The Bee Community : A Study of the Social Life and Structure of the Honey Bee. By F. H. METCALF, B.Sc. [Pp. 102, with 6 plates and 39 figures.] (A Bee Craft Publication, 1948. 7s. 6d. net.)

THIS book deals with the structure, development and social life of the honey bee in a way that fills more than one gap in current works on bees and bee-keeping—in a way also to appeal to the general reader interested in natural history and desiring to acquire a sound scientific foundation for his knowledge.

The bee is treated first as an insect, then follows an exceptionally clear account of growth by cell division, expressed in scientific terms, but readily understood with the help of the glossary provided. The structure and functions of queens, drones and workers are detailed in an interesting manner, followed by up-to-date accounts of what is known about what bees do and how they do it, fully supporting the thesis that it is difficult to deny them the possession of some sort of primitive intelligence.

Some near relatives of the honey bee are also described and illustrated, and nine pages are devoted to reminding us how much there is we do not yet know.

There is a chapter devoted to that most important service rendered by the bee in the pollination of flowers, a service worth to the nation many times the market value of the whole honey crop.

The reviewer has detected only two errors of fact, both of minor importance and perhaps due to overcondensation, for there is a remarkable amount of information crowded into the 102 pages provided. The reader is left to believe that all unimpregnated eggs must produce drones. Normally they do, but worker bees are known to appear amongst the drone progeny of unmated virgins under strict control, possibly by means indicated by the author in the section on cell division. Again, motorists generally allow that the spattering of a bee on the windscreen indicates a car speed of not less than about 40 m.p.h., but this is not a speed "very much greater than any insect is capable of."

The coloured illustrations will cause much pleasure, indeed the whole book is written and prepared in an attractive style.

E. B. W.

Nucleic Acids and Nucleoproteins. Cold Spring Harbor Symposia on Quantitative Biology, Vol. XII. [Pp. xii + 279, with 258 figures, including 20 plates.] (Cold Spring Harbor, L.I., N.Y., The Biological Laboratory, 1947. \$7.00.)

THE elucidation by Caspersson in 1934 of the give-and-take of nucleic acids whereby the chromosomes undergo their cycle of production and duplication, the so-called mitotic cycle, opened up the central problem of biology—the chemistry of protein propagation. A great concentration of theoretical and practical methods of attack on this problem has followed in which European and American work has been of equal importance. It has been directed to ascertaining the molecular structure of the nucleic acids themselves; to their estimation by spectroscopic, enzymatic and histochemical tests in a great variety of organisms and tissues, healthy and pathological, somatic and reproductive; and to the more purely biological studies of mutations and movements in chromosomes, bacteria, yeasts and viruses under the actions of chemical treatment and irradiation.

In order to summarise and synthesise this work, two symposia have been

held, one at Cambridge in 1946 (published by the Cambridge University Press) and one at Cold Spring Harbor in 1947 and now published as Volume 12 in the Series of Symposia on Quantitative Biology. Covering this enormous field of activity it was inevitable that the results of both should suggest a dysposium rather than a symposium. A welter of technical detail—in the American volume insufficiently digested—is not brought into any focus. And the brevity of Cold Spring Harbor does not entirely make up for its absence of coherence and structure. A conclusion is sometimes reached, but too often it is that “the existence of many correlations . . . may be of little pertinence for the evaluation of the validity of this assumption” (p. 221). A more discriminating list of invitations would have permitted more pertinence, more evaluation, and more validity. The advances made between 1946 and 1947 are also inevitably too slight to grip the imagination and hold the attention of the reader for a quarter of a million words. It is not enough to bring biology and chemistry together into one volume; it seems that they have to be brought together into one person, and this is what, apart from two or three of the European representatives, seems to have been lacking at Cold Spring Harbor. C. D. DARLINGTON.

Biochimica et Biophysica Acta. Vol. I, Nos. 1 to 6; Vol. II, No. 1. (Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd. 52s. 6d. per annum.)

THE publication of a new specialist scientific journal, especially if it is multi-lingual, raises immediately a number of questions, most of which are concerned with matters of high policy in the sphere of technical publications. There is no space to discuss them here, including as they do consideration of what are the legitimate parts to be played in their sphere by the scientific society, the University Press and the commercial publisher. *Biochimica et Biophysica Acta* comes from one of the last mentioned, and one that, judging by its activities since the liberation of the Netherlands, has high ambitions and high standards. No doubt this new journal is intended to replace, wholly or in part, Hoppe-Seyler's *Zeitschrift für Physiologische Chemie* and the *Biochemische Zeitschrift*, and this leads one at once to ask whether *The Biochemical Journal* and *The Journal of Biological Chemistry* do indeed require any such reinforcement.

This is the kind of question that to my mind can only be answered pragmatically. To attempt to answer it by inspecting the first number of the new journal would be to guess, with a greater or a less chance of guessing rightly. It is for that reason that I have refrained from reviewing the *Acta* until it had had a run of a year or so. I now have before me not only Vol. I, No. 1, but all six numbers of that volume and the first number of Vol. II. What have been their contents? What standard have these contents reached? Do they provide *prima facie* evidence that, had the *Acta* not been in existence, papers of real value in the domain of biochemistry and biophysics would not yet have been published?

There is no doubt that the already existing journals during the last twelve months have had no free space. The continued stringency of paper in the United Kingdom and the ever-growing output of original papers in the United States and Canada have fully occupied all available space in *The Biochemical Journal* and *The Journal of Biological Chemistry*. The time-lag between receipt of manuscript and ultimate publication has not

lessened perceptibly. The biochemical bus has undoubtedly been full, inside and out. The quality of the papers appearing during the period in question has, moreover, been no whit below that of pre-war days. We can be assured, then, that the papers published in *Biochimica et Biophysica Acta* during 1947 and 1948 to date could hardly have appeared elsewhere.

That brings us to the second question, namely, whether they have in fact made contributions of value to the science in question. When we find on the Editorial and Advisory Boards the names of British and American scientists of the standing of Astbury, Cori, Bernal, Harington and Krebs—to say nothing of Fromageot (France), Linderström-Lang (Denmark), Kluyver (Holland) and Tiselius (Sweden), to mention only a few, and among the contributors Brachet, Lederer, Engel, Massart, Westenbrink, Braunstein, Roche, K. Bailey, Hilditch, as well as several members of the two Boards—then surely we can assert with complete confidence that but for the publication of the *Acta* advances in Biochemistry and Biophysics would undoubtedly have suffered a conspicuous delay.

Our questions being then answered with an unconditional affirmative, it remains only to comment on one or two minor matters. Contributions, published in English, French and German, with summaries of each paper in all three languages, have come from Britain, the U.S.A., the U.S.S.R., France, Belgium, Denmark, Sweden, Germany, Turkey, Panama; they have covered a range of subjects and a width of technique that it would be wearisome to enumerate. The British and American editors, moreover, have buckled to it, so that the papers in English by non-English authors and the English summaries are now very much better than in the earlier numbers. The editors should, however, attend carefully to matters of terminology and symbols, so that words and letters—as, for instance, in describing optically active substances—should be used strictly in the way adopted by agreement between British and American chemical publishing organisations. Although μg appears now to have properly replaced γ , and ml. similarly to have ousted cm.³, there seems no reason for writing alpha and beta in full before the names of some isomerides, but not of others. A number of minor mistakes—some of them ordinary printers' errors—have been noticeable: they could be avoided by more rigorous editing of the English text. ("Formol" for formalin, "adenisine" for adenosine—also spelt correctly in the same paper—"glans" for gland, "not longer" for no longer, are just a few at random.) The punctuation could do with some discipline, particularly the use of commas, which would appear to follow no single one of the several conflicting systems still in use among writers of authority and in places to be devoid of logic altogether. These and other minor defects can, and undoubtedly will, be remedied in a relatively short time. The important thing is that *Biochimica et Biophysica Acta* has come and has come to stay.

A. L. BACHARACH.

MEDICINE

Lectures on the Liver and its Diseases. By H. P. HINSWORTH, M.D., F.R.C.P. [Pp. xiv + 204, with 58 figures, including 1 coloured plate.] (Oxford: Blackwell Scientific Publications, Ltd., 1947. 18s. 6d. net.)

THIS little book, which comprises the Lowell lectures given by Prof. Hinsworth at Boston in 1947, cannot fail to excite the interest and admiration of

pathologists and clinicians and, indeed, will have an appeal to the biologist who wishes to know something about the modern approach to disease processes. The author's aim has been to reduce to a few simple, but fundamental, reactions the complexities of liver upset. The logical approach is to consider in turn what happens in that organ when its blood supply is interfered with, its nutritional requirements are not met and noxious factors are allowed to bear upon the hepatic cells in their natural environment. From such considerations can be built up a rational account of the clinical pictures the physician has learnt to recognise by purely empirical methods. That this endeavour is not wholly successful is no fault of the author, for there are still many gaps in our knowledge of the function of the normal and abnormal hepatic cell. Despite a century of close scrutiny we are far from understanding the intricacies of the hepatic circulation, the part played by its nervous system, the dynamics underlying the production of the curious variety of lymph the organ turns out and, above all, the intriguing mystery of the closely guarded equilibrium maintained between the hepatic tissue and the organism's energy requirements. But Prof. Himsworth's lectures go far in clearing the way for a fresh attack on these puzzles, and his careful documentation of the significant advances in latter years should prove of great value, not only to the research worker, but also to the clinician who wishes to adjust his outlook to recent progress in hepatology. There are many excellent illustrations, some in colour, which show clearly the structural alterations most often met with in experimental and natural liver disease, and the index is a useful one. A few text errors, *e.g.* on pages 7, 21, 46, 54, 82 and 98, no doubt will be corrected in the next edition.

G. R. C.

PHILOSOPHY AND HISTORY OF SCIENCE

The Limits of Science. By the late LEON CHWISTEK, Ph.D. Translated from the Polish by H. C. BRODIE, M.A., and ARTHUR P. COLEMAN, Ph.D. [Pp. lviii + 347, with 12 figures.] (London: Kegan Paul, Trench, Trubner & Co., Ltd., 1948. 30s. net.)

THE appearance of this book is an event of deep significance; exactly why this should be so will only be fully manifest after close study. Meanwhile, two straightforward reasons can be discerned. Firstly, never before has a complete work of one of the greatest of Polish theoreticians become available in any "international" language. Secondly and consequentially, a sizable fraction of their monumental achievements in logistics and metascience—almost unknown hitherto in the West—will henceforward occupy a conscious place in the forefront of contemporary thought.

The volume before us is in fact a composite one, an "introduction" by Miss Brodie followed by the main text, which comprises a somewhat modified and enlarged translation of Chwistek's *Granice Nauki* first published in Poland in 1935.

Central in the theme is the establishment of a system of rational semantics—a wholly rigorous expressional calculus—by means of which the basic problems of science, philosophy and art may (formalistically) be solved. Essentially the method is an "apparatus," akin to a calculating machine, constructed out of pure ratiocination, and driven by a working substance which its builder calls "sound reason." Indeed, its mechanical nature is stressed: the ingredients of a problem enter at one end, and the solution

(often of incredible complexity and seeming improbability as judged by common-sense standards) emerges at the other. Competently handled, it is impossible to make mistakes.

The principle of "sound reason" is intractable: in Chwistek's eyes it means appeal to experience, coupled with a relentless rejection of all "absolutes," for example the "absolute knowledge" of Husserl and the phenomenologists. Oddly enough, this does not necessarily involve the total abandonment of metaphysics, rough-and-ready though they be in much of our modern natural science.

The afore-mentioned "introduction" is really a commentary. It is very helpful, but like all commentaries it offers a temptation to use it rather than the original. A certain textual carelessness is apparent—the unlovely word "formuli" confronts one several times, and there are some unholy mixtures of singulars and plurals, both here and throughout the volume.

One thing, however, is lacking. These pages are more than science, they are a work of art. How greatly therefore should we value a word-portrait of the artist; of Chwistek the thinker, controversialist, painter, a creative genius of the first order, and, to the end, the "gay crusader."

F. IAN G. RAWLINS.

L'Évolution de la Notion de Phénomène physique des Primitifs à Bohr et Louis de Broglie. By JEAN PELSENEER. [Pp. 177.] (Brussels: Office des Cours du Cercle des Sciences, 1947. Frs. 110.-.)

THIS book is based on a course of lectures on the history of scientific thought given at the University of Brussels in the winter 1946-7. The author states, in a short Preface, that it is published in the hope that it will fill a gap in the literature of the history of science, for, while the latter is assuming some importance at the present time, there is as yet no history of scientific thought.

The first four chapters are concerned mainly with Man's attitude towards natural phenomena. Beginning with primitive peoples, for whom nothing is impossible or absurd, M. Pelseneer goes on to discuss in turn the influence of religion on scientific thought, the importance of the idea of analogy, the evolution of the idea of natural law.

The fifth and last chapter, which occupies nearly two-thirds of the book, discusses Man's attempts to interpret the universe. Two extreme points of view are distinguished: the mechanistic and the conceptual. The former is represented by the atomism of Democritus, the mechanical theory of Descartes, the kinetic theory of matter. Its tendency is towards materialism and determinism. From the conceptual point of view, Nature appears as a vast network of mathematical symbols, representing such concepts as field, energy, entropy. The tendency here is towards idealism. These two extremes find expression in biology in physico-chemical explanations and teleological explanations respectively.

During the seventeenth and eighteenth centuries and the first half of the nineteenth century, complete mechanical explanations of the universe were held to be possible. The reaction against this view may be dated from 1850, when Clausius enunciated the second law of thermodynamics; during the second half of the century the concept of energy tended to displace the older mechanical concepts and to be regarded as the ultimate reality. At the present time, under the impact of the new physics, the general tendency is towards a non-mechanical, idealistic interpretation of the universe; as

Jeans says, "the universe begins to look more like a great thought than like a great machine."

The book can be recommended to all who are interested in the more philosophical aspects of the history of science. N. H. de V. H.

Nicolaus Copernicus : De Revolutionibus, Preface and Book I.

Translated by JOHN F. DOBSON, M.A., assisted by SELIG BRODETSKY, M.A., B.Sc., Ph.D. [Pp. 32, with 3 plates and 5 diagrams.] (London : Royal Astronomical Society, 1947. 3s. 6d. net.)

No complete English translation has yet been published of the historic book *De Revolutionibus* in which the sixteenth-century astronomer Copernicus established what is essentially the modern heliocentric theory of the solar system. The detailed calculations which occupy the latter part of that volume were soon to be superseded by the work of Kepler, and they are now of concern only to the student of the history of astronomy. However, a wider interest attaches to the earlier pages of the book. These include the dedicatory Preface addressed to the reigning Pope, Paul III, in which the author gives some account of the origins of his new planetary theory. They contain also the general description and diagram of the Copernican scheme of the Universe, the modern explanations of the seasons and of the precession of the equinoxes, and an early table of sines together with the set of propositions required to compute it.

These portions of the *De Revolutionibus* were translated from the Latin into English, shortly before his death, by the late Prof. John F. Dobson, formerly of Bristol University, assisted by Prof. S. Brodetsky of Leeds. Their version of the Preface and Book I (omitting only several trivial passages) has now been published by the Royal Astronomical Society.

The translation has been carried out with much care and scholarship, and full notes are provided on points of historical or scientific interest arising out of the text. It will enable the general reader to form at first hand an excellent idea of the essential contributions of Copernicus to astronomy, and of the quality (still strongly mediæval) of his thought. The translation is preceded by a brief sketch of the astronomer's career, in which, however, Tycho Brahe is strangely described as a "contemporary" of Copernicus acquainted with the latter's theories before their formal publication ; actually Brahe was not born until 1546, three years after the death of Copernicus and the appearance of his book. It may also be pointed out that the diagram included among the notes on page 30 gives the mistaken impression that the ancient astronomers made Mars the Earth's nearest celestial neighbour ; evidently *Mars's Orbit* should read *Moon's Orbit*.

The Frontispiece is a portrait of Copernicus from Gassendi's Biography, and the other two plates show pages of the *De Revolutionibus* reproduced from the original edition of 1543 ; the diagrams in the text are taken from the same source.

A. ARMITAGE.

The Royal Society Newton Tercentenary Celebrations 15-19 July, 1946. [Pp. xvi + 92, with 6 plates.] (Cambridge : at the University Press, 1947. 10s. 6d. net.)

THIS collection of addresses is not a biography of Newton, but in many readers it will whet the appetite for one ; it also suggests that there is material

for a biography differing somewhat from any that has so far appeared. In particular, the address prepared by the late Lord Keynes is based on much unpublished material, and his view is correspondingly divergent from the popular one. The wide range and great depth of Newton's work in mathematics and physics are, as was to be expected, well brought out by the different contributors; many who realise well enough that the *Principia*, the *Optics*, and the *Methods of Fluxions* do not exhaust the list of his achievements, will still be stirred and awed by the variety and the power of the discoveries listed in these pages. What may, however, surprise even those most familiar with the work on which his fame rests is the relatively small part which this entire subject played in his own emotions and life. Lord Keynes in fact goes so far as to say that he was the last of the magicians rather than the first of the age of reason; and at least it seems clear that Newton was not at all aiming at the mechanistic transformation of natural philosophy which he in fact completed. The *Lectiones Opticæ* were apparently regarded by him as a sort of clearing of the ground, something that could be got out of the way by mere analytical formalism; after that the real secrets of the Universe could be contemplated more immediately. And these secrets were mystical, cabbalistic, magical. At the age of thirty-seven, before he had published anything on mechanics or gravitation, he had decided to give up "philosophy"; it no longer seemed vital, and he grudged it time "which I think I can spend otherwise more to my own content and the good of others." The *Principia* was written only at the urgent request of his friends, and it was written in eighteen months; but at the same time, and for long before, he concentrated his energies for a considerable period twice a year on experiments that must be called alchemistic; he also spent a great deal of effort on interpretative studies of the more apocalyptic books of the Bible; and he believed without question that these were the real paths to truth. Probably even the oft-quoted passage about now and then finding on the seashore "a prettier shell than ordinary, while the great ocean of truth lay all undiscovered before me" indicates primarily a belief that the really important truths lay in a quite different direction from that which he had followed. It is much to be hoped that the full portrait sketched out in these pages may yet be drawn by a worthy master; meanwhile, the sketch itself is illuminating and attractive.

R. D'E. A.

Christian Huygens and the Development of Science in the Seventeenth Century. By A. E. BELL, Ph.D., M.Sc. [Pp. 220, with frontispiece, 6 plates and 61 figures.] (London: Edward Arnold & Co., 1947. 18s. net.)

NEWTON was born in 1642, the year in which Galileo died. Christian Huygens, the great Dutch mathematician and astronomer, overlapped both; born some thirteen years earlier than Newton, he was greatly influenced in his scientific work by the celebrated French philosopher Descartes. To us, for whom Newton's laws are the starting-point of modern physical science, it is extremely difficult to think ourselves into the frame of mind within which the pre-Newtonian natural philosophers worked; and it is the merit of the book before us that it makes it easier to understand why they, Huygens among them, thought as they did and for what reasons they worked at the problems which occupied them.

Dr. Bell has written an admirable book. It is divided into two parts,

the first a short, interesting and much-needed biography of Huygens, describing his early training in Holland, where his father, a distinguished scholar and diplomat, well known in France and England, encouraged the scholarly inclinations of his family, and whose international connections afforded the young Christian early opportunities of contact and discussion with foreign men of science. Later Christian Huygens lived and worked in Paris, under the patronage of the French Court, and was one of the founders and most active members of the Académie Royale des Sciences, under which he had an official position with facilities for experimental work. He also visited London, and maintained close contact and correspondence with many of the founders of the Royal Society.

The second part is devoted to an account of Huygens' scientific work—his contributions to mechanics and optics, his improvements of the astronomical telescope and the pendulum clock, his discovery of Saturn's ring, his study of the problem of gravity and development of the wave theory of light. Working in such fields the comparison with Newton is inevitable, and, although in some of these he was overshadowed and superseded by the great Englishman, Dr. Bell rightly claims for Huygens that he was one of the greatest scientific geniuses of all time. Not the least interesting part of the book is the assessment of Huygens' work and his place in science in relation to that of Newton.

T. M.

MISCELLANEOUS

Horology. By J. E. HASWELL, F.B.H.I. [Pp. xvi + 288, with 16 plates and 107 figures.] (London: Chapman & Hall, Ltd., 1947. 16s. net.)

THIS is a reprint of Haswell's book of 1928, including the supplement added to the 1937 edition. Mr. Haswell covers the fundamental principles of time measurement and time recording and describes the mechanism of clocks, watches and chronometers, but one might have hoped that a 1947 publication of this book would have contained a chapter on synchronous clocks, and at least some reference to the quartz clock. Every page of this book is a testimony of Mr. Haswell's comprehensive knowledge of horology, and he has written a valuable work of reference for the horological student. Indeed, Mr. Haswell's complete mastery of his subject has perhaps been responsible for limiting the usefulness of his book to some extent; horological technology has its own jargon, and there are many instances where definition of technical terms would have made the material more intelligible to the general reader.

A surprising conjecture on p. 106 should be queried. The implication that Alexander Bain's invention of the electric clock was an offshoot from a search for "perpetual motion" indicates a lack of appreciation of fundamental scientific principles on the part of Bain or Haswell—and one is inclined to acquit Bain.

The diagrams are exemplary and the book is singularly free from misprints, the only one noticed by the reviewer being the omission of a cube sign from one of the formulae on p. 151.

H. B.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

- Vector and Tensor Analysis.** By Louis Brand, Ch.E., E.E., Ph.D., Professor of Mathematics, University of Cincinnati. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. (Pp. xvi + 439, with 187 figures.) 33s. net.
- Integration in Finite Terms.** Liouville's Theory of Elementary Methods. By Joseph Fels Ritt, Davies Professor of Mathematics, Columbia University. New York: Columbia University Press; London: Oxford University Press, 1948. (Pp. x + 100.) 15s. net.
- Proceedings of a Symposium on Large-Scale Digital Calculating Machinery, 7-10 January 1947.** The Annals of the Computation Laboratory of Harvard University, Vol. XVI. Cambridge, Mass.: Harvard University Press; London: Oxford University Press, 1948. (Pp. xix + 302, with frontispiece and numerous figures.) 55s. net.
- The Size of the Universe.** By F. J. Hargreaves, F.R.A.S. Pelican Books No. A193. West Drayton: Penguin Books, Ltd., 1948. (Pp. 175, with 16 plates and 32 figures.) 1s. 6d. net.
- The General Properties of Matter.** By F. H. Newman, D.Sc., A.R.C.S., F.Inst.P., Professor of Physics, and V. H. L. Searle, M.Sc., Lecturer in Physics, University College of the South-West of England, Exeter. Fourth edition. London: Edward Arnold & Co., 1948. (Pp. xii + 431, with 103 figures and 19 tables.) 21s. net.
- Theory of Probability.** By Harold Jeffreys, M.A., D.Sc., F.R.S., Plumian Professor of Astronomy, University of Cambridge. Second edition. The International Series of Monographs on Physics. Oxford: at the Clarendon Press, 1948. (Pp. viii + 411, with 5 tables.) 30s. net.
- About Cosmic Rays.** By John G. Wilson, M.A., Ph.D., F.Inst.P. Sigma Introduction to Science 14. London: Sigma Books, Ltd., 1948. (Pp. 144, with 8 plates, 25 figures and 3 tables.) 8s. 6d. net.
- Les Actions à Distance. Les Forces Electrostatiques et la Gravitation. Relation $G = \frac{4}{3}(Nm)^2$.** By R. d'Aubry de Puymorin. Paris: Gauthier-Villars, 1948. (Pp. 78, with 11 figures.) Frs. 300.-.
- Die drei Grundphänomene der Physik und ihre Deutung.** By R. Orthner. Second edition. Vienna: Franz Deuticke, 1948. (Pp. viii + 112, with 9 figures.) Swiss Frs. 7.50.
- Report on Colour Terminology.** By a Committee of the Colour Group of the Physical Society. London: The Physical Society, 1948. (Pp. iv + 56.) 7s. net.
- Technical Optics, Vol. I.** By L. C. Martin, D.Sc., A.R.C.S., D.I.C., Professor of Technical Optics, Imperial College of Science and Technology, London. London: Sir Isaac Pitman & Sons, Ltd., 1948. (Pp. vi + 343, with 210 figures.) 40s. net.

- Discharge Lamps for Photography and Projection.** By H. K. Bourne, M.Sc., M.I.E.E., F.R.P.S., The Research Laboratory, The British Thomson-Houston Co., Ltd. London: Chapman & Hall, Ltd., 1948. (Pp. xvi + 424, with 186 figures and 45 tables.) 36s. net.
- Electron and Nuclear Physics.** By J. Barton Hoag, Ph.D., Head of the Department of Science, U.S. Coast Guard Academy. Third edition revised by S. A. Korff, Ph.D., Professor of Physics, College of Engineering, New York University. New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1948. (Pp. xii + 522, with 268 figures and 17 tables.) 27s. 6d. net.
- One Story of Radar.** By A. P. Rowe, C.B.E. Cambridge: at the University Press, 1948. (Pp. xii + 208, with 7 plates.) 8s. 6d. net.
- Principles of Radar.** By Denis Taylor, Ph.D., Superintendent, Telecommunications Research Establishment, Ministry of Supply, and C. H. Westcott, Ph.D., Physics Department, Birmingham University. Cambridge: at the University Press, 1948. (Pp. x + 141, with 5 plates, 52 figures and 4 tables.) 12s. 6d. net.
- Television.** By M. G. Scroggie, B.Sc., M.I.E.E., Consulting Radio Engineer. Second edition. London and Glasgow: Blackie & Son, Ltd., 1948. (Pp. x + 77, with 28 figures, including 9 plates.) 6s. net.
- Fundamental Principles of Ionospheric Transmission.** Department of Scientific and Industrial Research and Admiralty, Special Report No. 17. London: H.M. Stationery Office, 1948. (Pp. 81, with 69 figures.) 1s. 6d. net.
- Ultrahigh Frequency Transmission and Radiation.** By Nathan Marchand, Lecturer in Electrical Engineering, Columbia University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. (Pp. x + 322, with 140 figures.) 27s. net.
- Radio's Conquest of Space.** By Donald McNicol, Past President, The Institute of Radio Engineers. New York: Murray Hill Books, Inc.; London: Chapman & Hall, Ltd., 1946. (Pp. x + 374, with 53 figures.) 18s. net.
- Radio Receivers and Transmitters.** By S. W. Amos, B.Sc., A.M.Brit.I.R.E., Grad.I.E.E., Technical Instruction Writer, Engineering Training Department, British Broadcasting Corporation, and F. W. Kellaway, B.Sc., Principal, Apsley Day Continuation School, Hertfordshire. Second edition. London: Chapman & Hall, Ltd., 1948. (Pp. xii + 356, with 8 plates and 210 figures.) 25s. net.
- High Vacua. Principles, Production, and Measurement.** By Swami Jnanananda, D.Sc., Ph.D., F.Inst.P., Randall Laboratory of Physics, University of Michigan. New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1947. (Pp. xiv + 310, with 133 figures and 33 tables.) 30s. net.
- Velocity-Modulated Thermionic Tubes.** By A. H. W. Beek, B.Sc.(Eng.), A.M.I.E.E., Standard Telecommunication Laboratories. Modern Radio Technique Series. Cambridge: at the University Press, 1948. (Pp. x + 180, with 56 figures and 8 tables.) 15s. net.
- Protective Methods for A.C. Power Circuits and Machines.** By H. M. Marklew, M.Sc.(Eng.), A.M.I.E.E., Head of Engineering Department, Northampton College of Technology, and J. A. Collier, B.Sc.(Eng.), A.M.I.E.E., Lecturer in Electrical Engineering at the Borough Polytechnic, London. London and Glasgow: Blackie & Son, Ltd., 1948. (Pp. x + 142, with 98 figures.) 10s. net.

- Heat Conduction: With Engineering and Geological Applications.** By Leonard R. Ingersoll, Professor of Physics, University of Wisconsin, Otto J. Zobel, Member of the Technical Staff, Bell Telephone Laboratories, Inc., New York, and Alfred C. Ingersoll, Instructor in Civil Engineering, University of Wisconsin. International Series in Pure and Applied Physics. New York and London: McGraw-Hill Book Co., Inc., 1948. (Pp. xii + 278, with 42 figures and 21 tables.) 24s. net.
- Worked Examples in Engineering Science.** By H. J. Cooper, B.Sc., A.R.C.So.I., A.M.I.E.E., Head of Engineering Department, South-West Essex Technical College and School of Art. London: Hutchinson's Scientific and Technical Publications, 1948. (Pp. 159, with 59 figures.) 12s. 6d. net.
- Some Aspects of the Luminescence of Solids.** By F. A. Kröger, Natuurkundig Laboratorium, N.V. Philips' Gloeilampenfabrieken, Eindhoven. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. (Pp. xii + 310, with 74 figures and 27 tables.) 30s. net.
- Tables of Physical and Chemical Constants and some Mathematical Functions.** By G. W. C. Kaye, O.B.E., M.A., D.Sc., F.R.S., late Superintendent of the Physics Department, The National Physical Laboratory, and T. H. Laby, M.A., Sc.D., F.R.S., late Professor of Natural Philosophy, University of Melbourne. Tenth edition. London, New York, Toronto: Longmans, Green & Co., 1948. (Pp. viii + 194.) 21s. net.
- Theory of the Stability of Lyophobic Colloids.** By E. J. W. Verwey and J. Th. G. Overbeek, Natuurkundig Laboratorium, N.V. Philips' Gloeilampenfabrieken, Eindhoven. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. (Pp. xii + 205, with 54 figures and 22 tables.) 22s. 6d. net.
- An Introduction to Metallic Corrosion.** By Ulick R. Evans, M.A., Sc.D., Reader in the Science of Metallic Corrosion, Cambridge University. London: Edward Arnold & Co., 1948. (Pp. xxxvi + 211, with 66 figures and 19 tables.) 12s. 6d. net.
- A Scheme of Qualitative Organic Analysis.** By Frederick J. Smith, Ph.D., F.R.I.C., Senior Lecturer in Chemistry, City of Liverpool Technical College, and Emlyn Jones, M.Sc., F.R.I.C., Head of Chemistry Department, Rutherford College of Technology, Newcastle. London and Glasgow: Blackie & Son, Ltd., 1948. (Pp. viii + 320, with frontispiece, 5 figures and 89 tables.) 17s. 6d. net.
- The Chemistry of Acetylene and Related Compounds.** By Ernst David Bergmann, Daniel Sieff Research Institute, Rehovot, Palestine. Polytechnic Institute of Brooklyn, Lectures on Progress in Chemistry. New York and London: Interscience Publishers, Inc., 1948. (Pp. viii + 108.) 18s. net.
- Synthetic Methods of Organic Chemistry. A Thesaurus. Vol. I: 1942-1944.** By W. Theilheimer. Translated from the German by Hans Wynberg. New York and London: Interscience Publishers, Inc., 1948. (Pp. x + 254.) 30s. net.
- Isomerism and Isomerization of Organic Compounds.** By Ernst David Bergmann, Daniel Sieff Research Institute, Rehovot, Palestine. Polytechnic Institute of Brooklyn, Lectures on Progress in Chemistry. New York and London: Interscience Publishers, Inc., 1948. (Pp. xii + 138.) 21s. net.

- Organic Syntheses.** Vol. XXVII. R. L. Shriner, Editor-in-Chief. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. (Pp. vi + 121.) 13s. 6d. net.
- Advances in Carbohydrate Chemistry.** Vol. 3. Edited by W. W. Pigman, The Institute of Paper Chemistry, Appleton, Wisconsin, and M. L. Wolfrom, The Ohio State University, Columbus, Ohio. New York: Academic Press, Inc., 1948. (Pp. xxiv + 424, with 18 figures and 54 tables.) \$8.50.
- Chemistry of the Carbohydrates.** By William Ward Pigman, The Institute of Paper Chemistry, Appleton, Wisconsin, and Rudolph Maximilian Goepf, Jr., The Atlas Powder Co., Wilmington, Delaware. New York: Academic Press, Inc., 1948. (Pp. xviii + 748, with 35 figures and 68 tables.) \$10.80.
- The Chemistry of Organic Cyanogen Compounds.** By Vartkes Migrdichian, Chemist, American Cyanamid Company, Stamford, Connecticut. American Chemical Society Monograph Series. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1947. (Pp. x + 460.) 72s. net.
- Progress in the Theory of the Physical Properties of Glass.** By Dr. J. M. Stevels, Natuurkundig Laboratorium, N.V. Philips' Gloeilampenfabrieken, Eindhoven. Monographs on the Progress of Research in Holland during the War. Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1948. (Pp. xii + 104, with 26 figures and 22 tables.) 10s. net.
- Synthetic Petroleum from the Synthine Process.** By B. H. Weil, M.S. in Ch.E., Research Associate Professor and Head, Technical Information Division, State Engineering Experiment Station, Georgia School of Technology, and J. C. Lane, B.S. in Ch.E., Head, Information Section, Chemistry Division, Gulf Research & Development Company, Pittsburgh, Pennsylvania. Brooklyn: Remsen Press Division, Chemical Publishing Co., Inc., 1948. (Pp. xii + 303, with 20 figures and 26 tables.) \$6.75.
- Chemical Process Principles.** By Olaf A. Hougen and Kenneth M. Watson, Professors of Chemical Engineering, University of Wisconsin. Part II: Thermodynamics; Part III: Kinetics and Catalysis. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1947. (Part II: pp. i-xvi, 437-804, xvii-xlvi, figs. 98-166 and tables XXIII-LI; Part III: pp. i-xvi, 805-1107, xvii-xlvi, figs. 167-230, tables LII-LXI.) Part II: 30s. net; Part III: 27s. net.
- Flameproofing Textile Fabrics.** Prepared by a Staff of Specialists under the Editorship of Robert W. Little, Major, Q.M.C., under Research Project #27 of the National Research Council, National Academy of Sciences. American Chemical Society Monograph Series. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1947. (Pp. xx + 410, with 81 figures and 92 tables.) 40s. 6d. net.
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- Vorräte und Verteilung der mineralischen Rohstoffe.** Von Dr. phil. Felix Machatschki, o. Professor an der Universität Wien. Wien: Springer-Verlag, 1948. (Pp. viii + 191, with 6 figures and 13 tables.) Swiss Frs. 16.-.
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INDEX TO VOL. XXXVI

1948

GENERAL INDEX

- Accuracy of Observation, The Natural Limit of, R. Fürth, 396
- Adenosine triphosphate, synthesis of, 708
- Adrenal glands in relation to glycogen storage, 22
- "Agroicide" insecticides, 147
- Aircraft, The Spraying of Liquids from, E. G. Richardson, 206
- Alcohols used in perfumery, 38
- Aldehydes used in perfumery, 42
- Alkali halides, colour centres in, 628
- Americium, 280
- Anæmia, pernicious, 666
— folic acid and, 670
- Analytica Chimica Acta*, 139
- Anemometry, 472
- Anthropology, Recent Advances in Physical, N. A. Barnicot, 702
- Anti-anæmia factors, 672
- Antibiotics in the control of weeds, 687
- Appointments, 141, 325, 528, 711
- Arago, friend of Fresnel, 595
- Astronomy, Recent Advances in, A. Hunter, 70, 262, 460, 642
- Atmosphere, tropical, structure of, 86
- Atmospheric turbulence and wind structure, 472
- Atom bomb, 142
- Atomic Energy for the Non-Specialist, N. Feather (*Essay Review*), 535
structure, 439
- Barker (D.), Recent Advances in Zoology, 518
- Barnicot (N. A.), Recent Advances in Physical Anthropology, 702
- Binary stars, spectra of close, 645
- Biochemistry, Recent Advances in, W. O. Kermack, 283, 666
- Blood-groups, 702
- Blood-sugar level in human body, 16
- Books received, 188, 379, 572, 770
- Boswell (P. G. H.), The Thixotropy of Certain Sedimentary Rocks, 412
- Botany, Recent Advances in, W. H. Pearall, 127, 513, 685
- Boutry (G. A.), Augustin Fresnel: His Time, Life and Work, 1788-1827, 587
- British Anti-Lewisite, 287
- Brownian motion, 397
- Butterflies, scale pigments of, 315
- Camera, electron-diffraction, 5
lens for aeroplane mapping, 531
- Carbohydrate food in the animal body, oxidation of, 28; storage of, 18
- Cell walls, plant, structure of, 127
- Cells, plant, salt relations of, 690
— water relations of, 306
- Chemical compounds, specificity of the action of, on living tissues, 283
- Chemistry, A Century of British, T. S. Wheeler (*Essay Review*), 148
- General and Physical, Recent Advances in, J. W. Smith, 101, 273, 483, 656
- Organic, Recent Advances in, A. W. Johnson, 112, 496
- Chinicol, 288
- Cholesterol metabolism, study of, with isotopes, 248
- Oiliate Protozoa, Genetics of the, R. F. Ewer, 460
- Cirrhosis of the liver, 578
- "Clathrate" compounds, 664
- Clay Consolidation, Theoretical Aspects of, H. H. Macey, 633
- Clay-humus complex, 304
- Cloud Chamber Operation, Some Basic Features of, J. G. Wilson, 226
- Coal, cyclical nature of deposits of, 194
formation of, 193
- Colchicine bibliography, 513
- Colour centres in alkali halides, 628
theory of, 445
- Comparators, 55
- Condensation process of cloud chamber operation, 226
- Conduction, metallic, 616
- Consolidation in soil mechanics, 633
- Corpus allatum in insects, functions of the, 698
- Coulson (C. A.), Wave Mechanics in Physics, Chemistry and Biology, 486
- Counters, crystal, 468, 656
- Couplings, electro-magnetic, 327
- Crustal Movements, The Relation of Rhythmic Sedimentation to, A. E. Trueman, 193
- Crystals, use of, for the detection of ionising radiations and particles, 468, 656
- Curium, 280
- Curtis (W. E.), German War Research (*Essay Review*), 338
- Deaths, 141, 326, 529, 712
- Diabetes mellitus*, 13

- Diamond dies for wire drawing, 332
 Dipole moment measurements, solvent effects in, 483
 Doctor, The Training of a, H. C. Edwards (*Essay Review*), 721
 Echo-sounder, 424
 Ecology and land utilisation, 517
 Edwards (H. C.), The Training of a Doctor (*Essay Review*), 721
 Electrical equipment, testing of, 329
 noise, 268
 Properties of Nervous Tissue, W. F. Floyd, 214
 Electrolytic polishing of metal surfaces, 328
 Electron microscope, 8
 Optics, G. I. Finch and H. Wilman, 1
 Electron-diffraction camera, 5
 Electronic Structure of Solids, E. E. Schneider, 614
 Electronics, Some Applications of, in Metrology, E. J. B. Willey, 55
 Electrons in metals, 443, 614
 Emeléus (H. J.), The Silicones, 385
 Engineering, thixotropic sediments and, 418
 Entomology, Recent Advances in, A. D. Lees, 313, 695
 Equatorial asterlies, structure of, 87
 Esters used in perfumery, 50
 Ethers used in perfumery, 42
Euplotes patella, genetics of, 456
 Ewer (R. F.), The Genetics of the Ciliate Protozoa, 450
 Fat metabolism, investigation of, with isotopes, 245
 Feather (N.), Atomic Energy for the Non-Specialist (*Essay Review*), 535
 Fermi-Dirac statistics and metallic conduction, 619
 Fibres, swelling and shrinking of protein, 660
 Fibrosis of the liver, 578
 Finch (G. I.) [H. Wilman and], Electron Optics, 1
 Flares, solar, 262
 Floyd (W. F.), Electrical Properties of Nervous Tissue, 214
 Folic acid, 667
 — and human anemias, 670
 Fraunhofer's Collaborator, M. Schofield, 526
 Fresnel, Augustin: His Time, Life and Work, 1758-1827, G.-A. Boutry, 587
 Fungi, A Study of, C. T. Ingold (*Essay Review*), 726
 Fürth (R.), The Natural Limit of Accuracy of Observation, 396
 Gases, Inert, Fifty Years of the, M. Schofield, 66
 Gels, swelling and shrinking of, 656
 Genetics of the Ciliate Protozoa, R. F. Ewer, 450
 researches in, 514
 serological, 702
 Geochemistry, 506
 Geology, Recent Advances in, G. W. Tyrrell, 121, 292, 506, 676
Geophysical Abstracts, resumption of publication of, 529
 German War Research, W. E. Curtis (*Essay Review*), 338
 Glass, solder, 714
 Glucose, conversion of, to fatty acids, 26
 formation of tissue glycogen from, 17
 Glycogen storage in animal body, adrenal glands in relation to, 22;
 anterior pituitary lobe in relation to, 24;
 influence of insulin on, 19
 tissue, formation of, from glucose, 17
 Granite problem, 676
 Gregory (J. C.), Indeterminism and the Wish, 605
 Guinand's bicentenary, 526
 Hall effect, 624
 Hearing aids, 330
 Hexokinase in animal body, 30
 Himsworth (H. P.), Nutrition and Liver Disease, 577
 Holmes (W.), Recent Advances in Zoology, 131
 Honours, 140, 323, 527, 710
 Humus, formation of, 300
 Hunter (A.), Recent Advances in Astronomy, 70, 262, 460, 642
 Hydrogen, radioactive, 101
 8-Hydroxyquinoline, action of, on fungi and bacteria, 288
 Illinois State Geological Survey, 716
 Indeterminism and the Wish, J. C. Gregory, 605
 India, agricultural meteorology in, 716
 scientific research in, 712
 Inert Gases, Fifty Years of the, M. Schofield, 66
 Ingold (C. T.), A Study of Fungi (*Essay Review*), 726
 Innervation of skeletal muscle, 518
 Insect cranium, structure of, 313
 distribution, historical aspect of, 695
 repellents, 319
 Insecticidal mists and flying insects, 700
 Insects, functions of the corpus allatum in, 698
 Insulin, The Mechanism of Action of, F. G. Young, 13
 Inventor and his world, 718
 Isotopes, application of, to analytical purposes, 257
 radioactive, some applications of, 108, 252
 separation of, 78, 271
 The Use of, in Biology, G. Popják, 239

- Jacks (G. V.), Recent Advances in Pedology, 300
- Johnson (A. W.), Recent Advances in Organic Chemistry, 112, 496
- Kermack (W. O.), Recent Advances in Biochemistry, 283, 666
- Ketogenesis, influence of insulin on, 33
- Ketones used in perfumery, 47
- Leaves, water relations in, 689
- Lees (A. D.), Recent Advances in Entomology, 313, 695
- Lenses for microwaves, artificial dielectric, 715
- Lepidoptera, scale pigments of, 315
- Light, pressure of, making use of the, 272
- Liver, anti-pernicious anemia factor of, 672
- Disease, Nutrition and, H. P. Hims-worth, 577
- Macey (H. H.), Theoretical Aspects of Clay Consolidation, 633
- Mammals, classification of, 131
- Marine biological research, 427
- Martian "canals", 70
- Mass spectrographs, 78
- Massey (H. S. W.), Mathematical Physics (*Essay Review*), 334
- Mathematical Physics, H. S. W. Massey (*Essay Review*), 334
- tables for science and industry, 709
- Measurement, accuracy of, 396
- Metals, electric behaviour in, 443, 614
- Meteorology, agricultural, in India, 716
- Recent Advances in, M. K. Miles, 86 ; P. A. Sheppard, 472
- Meteors, daylight, 74
- Metrology, Some Applications of Electronics in, E. J. B. Willey, 55
- Micrometers, 57
- Microscope, electron, 8
- Microseisms and tropical storms, 98
- Microtecnic, 139
- Miles (M. K.), Recent Advances in Meteorology, 86
- Molecular structure, 440
- Mould growth in houses in New Zealand, prevention of, 146
- Mt. Palomar telescope, 70
- Muscle, skeletal, innervation of, 518
- Mussels, cleansing of, 530
- Necrosis of the liver, 578
- Neptune, discovery of the planet, 713
- Neptunium, 276
- Nervous Tissue, Electrical Properties of, W. F. Floyd, 214
- Nuclear physics, some elementary principles of, 240
- Nutrition and Liver Disease, H. P. Hims-worth, 577
- Observation, The Natural Limit of Accuracy of, R. Fürth, 396
- Ocean research, 423
- Oedometer, 683
- Oils, silicone, 391
- Optics, Fresnel's work on, 593
- Oxine, 288
- Paramecium aurelia*, genetics of, 450
- Pearsall (W. H.), Recent Advances in Botany, 127, 513, 685
- Pedology, Recent Advances in, G. V. Jacks, 300
- Perfumes, Synthetic, T. F. West, 38
- Periodic table of the elements, 626
- Petrology, 676
- Phosphorus, impurity centres in, 628
- Phosphorus, radioactive, in metabolic studies, 253
- Physica, Recent Advances in, F. A. Vick, 78, 268, 468, 650
- Physiology of marine animals, 433
- Piezoelectric crystals, 145
- Pigments in marine animals, 432
- scale, of butterflies, 315
- Planet, a new minor, 460
- Planetary satellite, a new, 466
- Plankton, study of, 430
- Plant cell walls, structure of, 127
- cells, salt relations of, 690
- water relations of, 306
- growth substances, 685
- Physiology, Recent Advances in, W. Stiles, 306, 690
- viruses, 515
- Plants, Brazilian, 130
- responses of, to light, 129
- Plutonium, 277
- Polarimeter, photoelectric, 64
- Popják (G.), The Use of Isotopes in Biology, 239
- Porous materials, absorption of sound on, 322
- Protein and amino-acid metabolism, investigation of, with isotopes, 250
- Protoplasm, swelling and shrinking of, 663
- Protozoa, Ciliate, Genetics of the, R. F. Ewer, 450
- Pteric acid synthesis and sulphanilamide, 675
- Pteroylglutamic acid, 668
- Pulse Code Modulation, 143, 332
- Quantum mechanical limitation of observational accuracy, 407
- Radio fading, 144
- Radioactive hydrogen, 101
- isotopes, some applications of, 108, 252
- Radioactivity in rocks, 508
- Radium content of ocean-floor sediments, 508
- Relaxation time of measuring devices, 401

- Resins, silicone, 392
 Rh-system of antigens, 703
 Richardson (E. G.), The Spraying of Liquids from Aircraft, 206
 Rocks, Sedimentary, The Thixotropy of Certain, P. G. H. Boswell, 412
 Rubber, silicone, 393
 Russell (F. S.), Pure and Applied Science of the Sea, 423

 Schneider (E. E.), The Electronic Structure of Solids, 614
 Schofield (M.), Fifty Years of the Inert Gases, 66; Fraunhofer's Collaborator, 526
Science News VI, 720
 Scientific Instrument Manufacturers' Association Handbook, 720
 Sea, Pure and Applied Science of the, F. S. Russell, 423
 water, chemistry and physics of, 428
 Sedimentary Rocks, The Thixotropy of Certain, P. G. H. Boswell, 412
 Sedimentation, Rhythmic, The Relation of, to Crustal Movements, A. E. Trueman, 193
 Sewage effluents, treatment of, 530
 Sheppard (P. A.), Recent Advances in Meteorology, 472
 Shrinking, Faraday Society discussion on, 656
 Silicones, The, H. J. Emeléus, 385
 Smith (J. W.), Recent Advances in General and Physical Chemistry, 101, 273, 483, 656
 Solar corona, the hot, 462
 flares, 262
 Solids, The Electronic Structure of, E. E. Schneider, 614
 Solvent effects in dipole moment measurements, 483
 Sound, absorption of, on porous materials, 322
 Spraying of Liquids from Aircraft, E. G. Richardson, 206
 Starch grains, swelling of, 662
 Stars, distribution of chemical elements in, 72
 double, 645
 Stellar absolute magnitudes, 642
 magnetic fields, 467
 Stiles (W.), Recent Advances in Plant Physiology, 306, 690
 Streptidine, structure of, 114
 Streptobiosamine, structure of, 115
 Streptomycin, chemistry of, 112
 Sulphanilamide, 385, 675
 Supermalloy, 524
 Supersaturation by adiabatic expansion in cloud chamber operation, 230
 Supersonics, 650
 Swelling, Faraday Society discussion on, 656

 Synthesis of vitamin A and derived products, 496

 Taxonomy, 131
 Tectonics, 121
 Telephony, long-distance, 143
 Telescope, 200-in., 70
 Thixotropy of Certain Sedimentary Rocks, P. G. H. Boswell, 412
 Time determination with a transit circle, 75
 Tobacco seeds, uses of, 718
 Trace elements in rocks, 512
 — in sea water, 430
 Trans-uranic elements and their chemical significance, 273
 Tritium, 101, 283
 Tropical atmosphere, structure of, 86
 storms, 91
 Trueman (A. E.), The Relation of Rhythmic Sedimentation to Crustal Movements, 193
 Tyrrell (G. W.), Recent Advances in Geology, 121, 292, 506, 676

 Ultrasonics, 650
 Universe, structure of, 73
 Uronic acids in soils, 303

 Vick (F. A.), Recent Advances in Physics, 78, 268, 468, 650
 Viruses, plant, 515
 Viscosity of mixtures of "normal fluids" 714
 Vitamin A, synthesis of, 496
 Vitamin B₁₂, 672
 Volcanology, 292

 Wave Mechanics in Physics, Chemistry and Biology, C. A. Coulson, 436
 positive-hole theory of, 624
 West (T. F.), Synthetic Perfumes, 38
 Wheeler (T. S.), A Century of British Chemistry (*Essay Review*), 148
 Willey (E. J. B.), Some Applications of Electronics in Metrology, 55
 Wilman (H.) [G. I. Finch and], Electron Optics, 1
 Wilson (J. G.), Some Basic Features of Cloud Chamber Operation, 226
 Wind structure and atmospheric turbulence, 472
 Wireworms, food-finding activities of, 317
 Woodworm in furniture, 718

 Young (F. G.), The Mechanism of Action of Insulin, 13

 Zoology, Recent Advances in, W. Holmes, 181; D. Barker, 518

INDEX OF REVIEWS

- Adhesives, The Technology of, J. Delmonte, 364
- Adolph (E. F.), Physiology of Man in the Desert, 565
- Aerodynamics, Elementary Applied, P. E. Hemke, 350
- Aerodynamics, Theoretical, L. M. Milne-Thomson, 734
- Aircraft Engines, Vol. II, 2nd ed., A. W. Judge, 157
- Aircraft Strength of Materials, H. D. Conway, 350
- Algebraic Geometry, Methods of, Vol. I, W. V. D. Hodge and D. Pedoe, 731
- Alsoe, S. A. Goudsmit, 741
- Altenburg (E.), Genetics, 562
- Ambrose (E. R.) [P. Sporn, T. Baumeister and], Heat Pumps, 746
- Andrade (E. N. da C.), The Atom and its Energy, 535
- Andrews (H. N.), Ancient Plants and the World they Lived in, 558
- Animal Nutrition, 2nd ed., L. A. Maynard, 177
- Anson (M. L.) [J. T. Edsall and], Advances in Protein Chemistry, Vol. III, 164
- Architectural Acoustics, Applied, M. Rettinger, 154
- Arkell (W. J.), The Geology of Oxford, 365
- Arnold (C. A.), An Introduction to Paleobotany, 558
- Artificial Insemination of Farm Animals, ed. by E. J. Perry, 176
- Asdell (S. A.), Patterns of Mammalian Reproduction, 372
- Association of British Chemical Manufacturers, Safety Rules for Use in Chemical Works, Pt. I: Model Rules, 3rd ed., 757
- Atom, The, 3rd ed., Sir G. Thomson, 535
- Atom and its Energy, E. N. da C. Andrade, 535
- Atomic Energy, What is ?, K. Mendelssohn, 544
- Atomic Physics, Introduction to, H. Semat, 740
- Atomic Physics, Introduction to, 2nd ed., S. Tolansky, 153
- Atoms, Meet the, O. R. Frisch, 545
- Avery (G. S.) [E. B. Johnson and], Hormones and Horticulture, 760
- Baldwin (E.), Dynamic Aspects of Biochemistry, 180
- Barlow (H. M.), Micro Waves and Wave Guides, 546
- Baumeister (T.) [P. Sporn, E. R. Ambrose and], Heat Pumps, 746
- Bee Community, The, F. H. Metcalf, 762
- Belcher (R.) [M. B. Thompson and], An Introduction to Quantitative Inorganic Analysis, 159
- Bell (A. E.), Christian Huygens and the Development of Science in the Seventeenth Century, 768
- Bennett (H.), Concise Chemical and Technical Dictionary, 165; Practical Emulsions, 2nd ed., 363
- Bessel Functions, Applied, F. E. Reiton, 342
- Binder (R. C.), Fluid Mechanics, 155
- Biochemistry, Dynamic Aspects of, E. Baldwin, 180
- Biochimica et Biophysica Acta*, Vol. I, Nos. 1-8; Vol. II, No. 1, 763
- Biology, Essentials of, W. F. Wheeler, 174
- Biology, General, 2nd ed., P. O. Strausbaugh and B. R. Weimer, 561
- Blackie (W. K.), Malaria, 374
- Boiler House and Power Station Chemistry, 2nd ed., W. Francis, 755
- Bouwers (A.), Achievements in Optics, 347
- Boyd (W. C.), Fundamentals of Immunology, 2nd ed., 567
- British Chemists, A. Findlay and W. H. Mills, 148
- British Medical Association, The Training of a Doctor, 721
- Bruges (E. A.) [E. W. Geyer and], Tables of Properties of Gases, 751
- Budker (P.), La Vie des Requins, 368
- Buxton (P. A.), The Louse, 2nd ed., 370
- Calculus, Advanced, D. V. Widder, 342
- Campbell (L.) [L. Jacchia and], The Story of Variable Stars, 539
- Canadian Bureau of Mines, Canadian Mineral Industry in 1945, 172
- Cantril (H.) [M. Sherif and], The Psychology of Ego-Involvements, 571
- Carr (T. H.), Sub-station Practice, 355
- Cathode Ray Oscillograph in Industry, 3rd ed., W. Wilson, 742
- Cellulose Chemistry, The Methods of, 2nd ed., C. Doré, 551
- Century of British Chemistry, F. Sherwood Taylor, 375
- Chapman (H. H.) [W. K. Meyer and], Forest Valuation, 760
- Chemical and Technical Dictionary, Concise, ed. by H. Bennett, 165
- Chemical Engineering Fundamentals, C. G. Kirkbride, 755
- Chemical Kinetics of the Bacterial Cell, C. N. Hinshelwood, 360

- Chemical Principles, A Rational Approach to, J. A. Cranston, 357
- Chemical Society 1841-1941, T. S. Moore and J. C. Philip, 375
- Chemical Technology, Encyclopedia of, Vol. I: A to Anthrimides, ed. by R. E. Kirk and D. F. Othmer, 756
- Chemistry, A Practical, for Higher School Certificate and Intermediate Students, 3rd ed., G. P. McHugh, 159
- Chemistry for our Times, E. C. Weaver and L. S. Foster, 357
- Chemistry, Fundamental, 2nd ed., H. G. Dehning, 747
- Chemistry, Lecture Experiments in, 3rd ed., G. Fowles, 358
- Chester (K. S.), Nature and Prevention of Plant Diseases, 2nd ed., 758
- Chimie Organique, Vols. I and II, A. Kirmann, 550
- Chwistek (L.), The Limits of Science, trans. by H. C. Brodie and A. P. Coleman, 785
- Clark (W.), Photography by Infrared, 2nd ed., 348
- Clarke (L.), Manual for Process Engineering Calculations, 356
- Cold Spring Harbor Symposia on Quantitative Biology, Vol. XI: Heredity and Variation in Microorganisms, 563; Vol. XII: Nucleic Acids and Nucleoproteins, 762
- Conant (J. B.), On Understanding Science, 569
- Conseil Permanent International pour l'Exploration de la Mer, Annales Biologiques, Vol. No. II, 1942-5, 562
- Continents, The Formation of the, by Convection, G. F. S. Hills, 758
- Controlled Atmospheres for the Heat Treatment of Metals, I. Jenkins, 160
- Conway (H. D.), Aircraft Strength of Materials, 350
- Cook (W. R. I.) [R. C. McLean and], Practical Field Ecology, 172
- Cooper (H. J.), Scientific Instruments, 747
- Copernicus (Nicolaus), De Revolutionibus, Preface and Book I, trans. by J. F. Dobson, 767
- Cosmic Rays, L. Jánossy, 739
- Coselett (V. E.), Electron Microscope, 546
- Coward (K. H.), The Biological Standardisation of the Vitamins, 2nd ed., 566
- Cranston (J. A.), A Rational Approach to Chemical Principles, 357
- Craft Laboratory, Harvard University, Electronic Circuits and Tubes, 743
- Culver (C. A.), Theory and Applications of Electricity and Magnetism, 541
- Curare, A. B. McIntyre, 373
- Cyanogen Compounds, 2nd ed., H. E. Williams, 748
- Das (M. M.), Electricity, 541
- Davies (A. Morley), An Introduction to Palaeontology, 2nd ed., 364
- Davies (M.), An Outline of the Development of Science, 570
- Davies (O. L.), Statistical Methods in Research and Production, 185
- Delmonte (J.), The Technology of Adhesives, 364
- Demerec (M.), Advances in Genetics, Vol. I, 176
- Deming (H. G.), Fundamental Chemistry, 2nd ed., 747
- Den Hartog (J. P.), Mechanical Vibrations, 3rd ed., 156
- Destriau (G.), Physique appliquée des Installations à Rayons X., 153
- Dethier (V. G.), Chemical Insect Attractants and Repellents, 759
- Detoxication Mechanisms, R. T. Williams, 567
- Dissociation Energies, A. G. Gaydon, 549
- Dobson (J. F.), Translation of Nicolaus Copernicus: De Revolutionibus, Preface and Book I, 767
- Doctor, The Training of a, British Medical Association, 721
- Dorée (C.), The Methods of Cellulose Chemistry, 2nd ed., 551
- Drugs from Plants, T. I. Williams, 373
- Dynamische Reaktionspathologie, K. von Neergaard, 568
- Earth, The Pulse of the, 2nd ed., J. H. F. Umbgrove, 170
- Eastman (E. D.) [G. K. Rollefson and], Physical Chemistry, 749
- Edsall (J. T.) [M. L. Anson and], Advances in Protein Chemistry, Vol. III, 164
- Electricity, M. M. Das, 541
- Electricity and Magnetism, Theory and Applications of, C. A. Culver, 541
- Electromagnetism, J. C. Slater and N. H. Frank, 542
- Electron Microscope, V. E. Coselett, 546
- Electronic Circuits and Tubes, Craft Laboratory, Harvard University, 743
- Electronics and their Application in Industry and Research, ed. by B. Lovell, 352
- Electrons in Gases, Sir J. Townsend, 738
- Electrons (+ and -), Protons, Photons, Neutrons, Mesotrons, and Cosmic Rays, 2nd ed., R. A. Millikan, 544
- Embryology Experimental, in the Netherlands 1940-1945, M. W. Woerdeman and C. P. Raven, 369
- Emulsions, Practical, 2nd ed., H. Bennett, 363
- Encyclopedia, van Nostrand's Scientific, 2nd ed., 196

- Engineering and Industrial Science, Records and Research in, 2nd ed., J. E. Holmstrom, 354
- English-Spanish Chemical and Medical Dictionary, M. Goldberg, 377
- Enogat (J.), *Science News* V, 571
- Enzymes, Chemistry and Methods of, 2nd ed., J. B. Sumner and G. F. Somers, 179
- Escalator Method in Engineering and Vibration Problems, J. Morris, 351
- Evans (E. Price) [A. G. Tansley and], *Plant Ecology and the School*, 172
- Évolution de la Notion de Phénomène physique des Primitifs à Bohr et Louis de Broglie, J. Pelseneer, 766
- Exclusion Principle and Quantum Mechanics, W. Pauli, 739
- Experiments, The Design of, 4th ed., R. A. Fisher, 184
- Fats, The Chemical Constitution of Natural, 2nd ed., T. P. Hilditch, 163
- Fatty Acids, K. S. Markley, 362
- Ferromagnetic Materials, New Developments in, J. L. Snoek, 738
- Field Ecology, Practical, R. C. McLean and W. R. Ivimey Cook, 172
- Findlay (A.) [W. H. Mills and], *British Chemists*, 148
- Fire and Explosion Hazards, The Technology of, Vols. I and II, R. C. Smart, 168
- Fisher (R. A.), *The Design of Experiments*, 4th ed., 184
- Flint (R. F.), *Glacial Geology and the Pleistocene Epoch*, 557
- Fluid Mechanics, R. C. Binder, 155
- Foods, Their Values and Management, H. C. Sherman, 178
- Fooks (E.), *X-Ray the City* I, 187
- Forest Valuation, H. H. Chapman and W. K. Meyer, 760
- Fossiles Tertiaires du Bassin de Paris, Catalogue des, R. Furon and R. Soyer, 558
- Foster (L. S.) [E. C. Weaver and], *Chemistry for our Times*, 357
- Fowles (G.), *Lecture Experiments in Chemistry*, 3rd ed., 358
- Francis (W.), *Boiler House and Power Station Chemistry*, 2nd ed., 755
- Frank (N. H.) [J. C. Slater and], *Electromagnetism*, 542; *Mechanics*, 735
- Frisch (O. R.), *Meet the Atoms*, 545
- Functions, Theory of, rev. ed., J. F. Ritt, 735
- Fungi, The, Vols. I and II, F. A. and F. T. Wolf, 726
- Furon (R.) [R. Soyer and], *Catalogue des Fossiles Tertiaires du Bassin de Paris*, 558
- Galaxies, H. Shapley, 539
- Garner (C. S.) [D. M. Yost, H. Russell and], *The Rare Earth Elements and their Compounds*, 358
- Gas Turbines, Modern, A. W. Judge, 156
- Gases, Tables of Properties of, E. W. Geyer and E. A. Bruges, 751
- Gaydon (A. G.), *Dissociation Energies*, 549
- Genetics, E. Altenburg, 562
- Genetics, *Advances in*, Vol. I, ed. by M. Demerec, 176
- Geology of Oxford, W. J. Arkell, 365
- German Research in World War II, L. E. Simon, 338
- Geyer (E. W.) [E. A. Bruges and], *Tables of Properties of Gases*, 751
- Glacial Geology and the Pleistocene Epoch, R. F. Flint, 557
- Goddard (F. W.) [E. J. F. James and], *The Elements of Physical Chemistry*, 2nd ed., 359
- Goldberg (M.), *English-Spanish Chemical and Medical Dictionary*, 377
- Goodstein (R. L.), *A Text Book of Mathematical Analysis*, 732
- Gorter (C. J.), *Paramagnetic Relaxation*, 737
- Gossypium, The Evolution of, J. B. Hutchinson, R. A. Silow and S. G. Stephens, 560
- Goudge (M. F.), *Limestones of Canada, Pt. V: Western Canada*, 171
- Goudsmit (S. A.), *Alcos*, 741
- Griffiths (E.), *Methods of Measuring Temperature*, 3rd ed., 346
- Groggins (P. H.), *Unit Processes in Organic Synthesis*, 3rd ed., 162
- Gross (G. L.) [J. B. Rosser, R. R. Newton and], *Mathematical Theory of Rocket Flight*, 548
- Gums, The Water-Soluble, C. L. Mantell, 753
- Hall (V. E.), *Annual Review of Physiology*, Vol. IX, 183
- Harris (R. S.) [K. V. Thimann and], *Vitamins and Hormones*, Vol. IV, 181
- Harrison (A. E.), *Klystron Tubes*, 352
- Haslett (A. W.), *Science in Transition*, 377
- Haswell (J. E.), *Horology*, 769
- Heat Pumps, P. Spora, E. R. Ambrose and T. Baumsister, 746
- "Heat-Treater", *Fundamental Principles and Applications of Induction Heating*, 346
- Hemke (P. E.), *Elementary Applied Aerodynamics*, 359
- Heredity and Variation in Microorganisms, Cold Spring Harbor Symposium on Quantitative Biology, Vol. XI, 563
- Hickinbottom (W. J.), *Reactions of Organic Compounds*, 2nd ed., 751

- Hilditch (T. P.), *The Chemical Constitution of Natural Fats*, 2nd ed., 163
- Hill (R. D.) [L. H. Martin and], *A Manual of Vacuum Practice*, 351
- Hills (G. F. S.), *The Formation of the Continents by Convection*, 758
- Himsworth (H. P.), *Lectures on the Liver and its Diseases*, 764
- Hinshelwood (C. N.), *The Chemical Kinetics of the Bacterial Cell*, 360
- H.M. Nautical Almanac Office, *Five Figure Tables of Natural Trigonometrical Functions*, 734
- Hodge (W.V.D.) [D. Pedoe and], *Methods of Algebraic Geometry*, Vol. I, 731
- Holmstrom (J. E.), *Records and Research in Engineering and Industrial Science*, 2nd ed., 354
- Hormone Research, *Recent Progress in*, Vol. I, ed. by G. Pincus, 182
- Hormones and Horticulture, G. S. Avery and E. B. Johnson, 760
- Horology, J. E. Haswell, 769
- Howells (W.), *Mankind So Far*, 368
- Hutchinson (J. B.) [R. A. Silow, S. G. Stephens and], *The Evolution of Gossypium*, 560
- Huxley (L. G. H.), *A Survey of the Principles and Practice of Wave Guides*, 353
- Huygens, Christian, and the Development of Science in the Seventeenth Century, A. E. Bell, 768
- Igneous Minerals and Rocks, E. E. Wahlstrom, 555
- Immunology, *Fundamentals of*, 2nd ed., W. C. Boyd, 567
- Induction Heating, *Fundamental Principles and Applications of*, "Heat-Treater", 346
- Inorganic Chemistry, *Modern Advances in*, E. B. Maxted, 158
- Insect Attractants and Repellents, Chemical, V. G. Dethier, 759
- Instruments, Scientific, ed. by H. J. Cooper, 747
- Jacchia (L.) [L. Campbell and], *The Story of Variable Stars*, 539
- James (E. J. F.) [F. W. Goddard and], *The Elements of Physical Chemistry* 2nd ed., 259
- James (H. M.) [N. B. Nichols, R. S. Phillips and], *Theory of Servomechanisms*, 736
- Jánosy (L.), *Cosmic Rays*, 739
- Jeffreys (H. and B.), *Methods of Mathematical Physics*, 334
- Jenkins (I.), *Controlled Atmospheres for the Heat Treatment of Metals*, 160
- Johnson (E. B.) [G. S. Avery and], *Hormones and Horticulture*, 760
- Judge (A. W.), *Aircraft Engines*, Vol. II, 2nd ed., 157; *Modern Gas Turbines*, 156
- Kamen (M. D.), *Radioactive Tracers in Biology*, 175
- Karrer (P.), *Organic Chemistry*, trans. by A. J. Mee, 3rd ed., 162
- Kinetics of Reactions in Solution, 2nd ed., E. A. Moelwyn-Hughes, 750
- Kirk (R. E.) [D. F. Othmer and], *Encyclopedia of Chemical Technology*, Vol. I: A to Anthrimides, 756
- Kirkbride (C. G.), *Chemical Engineering Fundamentals*, 755
- Kirmann (A.), *Chimie Organique*, Vols. I and II, 550
- Klystron Tubes, A. E. Harrison, 352
- Knaggs (N. S.), *Adventures in Man's First Plastic*, 552
- Laboratory Experiments in General Chemistry and Qualitative Analysis, G. W. Watt, 158
- Leguminous Forage Plants, 2nd ed., D. H. Robinson, 561
- Leverin (H. A.), *Peat Moss Deposits in Canada*, 173
- Light, Vision and Seeing, M. Luckiesh, 349
- Limestones of Canada, Pt. V: Western Canada, M. F. Goudge, 171
- Literature of Mathematics and Physics, Guide to the, N. G. Parke, 343
- Liver and its Diseases, *Lectures on the*, H. P. Himsworth, 764
- Louse, The, 2nd ed., P. A. Buxton, 370
- Lovell (B.), *Electronics and their Application in Industry and Research*, 352
- Luckiesh (M.), *Light, Vision and Seeing*, 349
- McHugh (G. P.), *A Practical Chemistry for Higher School Certificate and Intermediate Students*, 3rd ed., 159
- McIntyre (A. B.), *Curare*, 373
- McLean (R. C.) [W. R. I. Cook and], *Practical Field Ecology*, 172
- Malaria, W. K. Blackie, 374
- Mammalian Reproduction, *Patterns of*, S. A. Asdell, 372
- Mankind So Far, W. Howells, 368
- Mantell (C. L.), *The Water-Soluble Gums*, 753
- Markley (K. S.), *Fatty Acids*, 362
- Marsh (J. T.), *An Introduction to Textile Finishing*, 167
- Martin (L. H.) [R. D. Hill and], *A Manual of Vacuum Practice*, 351
- Martin (S. R. W.), *Synthetic Resin Chemistry for Students*, 754
- Mathematical Analysis, *A Text Book of*, R. L. Goodstein, 732

- Mathematical Physics, Methods of, H. and B. Jeffreys, 384
- Mauersberger (H. R.), Matthews' Textile Fibers, 5th ed., 168
- Marted (E. B.), Modern Advances in Inorganic Chemistry, 155
- Maynard (L. A.), Animal Nutrition, 2nd ed., 177
- Mechanical Vibrations, 3rd ed., J. P. Den Hartog, 156
- Mechanics, J. C. Slater and N. H. Frank, 735
- Mendelssohn (K.), What is Atomic Energy?, 544
- Metcalf (F. H.), The Bee Community, 762
- Meyer (W. K.) [H. H. Chapman and], Forest Valuation, 760
- Microanalyse, Quantitative Organische, 5th ed., F. Pregl and H. Roth, 361
- Microscope, The, J. H. Wredden, 367
- Microscope, The Intelligent Use of the, C. W. Olliver, 367
- Microwave Measurements, Technique of, ed., by C. G. Montgomery, 742
- Micro Waves and Wave Guides, H. M. Barlow, 546
- Millikan (R. A.), Electrons (+ and -), Protons, Photons, Neutrons, Mesotrons, and Cosmic Rays, 2nd ed., 544
- Mills (W. H.) [A. Findlay and], British Chemists, 148
- Milne-Thomson (L. M.), Theoretical Aerodynamics, 734
- Mineral Industry, Canadian, in 1945, Bureau of Mines, 172
- Moelwyn-Hughes (E. A.), Kinetics of Reactions in Solution, 2nd ed., 750
- Montgomery (C. G.), Technique of Microwave Measurements, 742
- Moore (T. S.) [J. C. Philip and], The Chemical Society 1841-1941, 375
- Morris (J.), The Escalator Method in Engineering and Vibration Problems, 351
- Mother Earth, G. W. Robinson, 174
- Mother Earth, T. A. Ryder, 378
- Muscular Contraction, Chemistry of, A. Szent-Györgyi, 371
- Naturalists, English, from Neekham to Ray, C. E. Raven, 375
- Nature, Harmony of, L. Richmond Wheeler, 761
- Nauth (R.), The Chemistry and Technology of Plastics, 554
- Neergaard (K. von), Dynamische Reaktions-pathologie, 568
- Newton (R. R.) [J. B. Rosser; G. L. Gross and], Mathematical Theory of Rocket Flight, 548
- Newton Tercentenary Celebrations, 1946, Royal Society, 757
- Nichols (N. B.) [H. M. James, R. S. Phillips and], Theory of Servomechanisms, 736
- Nuclear Physics in Photographs, C. F. Powell and G. P. S. Occhialini, 543
- Nucleic Acids and Nucleoproteins, Cold Spring Harbor Symposia on Quantitative Biology, Vol. XII, 762
- Occhialini (G. P. S.) [C. F. Powell and], Nuclear Physics in Photographs, 543
- Olliver (C. W.), The Intelligent Use of the Microscope, 367
- Optics, Achievements in, A. Bouwers, 347
- Organic Chemistry, P. Karrer, trans. by A. J. Mee, 3rd ed., 162
- Organic Chemistry, A Text-Book of Practical, A. I. Vogel, 752
- Othmer (D. F.) [R. E. Kirk and], Encyclopedia of Chemical Technology, Vol. I: A to Anthrimides, 756
- Owen (L. N.) [J. L. Simonsen and], The Terpenes, Vol. I, 2nd ed., 551
- Paleontology, An Introduction to, 2nd ed., A. Morley Davies, 364
- Paleontology, Outlines of, 3rd ed., H. H. Swinerton, 169
- Paleobotany, An Introduction to, C. A. Arnold, 558
- Paramagnetic Relaxation, C. J. Gorter, 737
- Parke (N. G.), Guide to the Literature of Mathematics and Physics, 343
- Pauli (W.), Exclusion Principle and Quantum Mechanics, 739
- Pearce (W. E.), Introduction to Wireless, 746
- Peat Moss Deposits in Canada, H. A. Leverin, 173
- Pedoe (D.) [W. V. D. Hodge and], Methods of Algebraic Geometry, Vol. I, 731
- Pelseneer (J.), L'Évolution de la Notion de Phénomène physique des Primitifs à Bohr et Louis de Broglie, 766
- Permo-Triassic Formations, R. L. Sherlock, 556
- Perry (E. J.), The Artificial Insemination of Farm Animals, 176
- Petrographic Micro-Technique, A. V. Weatherhead, 366
- Philip (J. C.) [T. S. Moore and], The Chemical Society 1841-1941, 375
- Phillips (R. S.) [H. M. James, N. B. Nichols and], Theory of Servomechanisms, 736
by Infrared, 2nd ed., W.
- Physical Chemistry, E. D. Eastman and G. K. Rollefson, 749
- Physical Chemistry, The Elements of, 2nd ed., F. W. Goddard and E. J. F. James, 359

- Physics, A University Text-Book of, Vol. I: Properties of Matter, 14th ed., J. H. Poynting, Sir J. J. Thomson and G. W. Todd, 344
- Physics, Intermediate, 3rd ed., C. J. Smith, 736
- Physiology, Annual Review of, Vol. IX, ed. V. E. Hall, 183
- Physiology of Man in the Desert, E. F. Adolph and Associates, 565
- Pincus (G.), Recent Progress in Hormone Research, Vol. I, 182
- Pippard (A. J. S.), The Experimental Study of Structures, 154
- Plant Diseases, Nature and Prevention of, 2nd ed., K. S. Chestor, 758
- Plant Ecology and the School, A. G. Tansley and E. Price Evans, 172
- Plant Ecology, Introduction to, A. G. Tansley, 172
- Plants, Ancient, and the World they Lived in, H. N. Andrews, 558
- Plastic, Adventures in Man's First, N. S. Knaggs, 552
- Plastics, The Chemistry and Technology of, R. Nauth, 554
- Plastics, The Chemistry of Commercial, R. L. Wakeman, 553
- Powell (C. F.) [G. P. S. Occhialini and], Nuclear Physics in Photographs, 543
- Poynting (J. H.) [Sir J. J. Thomson, G. W. Todd and], A University Text-Book of Physics, Vol. I: Properties of Matter, 14th ed., 344
- Pregl (F.) [H. Roth and], Quantitative Organische Microanalyse, 5th ed., 361
- Process. Engineering Calculations, Manual for, L. Clarke, 356
- Protein Chemistry, Advances in, Vol. III, M. L. Anson and J. T. Edsall, 164
- Psychology of Ego-Involvements, M. Sherif and H. Cantril, 571
- Quantitative Inorganic Analysis, An Introduction to, R. Belcher and M. B. Thompson, 159
- Radar Beacons, ed. by A. Roberts, 743
- Radar System Engineering, ed. by L. N. Ridenour, 547
- Radio Aids to Navigation, R. A. Smith, 744
- Radio Engineering, Vol. I, E. K. Sandeman, 745
- Radioactive Tracers in Biology, M. D. Kamen, 175
- Rare Earth Elements and their Compounds, D. M. Yost, H. Russell, Jr., and C. S. Garner, 358
- Raven (C. E.), English Naturalists from Neckham to Ray, 375
- Raven (C. P.) [M. W. Woerdeman and], Experimental Embryology in the Netherlands 1940-45, 369
- Rayons X, Physique appliquée des Installations à, G. Destrian, 153
- Reactions of Organic Compounds, 2nd ed, W. J. Hickinbottom, 751
- Relton (F. E.), Applied Bessel Functions, 342
- Requins, La Vie des, P. Budker, 368
- Resin Chemistry for Students, Synthetic, S. B. W. Martin, 754
- Rettinger (M.), Applied Architectural Acoustics, 154
- Richardson (E. G.), Sound, 4th ed., 540
- Ridenour (L. N.), Radar System Engineering, 547
- Ritt (J. F.), Theory of Functions, rev. ed., 732
- Roberts (A.), Radar Beacons, 743
- Robinson (D. H.), Leguminous Forage Plants, 2nd ed., 561
- Robinson (G. W.), Mother Earth, 174
- Rocket Flight, Mathematical Theory of, J. B. Rosser, R. R. Newton and G. L. Gross, 548
- Rollefson (G. K.) [E. D. Eastman and], Physical Chemistry, 749
- Rosser (J. B.) [R. R. Newton, G. L. Gross and], Mathematical Theory of Rocket Flight, 548
- Roth (H.) [F. Pregl and], Quantitative Organische Microanalyse, 5th ed., 361
- Royal Society Newton Tercentenary Celebrations, 767
- Russell (H.) [D. M. Yost, C. S. Garner and], The Rare Earth Elements and their Compounds, 358
- Ryder (T. A.), Mother Earth, 378
- Safety Rules for Use in Chemical Works, Pt. I: Model Rules, 3rd ed., Association of British Chemical Manufacturers, 757
- Sandeman (E. K.), Radio Engineering, Vol. I, 745
- Science, An Outline of the Development of, M. Davies, 570
- Science, On Understanding, J. B. Conant, 569
- Science, The Limits of, L. Chwistek, trans. by H. C. Brodie and A. P. Coleman, 705
- Science in Transition, A. W. Haslett, 377
- Science News V, ed. by J. Enogot, 571
- Sernat (H.), Introduction to Atomic Physics, 740
- Servomechanisms, Theory of, ed. by H. M. James, N. B. Nichols and R. S. Phillips, 736
- Shapley (H.), Galaxies, 539
- Sherif (M.) [H. Cantril and], The Psychology of Ego-Involvements, 571
- Sherlock (R. L.), The Permian-Triassic Formations, 556
- Sherman (H. C.), Foods, Their Values and Management, 178

- Silow (R. A.) [J. B. Hutchinson, S. G. Stephens and], *The Evolution of Gossypium*, 560
- Simon (L. E.), *German Research in World War II*, 338
- Simonsen (J. L.) [L. N. Owen and], *The Terpenes*, Vol. I, 2nd ed., 551
- Slater (J. C.) [N. H. Frank and], *Electromagnetism*, 542; *Mechanics*, 735
- Smart (R. C.), *The Technology of Industrial Fire and Explosion Hazards*, Vols. I and II, 168
- Smith (C. J.), *Intermediate Physics*, 3rd ed., 736
- Smith (R. A.), *Radio Aids to Navigation*, 744
- Snook (J. L.), *New Developments in Ferromagnetic Materials*, 738
- Somers (G. F.) [J. B. Sumner and], *Chemistry and Methods of Enzymes*, 2nd ed., 179
- Sound, 4th ed., E. G. Richardson, 540
- Soyer (R.) [R. Furon and], *Catalogue des Fossiles Tertiaires du Bassin de Paris*, 558
- Sporn (P.) [E. R. Ambrose, T. Baumeister and], *Heat Pumps*, 746
- Stars, Variable, *The Story of*, L. Campbell and L. Jacchia, 539
- Statistical Methods in Research and Production, ed. by O. L. Davies, 185
- Stephens (S. G.) [J. B. Hutchinson, R. A. Silow and], *The Evolution of Gossypium*, 560
- Strausbaugh (P. D.) [B. R. Weiner and], *General Biology*, 2nd ed., 561
- Structures, *The Experimental Study of*, A. J. S. Pippard, 154
- Sub-station Practice, T. H. Carr, 355
- Sumner (J. B.) [G. F. Somers and], *Chemistry and Methods of Enzymes*, 2nd ed., 179
- Swinnerton (H. H.), *Outlines of Palaeontology*, 3rd ed., 169
- Szent-Györgyi (A.), *Chemistry of Muscular Contraction*, 371
- Tansley (A. G.), *Introduction to Plant Ecology*, 172; [E. Price Evans and], *Plant Ecology and the School*, 172
- Taylor (F. Sherwood), *A Century of British Chemistry*, 375
- Temperature, *Methods of Measuring*, 3rd ed., E. Griffiths, 346
- Terpenes, *The*, Vol. I, 2nd ed., J. L. Simonsen and L. N. Owen, 551
- Textile Fibers (Matthews) 5th ed., ed. by H. R. Mauersberger, 168
- Textile Finishing, *An Introduction to*, J. T. Marsh, 167
- Thermodynamics, Applied, A. C. Walshaw, 547
- Thimann (K. V.) [R. S. Harris and], *Vitamins and Hormones*, Vol. IV, 181
- Thompson (M. B.) [R. Belcher and], *An Introduction to Quantitative Inorganic Analysis*, 159
- Thomson (Sir G.), *The Atom*, 3rd ed., 535
- Thomson (Sir J. J.) [J. H. Poynting, G. W. Todd and], *A University Text-Book of Physics*, Vol. I: Properties of Matter, 14th ed., 344
- Time and Thermodynamics, A. R. Ubbelohde, 345
- Todd (G. W.) [J. H. Poynting, Sir J. J. Thomson and], *A University Text-Book of Physics*, Vol. I: Properties of Matter, 14th ed., 344
- Tolansky (S.), *Introduction to Atomic Physics*, 2nd ed., 153
- Townsend (Sir J.), *Electrons in Gases*, 738
- Trigonometrical Functions, Five Figure Tables of Natural, H.M. Nautical Almanac Office, 734
- Ubbelohde (A. R.), *Time and Thermodynamics*, 345
- Umbgrove (J. H. F.), *The Pulse of the Earth*, 2nd ed., 170
- Unit Processes in Organic Synthesis, 3rd ed., ed. by P. H. Groggins, 162
- Vacuum Practice, A. Manual of, L. H. Martin and R. D. Hill, 351
- van Nostrand's Scientific Encyclopedia, 2nd ed., 186
- Vitamins, *The Biological Standardisation of the*, 2nd ed., K. H. Coward, 566
- Vitamins and Hormones, Vol. IV, ed. by R. S. Harris and K. V. Thimann, 181
- Vogel (A. I.), *A Text-Book of Practical Organic Chemistry*, 752
- Wahlstrom (E. E.), *Igneous Minerals and Rocks*, 555
- Wakeman (R. L.), *The Chemistry of Commercial Plastics*, 553
- Walshaw (A. C.), *Applied Thermodynamics*, 547
- Watt (G. W.), *Laboratory Experiments in General Chemistry and Qualitative Analysis*, 158
- Wave Guides, *A Survey of the Principles and Practice of*, L. G. H. Huxley, 353
- Weatherhead (A. V.), *Petrographic Micro-Technique*, 366
- Weaver (E. C.) [L. S. Foster and], *Chemistry for our Times*, 357
- Weimer (B. R.) [P. D. Strausbaugh and], *General Biology*, 2nd ed., 561
- Wheeler (L. Richmond), *Harmony of Nature*, 761
- Wheeler (W. F.), *Essentials of Biology*, 174

- Widder (D. V.), *Advanced Calculus*, 342
 Williams (H. E.), *Cyanogen Compounds*,
 2nd ed., 748
 Williams (R. T.), *Detoxication Mech-
 anisms*, 567
 Williams (T. I.), *Drugs from Plants*,
 373
 Wilson (W.), *The Cathode Ray Oscillo-
 graph in Industry*, 3rd ed., 742
 Wireless, *Introduction to*, W. E. Pearce,
 746
 Woerdeman (M. W.) [C. P. Raven and],
 *Experimental Embryology in the
 Netherlands 1940-1945*, 369
 Wolf (F. A. and F. T.), *The Fungi*, Vols. I
 and II, 726
 Wredden (J. H.), *The Microscope*, 367
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